

ROYAL COMMISSION ON VIVISECTION.

APPENDIX

TO

THIRD REPORT

OF

THE COMMISSIONERS.

MINUTES OF EVIDENCE,

April to July, 1907.

Presented to both Houses of Parliament by Command of His Majesty.



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1907.

ROYAL COMMISSION ON VIVISECTION.

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LIST OF WITNESSES.

In the order in which they appeared before the Royal Commission.

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1907. 16th Day - April 24th	Sir T. LAUDER BRUNTON, M.D., F.R.C.P., F.R.S.	Consulting Physician to St. Bartholomew's Hospital.	A Committee of Medical and Scientific Societies.	6,777	1
17th Day - May 1st.	Miss L. LIND-AF-HAGEBY	Author - - - - -	- - - - -	7,186	18
18th Day - May 8th.	Mr. HENRY MORRIS, F.R.C.S.	President of the Royal College of Surgeons of England.	Royal College of Surgeons of England.	7,651	41
19th Day. May 15th.	Major L. ROGERS, M.D., F.R.C.P., I.M.S.	Professor of Pathology at the Medical College, Calcutta.	- - - - -	8,029	59
	Rev. J. PAGE HOPPS -	Unitarian Minister.	The Social Purity Alliance.	8,312	69
20th Day - May 29th.	Rev. L. S. LEWIS, M.A.	Clerk in Holy Orders - - -	Church Anti-Vivisection League.	8,629	79
	Miss A. L. WOODWARD	Hon. Assistant Secretary, Church Anti-Vivisection League and World League of Opponents of Vivisection.	" " "	8,786	87
	Mr. G. H. BURFORD, M.D.	35, Queen Anne Street, Cavendish Square.	World League of Opponents of Vivisection.	8,899	92
21st Day - June 5th.	Miss L. Lind-af-Hageby (re-called).	- - - - -	- - - - -	9,105	100
22nd Day - June 12th.	Mr. (now Sir) H. R. SWANZY, M.D.	President of the Royal College of Surgeons in Ireland.	Royal College of Surgeons in Ireland.	9,752	121
	Mr. E. A. SCHÄFER, LL.D., Sc.D., F.R.S.	Professor of Physiology at Edinburgh University.	Royal Society of Edinburgh and Edinburgh University.	9,980	130
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24th Day - June 26th.	The Hon. Stephen Coleridge (re-called).	- - - - -	- - - - -	10,551	164
25th Day - July 3rd.	The Hon. Stephen Coleridge (re-called).	- - - - -	- - - - -	10,991A	185

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1907. 26th Day - July 10th	Mr. C. J. MARTIN, M.B., D.Sc., F.R.S.	Director of the Lister Institute of Preventive Medicine.	A Committee of Delegates of Medical and Scientific Societies.	11,630	208
27th Day - July 17th.	Mr. C. J. Martin (re-called).	- - - - -	- - - - -	12,141	236
	Mr. D. W. BUXTON, M.D., B.S., M.R.C.P.	Consulting Anæsthetist to the National Hospital for Paralysis and Epilepsy; Anæsthetist and Lecturer on Anæsthetics in University College Hospital.	Society of Anæsthetists.	12,383	243
28th Day - July 24th.	The Right Hon. Sir J. FLETCHER MOULTON, F.R.S.	Lord Justice of Appeal.	- - - - -	12,691	251

MINUTES OF EVIDENCE

TAKEN BEFORE THE

ROYAL COMMISSION ON VIVISECTION.

SIXTEENTH DAY.

Wednesday 24th April 1907.

PRESENT :

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.

Sir W. S. CHURCH, Bart., K.C.B., M.D.

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.

Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.

Mr. W. H. GASKELL, M.D., F.R.S.

Mr. J. TOMKINSON, M.P.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Sir T. LAUDER BRUNTON, M.D., F.R.C.P., F.R.S., called in; and Examined.

6777. (*Chairman*.) You are a Doctor of Medicine, and a Fellow of the Royal College of Physicians, a Fellow of the Royal Society, and you have other degrees, honorary and otherwise?—That is so.

6778. I believe you are consulting physician to St. Bartholomew's Hospital?—I am.

6779. And have you taken, from an early period in your studies in medicine, a great interest in this question of experiments on animals?—I have.

6780. And you have had practical experience yourself?—I have.

6781. For a good many years past?—For a good many years.

6782. Did you confine yourself to any particular class of object originally? We have had some witnesses who have confined themselves principally to the operation of drugs, others to particular organs of the body, and so forth. Were your experiments directed to any particular subject originally?—The particular subject at which I was working was to ascertain the action of drugs, especially with a view to utilising them in the treatment of disease.

6783. I think it would be convenient, as some members of the Commission have to leave at a rather early hour, and may not be able to get back soon, if I were to ask you now with reference to an experiment of yours, taking it now a little out of order, that was referred to by Mr. Graham. I think you have had sent to you a note of what he said?—Yes, I have got it before me.*

6784. He quoted from memory, and without reading from the book, some report that he had seen of these experiments, and he gave the general effect of it as being that cats were baked to death in order to ascertain, as I understood, what the effect of a high temperature was on them, or what temperature they could live in. I do not quite understand, from what he said, what the precise object was. Perhaps you will explain the experiment in your own way?—Yes;

and perhaps I might be allowed to go back a little further even than that experiment, because, by doing so, I should give a sort of continuous view of what I was doing.

6785. If you please?—In 1870, Professor Stricker found that rabbits, when completely narcotised with chloral, lost their power of maintaining their temperature and died. If those same rabbits, instead of being laid out upon a table, where they could cool readily, were simply enveloped in cotton wool, they lay senseless for many hours and ultimately recovered.

6786. You mean enveloped in cotton wool during the experiment?—Practically what he wanted to do was to examine the tissues microscopically, their ears, and so on, and he found it was convenient to render them completely senseless and immobile by a large dose of chloral, that rendered them completely senseless so that they could neither feel nor move. They remained in this condition for many hours, but unless they were kept warm they ultimately died. If, however, he simply wrapped them up in a sheet of cotton wool, at the end of several hours they began gradually to recover. He directed my attention to this, and I found that if, instead of merely wrapping them up in cotton wool, I kept them warm artificially with an ordinary hot water bag, or by a tin hot water can, the animals would bear a bigger dose of chloral, and still recover, than if they were simply wrapped up in cotton wool. And I am glad to say these experiments of mine have borne practical fruit; because, not very long after I published them, the late Dr. Young, of Florence, had a case of an overdose of chloral, when the patient, an old lady, would certainly have died but for the fact that he remembered the results of my experiments, about which I had been conversing with him. He wrapped her up in cotton wool and put hot water bags round her, and she came round. That was the first thing that drew my attention to the effect of temperature upon the action of drugs. The next thing I wanted to find out was why certain drugs, given in cases of disease accom-

Sir T. L.
Brunton,
M.D.,
F.R.C.P.,
F.R.S.

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* See Questions 5844-5849, 6079-6081, 6131-6140.

Dr. Graham, in returning his own corrected evidence on 15th March, had also appended the following note:—"Though there is nothing to correct in the above account, it may save time and controversy if I add that since I gave evidence I have looked up the detailed account of the experiments which preceded the above and which were recorded in the Reports of St. Bartholomew's Hospital for 1871, Vol. VII., p. 216, by Sir (then Dr.) Lauder Brunton alone. He gives details of his method. The animals were narcotised only with chloral—once with laudanum—and so were kept still. The heating apparatus was a tin with double bottom and sides into which hot water was poured, and a hot water bag was put on the belly. The heart was pierced by a needle and the thermometer was in the rectum. These methods may or may not have been followed in the later series I quoted.

J. W. G.

Sir T. L.
Branton,
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F.R.S.

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panied by high fever, do not act in the usual way. It was pointed out by Professor Thomas that pneumonia, when treated by digitalis, does not yield to digitalis in the way you would expect from the action of the drug upon a healthy man. The digitalis seems, in the case of a person having pneumonia, not to act upon the heart in the same way that it would upon an ordinary person. It occurred to me that this might be due to the effect of temperature upon the heart, and so I proceeded to make a number of experiments upon animals with high temperatures. If you keep a healthy animal or a healthy man in a warm place, as a rule their temperature does not rise very high; it rises a little, but not very high, as everyone knows who has gone into a Turkish bath; but if you narcotise an animal completely, and then keep it warm, it has lost its power of regulating the temperature, and the temperature goes on rising and rising until the animal dies, although the animal remains perfectly unconscious for the whole time. I therefore narcotised animals (and especially I used rabbits in that series of experiments) by a large dose either of chloral or opium. I then placed them in a bath, of which I have had a copy made. The object of the experiment was to imitate the conditions of a patient suffering from high fever and lying in bed. To have had any pain inflicted would have tended to vitiate the experiment. The description was fully given in the paper that I wrote in the St. Bartholomew's Reports, Volume VII, for 1871, at page 218.

6787. Will you please read the description of the bath?—"These experiments were made by narcotising the animal with opium or chloral, and laying it in a tin vessel about 20 inches long, by six inches broad and three inches deep in the inside, and well padded with cotton wool. The vessel was double, and by pouring hot water into it the temperature of the rabbit was gradually raised. The belly of the animal was also covered with cotton wool, and sometimes with an india rubber bag containing hot water. To make respiration easier a cannula was introduced into the trachea, and in some experiments the inspired air was passed over warm water so as to warm it and saturate it with moisture, and lessen the loss of heat from the lungs."

6788. I thought it necessary to ask for that description, because Mr. Graham said that the animal, a cat, I think he said, was put into a stove and baked to death?—Yes, and Miss Frances Power Cobbe also described these experiments as "roasting to death."

6789. (Sir William Church.) Mr. Graham's words are: "They were put into a kind of stove and were heated until they died"?—Yes. Then I may pass over the means, perhaps, of recording the beats of the heart; and then I go on to say: "As the animals were completely narcotised and remained perfectly motionless, the thermometer remained undisplaced in the rectum from the beginning to the end of the experiments." That shows how complete the narcosis was, that the thermometer introduced into the rectum remained perfectly undisturbed the whole time. The animal did not make the slightest movement.

6790. (Chairman.) Are we to understand that the object of this experiment was to test to what temperature you might safely raise the heat of an animal while it was under narcotics?—No; the object of the experiment was to find out what the condition of the heart was in what is known as hyperpyrexia. In many cases of disease the temperature of the patient rises high above the normal, and then these patients are in a state of extreme danger, and unless you can reduce the temperature in some way we know that these patients will die. The object of these experiments was to find out what the condition of the heart and circulation was in pyrexia, and especially in hyperpyrexia, so that one might be able to use appropriate remedies to reduce the temperature and slow the circulation.

6791. Was the animal operated on after it was in this box or while it was in the box?—There was almost no operation done on it. The only operation that was done was that it was first of all completely narcotised by a subcutaneous injection of chloral or opium; the animal became completely senseless, and then a cannula was introduced into the trachea at the same time to allow us to warm the air entering the lungs. The only other operation done upon it was to put an ordinary sewing needle between the ribs and attach it by means of a thread to a lever so as to record the movements of the heart, in order to see how quickly

the heart was beating in accordance with every rise of temperature in the animal. The object was to ascertain the action of the high temperature of the hyperpyrexia of the high fever upon the organs of the body, and especially the heart, with a view to finding out how we were to prevent danger of death to our patients when suffering from high fever.

6792. And did you attain the object?—To a certain extent, yes.

6793. Do you consider that the experiment was a useful one?—Certainly.

6794. Not only justifiable to try it, but it was useful in its results?—Assuredly, I think.

6795. (Sir Mackenzie Chalmers.) The experiment was absolutely painless throughout?—Absolutely.

6796. (Chairman.) I think, in fact, if I recollect aright, Mr. Graham said in his evidence that obviously it was impossible to perform it under anæsthetics?—Yes, he said so. This is not the series of experiments referred to by Mr. Graham, but this is the apparatus used in the series, to which he refers in "The Practitioner."

6797. Before you finish with that, was the rabbit killed before it came out of the anæsthesia, or was it allowed to recover?—They were all killed, either killed or died from the effects of the chloral.

6798. (Mr. Tomkinson.) I thought the object was to see whether they would not recover by the preservation of their heat?—No, excuse me; these are two entirely different series. The first series of experiments I referred to was upon the effect of heat in preserving life; this second series was upon the effect of heat in causing alteration in the heart.

6799. (Sir William Church.) As a matter of fact, these animals were narcotised by a poisonous dose of chloral or morphia?—Always.

6800. So that they naturally died?—Yes, these animals would have died anyhow whatever we had done to them.

6801. We have been told here repeatedly that morphia is not an anæsthetic; that depends, of course, upon the quantity. We have been told also that chloral is not an anæsthetic; that also depends upon the quantity. These animals receive poisonous doses in order to completely narcotise them?—Yes; and as to the statements that chloral and opium or morphia are not narcotics, and do not remove pain, there is no other word for it, it is simply a lie; you may as well say that chloroform does not remove pain. If you give any animal a sufficiently large dose of chloral or opium, you so completely abolish sensibility that there is nothing you can do that will awaken its sensibility; you might burn it alive, you might throw it on the coals, and it would remain unmoved. I have never tried it, but I know you can conduct any experiment which would in the ordinary nature of things be painful, but there is no pain attached to it. The animal is as senseless as a piece of board.

6802. (Colonel Lockwood.) But it all depends on that statement "if a sufficiently large dose is given"?—Quite.

6803. (Chairman.) You are speaking of chloral and morphia?—I am speaking of chloral and morphia. I may say that nearly all my experiments were done after I had been in Professor Ludwig's laboratory in Leipsic, in which the invariable rule was that before any experiment was done the animal should be thoroughly narcotised. Dogs were frequently used, and the way they narcotised them was to inject into the vein that runs on the hough joint, and which is there prominent, at least a drachm of tincture of opium. The usual effect of this was that in less than half a minute the animal was completely senseless, and never came round.

6804. You were going on to the actual experiment which Mr. Graham was referring to in his evidence. What you have told us was introductory, leading up to the other?—Yes; he refers to the "Practitioner," Volume 33, page 273; and the reason why I have given this long explanation of this series of experiments is not only that Miss Frances Power Cobbe described this series as "roasting rabbits alive," but that the second series, the series with digitalis referred to by Mr. Graham, were a continuation more or less of this first series, and were made in exactly the same way. At page 275 of the "Practitioner," Volume 25, the papers quoted by Mr. Graham—Dr. Cash and I say, after referring to the first series of experiments, which I have

just been describing, "In experiments of the same kind which we have made upon cats"; and the reason why no full description of the method of the experiment was given in the "Practitioner" was that the whole method had been so completely described before that anyone who wished to know how the experiments were conducted had only to refer to the former paper.

6805. When you say, "In experiments of the same kind," what was the preceding sentence; because I suppose it referred to the previous experiments?—Shall I read the whole?

6806. What I want to get at is this: I want to see whether a person reading those words, "of the same kind," would have any means on that page of tracing back and seeing what the previous experiments were; because it is assumed that these animals were slowly baked to death without any anaesthetics, as Mr. Graham says?—Then, perhaps, I had better read the sentence before: "We have therefore attempted, as a first step in our investigation, to ascertain the effect of rise of temperature alone on the pulse in the cat, which animal we employed in the present research. In a former series of experiments made by one of us—" Then comes, in a footnote, a reference to this series, to which I have already referred, "on the effect of heat upon the pulse in the rabbit, it was found that the pulse-rate increased with considerable regularity as the temperature of the body rose, as is shown by the following experiments, in one of which the vagi were uncut and in others were cut. At the commencement of the experiment there were slight irregularities before, which have not been copied here. After the temperature had risen above a point which is marked by thick type in the table, the pulse rate began to fall." Then follows a table of the temperatures and the corresponding rates of the pulse; and immediately after the table comes the expression I have just used: "In experiments of the same kind which we have made upon cats."

6807. So that any investigator reading that and seeking to ascertain the whole truth about it, would have the means of seeing precisely what the nature of the experiment was?—Precisely.

6808. And the conditions under which it was performed?—Precisely—the reference is fully given; and the reason why it was thought unnecessary to give it again was that anyone who wished to ascertain precisely how the experiments were done could do so by referring to the original paper—the first paper. But this paper on digitalis was not written for physiologists or pharmacologists, but for general practitioners, and had a practical object in view; it was to tell them what alterations they might expect to find in the action of digitalis if they gave it to a person suffering from high fever; and so after giving, in such a way as the general practitioner would understand, a *résumé* of our results, we then went on to the practical conclusions, which are practically that "The administration of digitalis, or of drugs which act like it, to patients in a febrile condition is therefore likely to have much less effect upon the pulse than at the normal temperature; and if the temperature be very high they may have no effect at all whilst this persists." And then comes rather an important point: "When the temperature began to fall the pulse naturally became slower, and this slowness is increased if digitalis has been given at the height of the fever. It is therefore evident that digitalis and its congeners, if they are given at all when the temperature is high, should be given with great care; for, otherwise, the medical man may be induced, by the apparent inaction of the remedy, to push its administration too far during the fever, with the consequence of producing too great depression of the pulse during defervescence." Now this is a very great practical danger, because, in a case of high fever, you are tempted to use every means in your power, and to push every means to reduce the temperature, and to slow the pulse, which is going tremendously quickly; and you are apt to forget that in a fever like pneumonia, which ends after a definite period, the temperature will fall and there will come a condition of defervescence, and very likely of collapse, and that if you are careless in pushing your digitalis too far during the fever you may help to produce a greater condition of collapse after the fever is over and kill the very patient you want to save.

6809. Was this experiment that you made with a cat conducive to, or was it essential to, the discovery of which you have just told us?—Yes, surely. I do not see how you could have done it without.

6810. You say it was necessary; it was the way in which it was discovered?—It was the way in which it was discovered, and I do not see how you could have discovered it in any other way. The only other way in which you could do it would be to experiment upon a number of patients. The difficulty in experimenting upon patients is that every medical man who is called in to a patient feels himself bound to do his best for that individual patient. He is not called in to advance the science of medicine by what he tries upon his patient; he has got to consider that individual patient and do the best for him. Whereas, if you are working upon a lot of animals, you do what is best to attain knowledge without considering whether the animal is going to reap any particular benefit from what you give it or not.

6811. I think every member of the Commission would agree with you there, that when a doctor is called in his first duty is to his patient and not to science?—Surely.

6812. That is your description as given from your book and your comment of actual experiment which is referred to here?—Yes, and here I have had a model made.

6813. I should like to have this quite clear, without referring back again, that the heating was carried on in the way which you have described in that box with cotton wool and under a fatal dose of chloral?—Yes, or of morphin or of opium.

6814. Ultimately fatal?—Yes.

6815. Then I gather that it is not true to say what Mr. Graham says in answer to this question: "You do not wish us to understand that these poor creatures were slowly baked to death, because that could not be under anaesthesia, and it would be most awful torture"? To which he replied: "They were slowly done to death by heat." That, taken in conjunction with the question, is not very accurate?—It is not so.

6816. (Colonel Lockwood.) Do you say chloral and opium?—No, chloral or opium. Once or twice I have used the two together, but as a rule it is unnecessary to use the two; but once specially I remember using the two together.

6817. (Sir William Church.) But as a matter of fact in your second series of experiments on these cats the majority of them did not recover, I think?—No, those all died too; they all got this fatal dose. But then there is yet another series which, perhaps, I had best refer to, they have not been referred to in the evidence, but still Mr. Graham has evidently got a notion of a stove of some kind.

6818. I think, if I might say so, Mr. Graham mixed up the sets; because he led me to think that some of the animals recovered?—He has probably mixed up yet a third set, and these were a series of experiments upon the action of aconite as an antipyretic. In many cases of fever small doses of aconite seem to reduce the fever very much; and so Dr. Cash and I tried to see how far the aconite would do this in animals, and more especially we used pigeons. In pigeons the temperature is higher than in ordinary animals; the normal temperature of a pigeon being about 104, whereas in man it is only 98.2 Fahrenheit. With these pigeons we did use a kind of a box, and as that will very likely be referred to later on—I mean that they may mix it up again—I have thought well to take the box question up as well. This box was simply a metal box with a glass top, and surrounding the metal box was a second case in which you could put either hot or cold water, so as to raise the temperature of the box to such a degree as we liked. We padded the inside with brown paper, so as to prevent any contact of the animal with the metal itself, because we did not want any local chill or any local burning to vitiate the experiment. All we wanted to do was to find out the condition of fever such as would occur in a patient in bed. We raised the temperature of the animals by raising the temperature of the box, in some cases to 106 Fahrenheit; but this can hardly be described as baking alive, because I have found by inquiry at the Jermyn Street Bath that, whereas our highest temperature was 106 Fahrenheit the lowest temperature of the Jermyn Street Bath is 120 Fahrenheit, their highest temperature running up to 230.

6819. (Chairman.) Do you mean the temperature of the atmosphere at which the animal was was 106?—Yes. May I bring the figure of the box round to show it you? (Exhibiting the figure of the box to the Commissioners.) We raised the temperature of the box in some cases up to 106°.

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6820. (Mr. Tomkinson.) That was the temperature of the air which the animal breathed in the box?—Yes.

6821. 106° only?—Yes, whereas the lowest temperature of the Jermyn Street Baths is 120°.

6822. The first room is 120° or 125° and runs up to 220°?—Yes.

6823. I have sat in 200° myself?—So that although a box was used here which might be described as a stove, an animal was put into it, the pigeons sometimes at 107° did show symptoms of discomfort and feeling too hot; but that could hardly be described as roasting them alive.

6824. (Chairman.) Mr. Graham refers to the temperature of the cat, he says, as being raised to 111° in one case and 113° in another. Was it in that experiment?—No; that was in the series of experiments of which I spoke before.

6825. The rabbit?—No; that was the one with cats on digitalis, but carried on with the same instrument as the experiment on rabbits.

6826. Carried on in the tin bath?—Yes; the tin bath I have already shown, and of which I have read the description (6787). If you narcotise a cat completely with either chloral or with opium and leave it just with the cotton wool round it, and it is warm, its temperature gradually rises, because the narcosis disturbs the normal power of regulating the temperature, so that the temperature of the cat will rise to 111, 112, and 113, and then that is hyperpyrexia, and they die, just as those patients who have acute rheumatism often die with high temperature in a hospital ward.

6827. In the case of the cat, the intention was that it should not recover?—That it should not recover.

6828. And you were testing it under this complete anaesthesia to see at what point that hyperpyrexia would occur?—Yes, that was, however, rather incidental. What we wanted to find out was the condition of the heart throughout.

6829. I should rather say that you were testing it with the full understanding that it would occur at one point?—Yes.

6830. (Colonel Lockwood.) I only wish to ask you a few questions as to the present law. I note that you say in your *précis* that the law, instead of being made more stringent, ought to be relaxed. Do you look upon the present laws on the subject of animal vivisection as a stop to science?—To a certain extent I do.

6831. And in consequence, do you think that our knowledge of science is behind that of foreign nations where those laws do not exist?—It would be behind if the foreign nations had the same laws as we have; but we keep pace with them by learning what they are doing. We depend for a great deal of our information upon what we get from abroad.

6832. Then our superior intellect enables us to outstrip them?—In some cases; certainly not in all.

6833. Therefore I presume you would be very much opposed to any further restrictions upon experiments upon living animals?—Surely. Perhaps I might be allowed to give one illustration of how the Act has interfered with work which might have been beneficial. About 1871, with Sir Joseph Fayrer, in making experiments upon snake venom, we found out that there were certain substances which would destroy snake venom absolutely; but we had not succeeded in obtaining immunity or saving life after a big dose had been injected. We could not administer the antidote in such a way as to prevent the action of the poison when once introduced. Then came the anti-vivisection laws, and we could not continue our experiments at all at home. The consequence of that was that they lapsed for a number of years, and they were only taken up again about two years ago by Major Rogers. He was able to take them up again because Professor Waller had invented a method of giving chloroform continuously for thirty hours or more, so that he was able to give the poison and then keep the animal for many hours under the anæsthetic, and then he could try the effect of the antidote. I had invented a small lancet which I thought might be useful, but it had not practically been tried until Major Rogers got this chance of doing it, when he found that it answered quite well. He has since been using it in India, and has, I think, now got either seventeen or twenty cases of snake-bite in which the persons would almost assuredly have died and who

have been saved by the use of the instrument. I have one here; it is made in two forms (*exhibiting the same*). It consists of a small lancet, in the handle of which is some permanganate of potash. In using that, the punctured wound made by the snake's fang is simply converted into a slit, and permanganate of potash is rubbed in after being wetted with saliva.

6834. You claim that your little lancet has been discovered by the assistance of experiments on living animals?—It was made in consequence of a number of experiments that I made with Sir Joseph Fayrer.

6835. (Chairman.) Colonel Lockwood's question was whether the shape of the lancet was discovered by experiment. I do not know whether you mean that?—The shape of the lancet is of no importance.

6836. (Colonel Lockwood.) I did not mean that.

6837. (Chairman.) The cure is by permanganate of potash?—Yes.

6838. (Colonel Lockwood.) All obtained by experiments on living animals?—Yes, experiments which were made quite thirty years ago.

6839. Have you ever used curare only, in experiments on living animals?—Yes.

6840. Latterly?—No, I cannot very well say whether I have used it only.

6841. When performing an operation under licence from the Home Office?—No, any experiments I made with curare would be previously to the existing law.

6842. (Chairman.) Previously to 1876?—Yes.

6843. (Colonel Lockwood.) And never since?—I cannot say that I even did it before. It is just possible that I may have done it, but I cannot recollect it.

6844. Have you witnessed a very great number of experiments, and conducted yourself very many?—Yes.

6845. Do you, or do you not think that further inspection of laboratories where these experiments are carried on is advisable?—I do not think it would be any good at all; I do not think it is advisable.

6846. You believe that the experiments in these laboratories are conducted absolutely painlessly?—Yes.

6847. And without unnecessary suffering?—I do.

6848. (Sir William Collins.) In the interesting *précis* which you have sent us I see you state that in your opinion the majority of those who desire the abolition of experiments on animals "are actuated by praiseworthy motives"?—Yes.

6849. But you think that their opposition is founded on ignorance?—In many cases it is, I am sure.

6850. You trust that the evidence taken by this Commission may help to dispel that ignorance?—Yes.

6851. I see you call attention to the importance of bearing in mind a liberal interpretation of the word "experiment" in dealing with the whole question of the effect of experiments upon living animals for scientific purposes?—Yes.

6852. You speak of experimentation as the condition under which all living beings exist?—Yes.

6853. And you say that medical men also know only too well that every effort they make to alleviate pain or cure disease is an experiment made on their patients?—Yes.

6854. Then you refer to the natural experiments, as it were, of epidemic diseases, as in the case of the epidemic of measles in the Fiji Islands?—Yes.

6855. You state in regard to that case that a disease which like measles is comparatively harmless in a population where it is endemic, as in this country, may become a devastating pestilence when introduced into a country like the Fiji Islands, where it was formerly unknown?—Yes.

6856. Do you mean that it was because of the action on a virgin soil, as it were, that the fatality was so great in that case?—I think that was so; I believe that you actually do get people used to the conditions under which they are, both hereditarily and personally. Newcomers, when going to Venice, for example, suffer from diarrhoea and disorders of the digestion, which people who are living in Venice do not suffer from, although they are under exactly the same conditions, drinking the same water, smelling the same smells, and altogether under the same conditions.

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6857. With regard to that particular epidemic of measles in the Fiji Islands in 1874, do you happen to remember the remarks upon it of Hirsch, in his "Historical and Geographical Pathology"?—I do not.

6858. I see that under the head of "Severity of type due to neglect of the sick," he suggests that the mortality in that case was due to the fact that the afflicted were exposed unattended and untreated. That consideration would have to be borne in mind in assessing the cause of fatality there, would it not?—It would; but, of course, in the Fiji Islands, leaving them unattended would not have the same bad effects as it would here. The climate of Fiji has to be taken into account as well.

6959. I see Hirsch suggests that every complication was invited and rendered more intense by this neglect and exposure?—I should think it is very doubtful indeed. I do not think Hirsch ever went to the Fiji Islands. I have never been there either; but I have known people who have been there, and from what I learn about the climate there, it seems to me that Hirsch's remark is perhaps not to be taken too literally.

6860. Do you know also that he suggested that those that were treated did not die at an unusual mortality in that particular epidemic?—No, I did not know that.

6861. With regard to snake poison, I see in the little box you have handed into the Commission there is a paper which states "The best remedy that can be carried out for this purpose" (that is to say, against snake-bite) "is permanganate of potash, the efficacy of which as an antidote to the poison, not only of the cobra, but of other venomous snakes, when properly applied, has been conclusively demonstrated"?—Yes.

6862. Do you agree with that?—It might be advisable for this Commission to get Major Rogers to tell them exactly what he has found. He has, I think, seventeen or twenty cases, or perhaps more, which have been quite successful.

6863. When did Major Rogers begin the experiments to which you allude. What is he?—He is in the Indian Medical service, but he is home just now on leave.

6864. Will you tell the Commission when he began the experiments; you said a short time ago; you did not mention the year?—I think it was about three years ago. It was 1904.

6865. (Chairman.) There have been experiments, I understand, upon animals to test the nature of permanganate of potash?—Yes, there were experiments made by Sir Joseph Fayrer and myself about 30 years ago. Those were stopped in the middle because of the vivisection laws, and they were taken up again, in 1904, I find it is, by Captain Rogers, as he then was, and he was able to continue them because he had got an apparatus for keeping an animal under an anæsthetic for thirty hours, so that he could give the snake poison and give the antidote and see whether the animal lived.

6866. (Sir William Collins.) Are you acquainted with the experiment of Calmette and others with regard to anti-snake venom sera?—Yes, perfectly.

6867. Do you agree that a serum has been found which is valuable against snake bite?—Certainly. But the difficulty is that the serum that is prepared, for example, for a krait, will not act for a cobra, and *vice versa*, whereas this will act for any poisonous snake, for krait, cobra, or water snake.

6868. Then permanganate of potash in your opinion is a universal remedy?—Yes, and more than that, I can testify from my own experience that it is an antidote to ordinary septic poisoning.

6869. And you regard that as conclusively demonstrated?—Yes.

6870. Have the various treatments by serum and your treatment been largely utilised in India?—Not yet. The difficulty of the serum treatment, of course, is that the serum and the snake are not likely to be in the same place. You do not always get the snake and the serum together as in Calmette's case, where he got bitten while lecturing, and applied the serum forthwith, which he made himself. As a rule you cannot use the serum, and even this lancet has not been largely used yet. It is only just beginning to find its way; but I think that already Major Rogers has had 17 or 20 successful cases.

6871. You can hardly say that the mortality from snake bite in India has as yet been reduced by any treatment based upon experiments on animals, can you?—One can say definitely that it has been reduced; it has been reduced; it has been reduced at least by 17 or 20 cases. But we cannot say in what proportion it has been reduced. I do not know how many people have tried this lancet, but in 17 or 20 cases where it was tried it was successful, and in so far the mortality has been reduced.

6872. I see in the year 1905 according to the official figures, there were 21,797 deaths from snake bite in India?—Yes.

6873. A mortality of 94 per million living?—Yes.

6874. Whereas in 1876 there were 15,819 deaths, or a rate of 85.4 per million living?—Yes.

6875. So that apparently the numbers at present are unfortunately increasing?—There is all the more need for a liberal distribution of these little lancets.

6876. Has the Indian Government adopted them?—I do not think it has. If it would simply adopt these little lancets and sell them, as it sells packets of quinine for a few pice at every postage station, the mortality in India from snake bite would be very materially reduced in a short time.

6877. Or distribute them gratuitously?—If they would sell them at cost price, they might be produced on a large scale at about a half-penny each, I believe. Of course the Indian peon is an awfully poor man. The price of a lancet represents so much rice to him.

6878. But if you wish to let your antidote be as universal as the bane, it would be desirable to make it gratuitous?—Yes, it would be.

6879. Can you refer me to a record of the 17 or 20 cases in which the success has been conclusively demonstrated?—I am not sure that I can, because I chiefly quote from a letter to myself from Major Rogers; but he has, I know, published some of them.

6880. Can you refer me to the publication?—No, but I will get it for you willingly.

6881. Is Major Rogers a Government official?—Yes, he is Professor of Pathology in the Calcutta Medical College.

6882. Would the report on these 17 or 20 cases in which the efficacy of the remedy has been conclusively demonstrated, appear in an official report?—I do not know whether it is an official report or whether it is in a medical journal, but it will be in one or the other, no doubt.

6883. Have you the letter by you on which you base your evidence?—No, not here, but I have got it by me. If you would like the letter I can send it on.

6884. (Chairman.) Major Rogers himself is in England, you say?—Yes, I can send you his address. He is, I think, working every day at the School of Tropical Medicine, and he gave the lecture on kalaazar before the Royal College of Physicians two or three weeks ago.

6885. And if the letter is not a private one perhaps you would let us have a copy of it?—Certainly.

6886. (Sir William Collins.) As to the administration of digitalis in pneumonia, I asked the President of the Royal College of Physicians, Sir Douglas Powell, when he was here, whether he would give us his opinion as to any rules which have been obtained as the result of experiments on living animals for the administration of digitalis in pneumonia, and he replied that he was not aware of any experiments which had established the usefulness of digitalis specially in pneumonia?—That is quite true. The usefulness of our experiments was chiefly cautionary. That a man might be tempted to use digitalis in pneumonia and thereby do harm.

6887. Is it the result, in your opinion, that digitalis ought not to be used in pneumonia?—My own belief is that sometimes it may be useful, but as a general thing it is advisable not to use it. Our experiments showed that it is possible to use digitalis in pneumonia and thereby do harm.

6888. Then in regard to amyl nitrite, a question which has been raised by other witnesses, I see that in your *précis* you state that by finding in a case of angina pectoris that the blood pressure rose too high, and having learned through experiments on animals that nitrite of amyl would lower the pressure, you applied

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the drug to the treatment of the disease with perfect success?—Yes.

6889. What was the date of the first publication of yours on the subject?—It was in July, 1867.

6890. In that publication did you refer to a previous work of Sir Benjamin Ward Richardson upon the subject?—Yes.

6891. Did you say "Little attention, however, was paid to it for some years until it was again taken up by Dr. B. W. Richardson"?—Yes.

6892. Have you seen Dr. Richardson's statement in regard to it in his book called "Vita Medica"?—No, I do not think I know it. I only know his published papers in the British Association reports.

6893. It has been doubted whether the discovery of nitrite or amyl was based upon experiments upon animals or trying it on the human subject. Dr. Richardson stated, "Research with nitrite of amyl proceeded in the usual way. I made myself the first victim, and tried the effects of the substance by taking it both in the form of vapour and fluid. I had also some friends who were willing to follow in my wake." Did he not produce it at the meeting of the British Association, I think in 1863, when its effects were noted by some of those present, who tried it by inhalation?—It may have been so; but his chief account of nitrite of amyl is given in the Report of the British Association for 1864, where he refers to an earlier paper which he had read at the Newcastle meeting in 1863; but on looking up the volume for 1863 I cannot find any reference whatever to Dr. Richardson's paper, either in the Index of the Report or in the Index of Communications. I have brought it here so that there should be no mistake. This is the volume for 1863 (*handing in the same*.) So that it cannot have been of any very great importance, I think. Then in the Reports for 1864 there is a very full account, but his reports deal much more, in 1864, with the experiments upon animals than the experiments upon himself. Here are the experiments upon animals (*handing in the volume*). He gives here those upon the skin and so on. Then at page 126 he gives a *résumé* of its action. Then in the next year he took it up again, in 1865, at Birmingham, and he again gave a *résumé* of its action even shorter than what he gave in 1864. Perhaps I might read the *résumé* that he gave himself. He says, "Before proceeding further, permit me to invite the attention of the section to a very brief summary of the facts brought forward at our last meeting in 1864, respecting the nitrite of amyl. I showed then that nitrite of amyl was: 1. The most powerful excitant of vascular action known. 2. That in animals whose bodies admit of its spontaneous evaporation, the nitrite suspends animation, and that in animals of higher organisation it induces a condition resembling cataleptic insensibility. 3. That it is not an anæsthetic. 4. That the effects produced by it are developed on the motive powers of the organism, which it first vehemently attacks, and then paralyses. 5. That it arrests the process of oxidation." So that, you see, all he did there was to show that it was a "powerful excitant of the vascular action," without going into details. He did suspect, and correctly, that the excitement of the heart was probably due to the effect of nitrite of amyl in dilating the capillaries and lessening the resistance; but he did not pursue that any further. He went on, in his report of 1865 and later, to discuss the question of the action of other compounds of amyl, and left the nitrite, which was the most important, alone; so that the question of how it acted remained unsolved. It was then taken up by Rutherford and Gamgee, who made experiments upon animals with an ordinary manometer, and found that the blood pressure was greatly reduced by nitrite of amyl. At that time I was working a good deal in the laboratory with Gamgee, and knew all about the experiments. I had made none on nitrite of amyl myself, but I was very susceptible to nitrite of amyl, and Gamgee very often tested it upon me, taking traces of my pulse; and then I saw the extraordinary depression in the tension of the pulse which was produced by nitrite of amyl. At that time I was house physician in a hospital in Edinburgh, and there was one poor man in the wards suffering dreadfully from angina pectoris; he used to have an attack every night, and for two hours the unfortunate man would sit on the edge of his bed and could not move forward, backward, or to one side, with his face pale and the sweat pouring off it, in perfect agony. I used to sit by him and try to do what I could to ease his pain. I gave

him chloral, I gave him ether, alcohol—everything I could think of. It was all no use. I took tracings of his pulse to see what was happening, and I found that his pulse during the attack became very tense. I tried everything that I could think of, and one day he said, "Well, you have been very good, but it is no use; let me go." I said, "Well, I will try one thing more, and if it does not do you any good you shall go out to-morrow." I went straight up to Gamgee and asked him for some nitrite of amyl, because I thought if nitrite of amyl lowers the tension so much, and this poor man's tension rises during the attack, if I gave amyl it ought to ease him. I brought it back and gave it, and it was perfectly successful. But for the experiments I should never have known the action of nitrite of amyl—nobody would have done so. Richardson's experiments upon himself, you see, simply showed that it flushed the face, that it excited the action of the heart, and produced palpitation; and produced a feeling of giddiness and wanting to sit down, and that was all.

6894. Does not Richardson say, "My description of the effects of the nitrite was that it relaxed the living muscles"?—I suppose that was what he meant to say; I do not think it is in the reports.

6895. Is anything you have quoted incompatible with Dr. Richardson's description of his own researches of 1867, namely, that "the research with nitrite of amyl proceeded in the usual way. I made myself the first victim and tried the effects of the substance by taking it both in the form of vapour and fluid"?—No; he probably tried it, but these experiments would not have helped—his observations did not help at all with regard to angina pectoris.

6896. But you referred to his experiments in your paper of 1867, did you not?—Yes, surely, because it was he who drew attention to the action of the drug. He drew attention to it, and Gamgee and Rutherford followed it up and worked it out, and then I applied it.

6897. (*Chairman.*) How do you trace the benefit of that discovery to experiment on animals?—Because, but for the experiments on animals made by Gamgee and Rutherford, we should not have known that it reduced the pulse tension.

6898. (*Sir William Collins.*) In a letter to the "British Medical Journal" on the 16th of March of this year I see you state that "any benefits which may result to patients from either plan (the inhalation of oxygen or the injection of strychnin) must be regarded as the fruits of experiments on animals"?—Yes, because in regard to strychnin, the first experiments that were really of much use in strychnin were made by Prokop Rokitsansky in Professor Stricker's laboratory many years ago. Usually, when you cut the spinal cord across, just at the junction of the head and spine, all respiratory movements cease; but if you inject strychnin beforehand they do not quite cease, and if you inject strychnin afterwards you get certain respiratory movements, imperfect no doubt, but still an attempt at respiratory movement, showing, apparently, there is a certain amount of respiratory centre which extends a little way down into the spine. In consequence of this, one has been accustomed to give it in failure of respiration. Then in the case of the heart, if you tie a ligature around a frog's heart, the junction of the venous sinus and the auricle, it stops; but if you inject a little strychnin into it it will begin again; and if you inject strychnin beforehand, before tying the ligature round, it does not always stop. This appears to me to show—although I daresay Dr. Gaskell may have a different explanation to give of the fact—that the strychnin acts as a stimulant to the heart; and, practically, when we have come to try the result upon patients, we find that strychnin has got a stimulating action in keeping up the respiration and keeping up the heart in cases where both seemed to be about to fail.

6899. I was going to ask you whether the claims you make on behalf of oxygen and strychnin are generally accepted by the profession?—I believe they are.

6900. I notice that on the 23rd of March, this year, in the "British Medical Journal," Sir James Barr seems to speak rather disrespectfully of your discoveries. He says, "Now for Sir Lauder Brunton's other discovery of the use of oxygen and strychnin in pneumonia. I am pleased to think that the craze for cylinders of oxygen is dying out, notwithstanding this spasmodic attempt to revive it." And again he

says, "I rather hesitate to conjecture how many persons have been poisoned by it since that fateful day when Sir Lauder Brunton 'scribbled the other half of his paper in a railway train.'" Perhaps you would like to make a statement in regard to that?—I have heard from a good many doctors since, and they do not agree with Sir James Barr, and they do agree with me. A man of large clinical experience, for example, in Scotland, who has got one of the largest consulting practices in Scotland, as well as a hospital appointment, says that he quite agrees with me and does not agree with Sir James Barr.

6901. I need hardly say, as an old pupil of yours, that I have great respect for your opinion.

6902. (*Mr. Tomkinson.*) I notice that you used the word "narcotised" always. You mean that to be equal to anaesthetised?—It is something more than anaesthetised.

6903. The use of opium, chloral and morphia produces a state of insensibility?—Yes.

6904. Is that at all akin to that insensibility which is produced in a hopelessly drunken person that we see sometimes?—Yes, but to a much greater extent.

6905. But one does occasionally see, unfortunately, people apparently absolutely insensible from drink. Would you say that those people were insensible to pain?—Yes.

6906. Do you mean that although it is almost impossible to rouse them, if an operation were performed on them they would not cry out or show some sense of pain?—Certainly they would not. That observation is as old as the time of Solomon, because Solomon makes the drunken man say, "They have beaten me, and I felt it not. I will go and have some more—I will seek it yet again."

6906A. Then in regard to that case which has been called the baking of dogs, I think it is well it should be absolutely understood that in the case of the first experiment it was merely an artificial external means of keeping up the temperature of the animal while it was under this dose?—Yes, that is so.

6907. And to prevent the loss of temperature, the loss of heat, which is induced by that dose, unless it is kept in by artificial means?—Yes.

6908. And I understand those artificial means were merely external in the shape of a warm foot-warmer and cotton wool?—Yes.

6909. And that it would not be applied to an animal in a greater heat than possibly a very warm bed into which a patient would be put?—Yes.

6910. And could in no case cause pain or burning, or anything to the skin?—No, in no case would it have caused pain, or burning, had the animal not been narcotised.

6911. A process such as that would hardly need any protection from the law?—Those experiments were done before the law was passed.

6912. It would not render the operator or exhibitor of it liable to prosecution for cruelty to animals?—No; but in some of those cases, in addition to this outward case, after the animal was narcotised, a cannula was introduced and warm air was passed in, and then that, of course, became an operation.

6913. Was not that the second series?—The second series.

6914. But the whole statement about this heating never exceeded a temperature of 111 degrees, which the animal was called upon to inhale?—We never reached as much as that. It was the animal that reached that temperature, but the external temperature was never so much.

6915. (*Chairman.*) I think it was 106° you gave?—Yes, as the external temperature.

6916. (*Mr. Tomkinson.*) Do you know what the normal temperature of cats and rabbits is?—The temperature of the rabbit is an exceedingly variable thing; it has very little power of preserving a steady temperature; I speak under correction by Dr. Gaskell, but my impression is that it runs practically from about 100° to 104°.

6917. Rather higher than in man?—Yes, and in pigeons it is about 104°.

6918. You do not often use curare?—No, I have very

rarely used it. I do not think I have used it at all since the passing of the Act, because I always had the feeling that curare alone might not completely destroy sensibility. I may, perhaps, mention that I was very anxious to find that out, and when I was in Leipsig in 1869, I proposed to Professor Ludwig to give me curare, and then, perhaps, apply a hot iron or something to the back of my hand, and keep up artificial respiration till I came to life again; and then I would tell him whether it would hurt or not; but he said that the risk was too great, that the means of keeping up artificial respiration were not sufficiently good, and he thought it was not worth while to do it—and he did not do it.

6919. With regard to inspection, you are not favourably disposed to increased inspection?—No, I do not think it would do any good.

6920. Have you seen a letter, in "The Tribune," I think it was, from Miss Lind-af-Hageby, headed "Vivisection and Inspection"?—No.

6921. She writes that while in the years 1903, 1904 and 1905, the number of experiments performed was 89,581, the united labours of the two inspectors led them to witness 93 experiments only, leaving the cost to the country, taking their salaries, 2,550 guineas, whilst 89,488 experiments were left uninspected. That shows a very small fraction of inspection of the experiments that take place. Now, in regard to inspection by the public, we will say, would it not be well, do you think, in order to set the public mind at rest, from your point of view, that there should be some right of entry by authorised people occasionally to witness experiments?—I do not know that it would.

6922. What would be the objection?—The objection would be that they would describe things in a way that was coloured by their own imagination. In the same way as they misstate what I have done with regard to warming animals, so they would be apt to describe any experiments in a falsified way.

6923. I did not in the least mean a newspaper reporter for the sake of copy, but, say, medical men, or perhaps Members of Parliament, on presentation of their card?—No, I think that anyone who wanted to inspect should be authorised. There would be no objection to having them if they applied to a central authority, the Home Office, and the Home Office thought they were right and proper persons to see them—then, by all means, let them go. But it should not be merely on presentation of a card. The Home Office should be certain that the person was a right and proper person to go and see the thing, and then, of course, there could be no objection whatever.

6924. (*Chairman.*) I will go back now to the general line of your evidence. I will leave out those matters which you have already dealt with in answering the members of the Commission who have been examining you. It has been suggested to us by several witnesses that the results of experimenting on animals and relying on the results of experiments has dulled the edge, as it were, of clinical observation, and that doctors do not now give the same attention to clinical observation that they did before. Have you formed any opinion as to whether that is so—do you think it has made any alteration whatever in the zeal with which a doctor pursues his clinical observations when he is attending a patient?—I do not know that that is so. Experimentation on living animals has given doctors so many more means by which they can readily ascertain things assuredly, that perhaps in some respects they may be said not to have the same zeal as before. To take an example unconnected with experiments upon animals, formerly a doctor had to depend entirely for his knowledge of fever upon inspection of the tongue, the feeling of the pulse, and the feeling of the skin. Now, instead of taking so much trouble with the pulse and skin he not infrequently just puffs a clinical thermometer into the mouth; he reads off the temperature precisely, and he knows exactly what the fever is. He does that, of course, in less time and with less trouble, and he prefers to do it in that way. Perhaps if there were two doctors, one of whom was accustomed to trust to the thermometer and the other had never known of the thermometer, and they were put down by the bedside of a patient, if the doctor who used the thermometer had broken his thermometer he would not be so good as the man who had never used one.

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6925. But taking the whole knowledge and instruction of doctors together and assuming what you say about clinical observation, do you think that their power of dealing with disease has increased or the reverse?—It has increased incomparably; and I can speak to that because I came in just at the time when the change was beginning. When I was a student there were no remedies for producing sleep except opium, henbane, and tartar emetic. When a man had no fever on him we gave him opium or some henbane; and if he had fever we gave him opium and tartar emetic. I remember perfectly well when I was house physician one of the other house physicians got ill with typhus, and he was very sleepless, and we watched with great interest the introduction of a new remedy, bromid of potassium, to see whether it would give him any sleep. Since that time we have had any number of drugs to produce sleep. We have got chloral, veronal, sulphonal, and trional, and a whole lot of others, and we can now produce sleep in a way that we could not dream of doing before.

6926. Was bromid of potassium discovered by experiments on living animals?—I do not think so—it is a rather difficult thing to trace how it came into use. My own belief is that it came in rather by chance observation upon men.

6927. Were these other soporifics to which you have referred discovered by experiments on animals?—Entirely.

6928. Have they the same kind of operation as bromid of potassium to the same extent, or have they advantages over it?—They have considerable advantages. Bromid of potassium is more of a general nerve sedative rather than a sleep producer. These others are distinctly producers of sleep or soporifics, and they have all been made upon definite principles. They have all been tried upon animals, and have been introduced in consequence of experiments upon animals.

6929. And in those experiments on animals is the object first to discover the effect—whether they cause sleep, and then to discover also what is a safe dose?—Yes, both those things.

6930. And are experiments of both those kinds necessary before you venture to try them on mankind?—Yes, absolutely; because the life of an animal is so much less valuable than the life of man, and we cannot afford to test these things upon man.

6931. That is only an instance that you have been giving us in the matter of producing sleep; but I was asking you a general question, and I think you answered it in the affirmative, that you thought there had been a great improvement of knowledge?—An enormous improvement.

6932. Do you see any reason to suppose that that increase of knowledge would have come from mere clinical observation without experiments upon animals?—No. One may say that it certainly would not have come, because clinical observations have been carried on for very many centuries, and have led to nothing. Taking again the case of sleep, clinical observation has been carried on for millions and millions of years, and all that time opium and henbane and tartar emetic were the only results; whereas in 50 years we have got any number of new drugs.

6933. Leaving out the question of surgery?—I am speaking only of the question of hypnotics or soporifics.

6934. But my question was rather general—would your answer apply to general medical knowledge?—Yes.

6935. And surgical knowledge?—And surgical knowledge as well.

6936. You refer in your *précis* to the value of various discoveries by experiment, with which I need not trouble you, because we have heard of them from several witnesses—Harvey's discovery of the circulation of the blood followed by others afterwards. With regard to the diagnosis of cardiac disease is that a thing in which experiments on animals have improved knowledge?—Our recognition of heart disease is based upon the change in the cardiac sounds. The explanation of those changes was first afforded by a Committee of the British Association who made experiments on animals and ascertained definitely that certain alterations in the cardiac sounds were associated with definite changes in the valves of the heart, and it is to those

experiments that we owe our power of recognising the presence of cardiac disease.

6937. What was the date of that Committee?—1836, I think.

6938. Then in the treatment of heart disease, has advance been made in consequence of experiments on animals?—One may say that the whole treatment of heart disease is really dependent upon the results of experiments upon living animals.

6939. Both as to drugs and as to regimen generally?—Yes, in regard to drugs especially one may say. When I was a student digitalis was regarded as a drug which would weaken the heart, and was to be avoided in all cases of cardiac weakness, whereas now we know that digitalis is an extremely powerful tonic to the heart, and we use it in cases of cardiac weakness instead of avoiding it.

6940. I do not know whether you have had an opportunity of reading the evidence which Dr. Cushny gave before us?—Yes.

6941. Do you agree with his evidence about digitalis?—Yes.

6942. Then I need not take you into it at length, because he went very elaborately into it?—Quite so.

6943. Is the study of blood pressure in the blood vessels a matter of great importance?—It is, I think, a matter of the utmost importance in regard to the prolongation of life, because it is just when a man has gained most experience and has got the widest power that he very often begins to fail, and very often dies off suddenly, or to suffer from some condition of the heart which prevents him from taking any active part in life. The condition which arises with advancing years is one of increased tension within the vessels. We can now recognise that tension by means of instruments which have been worked out in consequence of experiments upon animals; but this would be of little use to us were it not that by means of drugs, which have also been worked out by experiments on animals, we are able to reduce this tension and to keep it down so that we are able to preserve lives for many years after they would otherwise come to an end.

6944. Why do you say that you discovered the state of tension by virtue of experiments on animals?—Because it is only by experiments upon animals that we have learned anything about the tension inside the vessels. The first experiments upon this point were made by the Rev. Stephen Hales, under the auspices of the Royal Society. He put a tube into the carotid of an animal and saw how far the blood mounted in the tube.

6945. We need not trouble you with regard to that; we have had it gone through?—But shall I explain the practical value of it.

6946. If you please?—When you take the blood pressure of a man (I wish I had brought the instrument down; it is very easily done; you can take it in half a minute now), if you find that the blood pressure is low, you can give the man a tonic which will act on his heart and vessels and raise the temperature up to the normal, thus increasing his power; but if you find that the blood pressure is above a certain tension, then you know that that man is liable either to have a vessel give way in his brain, and so to become paralysed, or to have the tension become too high for his heart to withstand, and then the heart begins to fail and stop, or he gets angina pectoris. In such a condition you can put him upon a diet and upon medicines which will lower his tension and keep it at the normal, thus preserving his life or preventing either the paralysis or the cardiac pain which would otherwise come on.

6947. I think you have already explained what you have to say about the effect of nitrite of amyl on angina pectoris, in answer to Sir William Collins?—Yes.

6948. I need not ask you any further questions about that. As regards dropsy, have there been considerable discoveries made in consequence of experiments on animals?—Yes, because through experiments on animals we now know that we can influence dropsy in more ways than one; that we can influence it through the blood vessels by preventing too much fluid being passed out, or we can influence it through the kidneys by causing them to secrete more freely, and we can influence it also by drugs which act upon the com-

position of the blood and prevent exudation from the vessels.

6949. These are things that you have learned to do since the beginning of the era of experiment?—Surely. These have all come within the last fifty years.

6950. These are remedies which had not been discovered so far by clinical observation?—That is so.

6951. I do not know whether you have said all that you wish to say about respiratory stimulants and strychnin?—I have said all I wish to say about that.

6952. I think you wish to tell us something as to abdominal surgery?—I regard abdominal surgery, really, as starting with some experiments upon animals. The first experiments that were made from which one can trace more or less continuously the present use of surgery in abdominal disease were the experiments of Ludwig and Thiry, who were investigating the action of intestinal juice. They cut out one piece of intestine and sewed the ends of the cut intestine together, making a continuous tube, leaving one part out.

6953. These were experiments on animals?—Yes. These experiments were continued for a long time, more or less, only in the laboratory, and then they were taken up by the surgeons, more especially by one of the Viennese surgeons, Billroth, who did a great deal in that way, and I think that Billroth's taking them up was really due to the influence of his colleague Professor Stricker, Professor of Pathology.

6954. And have these experiments made possible that and other operations in the intestines which were not possible before?—Yes, of course.

6955. (*Sir William Church.*) You would not say that directly; you would not say that those operations alone have made that possible; there have been others?—I meant that those were the starting point; and from those, I think, you can find a regular succession.

6956. (*Chairman.*) Of experiments of the same kind in that direction?—Yes.

6957. (*Sir William Church.*) That must be taken in connection with Listerism, for instance?—Experiments made before Listerism are like operations before Listerism; they would very likely be unsuccessful, and it is only since the introduction of antiseptic precautions that these experiments have been done very much in the laboratory or elsewhere.

6958. (*Chairman.*) I do not think it is necessary to ask you upon every one of these particular instances that you give, but you attach great importance, I gather, to the result of experiments with regard to myxedema and cretinism?—I do.

6959. Would you tell us what you have to say with regard to that subject?—I thought that had been taken up by some of the other witnesses. I do not know that I can give you such a connected history of myxedema and cretinism as some other man would, Sir Victor Horsley, for example, but practically it was found that the removal of the thyroid gland produced a condition in which cellular tissue became thickened and the animal became enfeebled; and in cases where that was done in man it produced a condition of imperfect action of the mind resembling those cretins who used to be found so commonly in some of the Swiss valleys; so that the function of the thyroid gland became connected with this condition of the thickened connective tissue and stunted growth and feeble development of the mind. It was then found that by giving the thyroid gland in this condition, especially to children, the feebleness of mind disappeared, the child began to grow, and at once the dwarfed idiot began to develop into a perfectly normal man or woman.

6960. Has this remedy of the thyroid gland been tried in the Swiss valleys, do you know?—I do not know. I have no doubt it has; but I should think there must have been something else that has led to the disappearance of the cretins; because 50 years ago in the Zermatt Valley you found the whole place crowded with them, with those enormous goitres; and before the introduction of the use of the thyroid these had to a great extent disappeared, so that there must have been some other factor, though I do not know what it was, which led to the disappearance of cretinism in those valleys. But this condition of myxedema now is very commonly recognised at once, whereas it was not recognised before, and we find that in almost every case now we are able to remove it, for a time at least, by the administration of thyroid gland.

6961. (*Mr. Ram.*) How is it used—hypodermically?—No; at first it was given hypodermically, but then we found it could be given perfectly well in tabloids, or the extract of the gland in solution could be taken internally and produced all its effects.

6962. (*Chairman.*) I forget from what animals the thyroid is taken?—I think generally they take it from the sheep.

6963. (*Sir Mackenzie Chalmers.*) From sheep killed in the ordinary way?—Yes.

6964. Immediately after death?—Yes, immediately after death—I believe so; but I am not quite sure whether other animals may not be used instead.

6965. (*Chairman.*) Some witnesses have made a great point—I do not know that it has been supported by any very precise evidence—of the danger of introducing the materials which form part of another animal into the human body, some upon the ground that there is something abhorrent to the feelings in it, and others that it gives to the person who takes that remedy some part of the susceptibilities of the animal which to some extent affect the mind and body of the person according to the animal from which it was taken. Do you, as a scientific man, see any foundation for that?—No. Of course the whole idea of eating meat at all is to many people abhorrent, but otherwise I do not see anything in it.

6966. You do not see any difference between eating a slice of a leg of mutton and some of the thyroid gland, as far as that argument goes?—No.

6967. Does it make any difference as to whether you give it through the mouth or injected into the body?—Yes, in some cases at least. For example, the blood of an animal cooked which you take into the human stomach is digested there and does no harm; but if it is injected into the carotid or into the jugular vein it would do harm. That is the reason why the old plan of infusing the blood of animals into man was given up. But there is no other reason for supposing that there is any special action.

6968. Of course it is very much the same as the objection taken to vaccination from a healthy calf?—Yes.

6969. From an unhealthy one, I suppose there would be an objection?—Yes. And then, of course, one does not like to say, that in a very widespread superstition there may be no element of truth; because we find that cannibals believe that if they take the heart of a strong man and eat it, they get his strength going into them; and the Hottentots and other uncivilised nations are very fond of eating the heart of a lion, so as to gain the strength and courage of the lion; so there may be something in it, but I do not think there is a great deal.

6970. I see you say that one of the most important inventions in regard to the relief of pain, may be considered to be the introduction of hypodermic medication, which is the result of experiments made upon animals?—Yes.

6971. I do not quite follow that. It would seem to me that there is nothing in this that could not be done by clinical observation?—It is quite true.

6972. That any hypodermic injection might serve the purpose, instead of swallowing?—The way it came about was this: Sir Robert Christison was a student of Orfila's in Paris, and Orfila made a very complete series of observations on the action of all kinds of drugs upon animals, which he recorded in his *Toxicology*. Sir Robert Christison was a pupil of Orfila's, and used to talk a good deal about Orfila and his experiments; he talked about them to old Dr. Alexander Wood, and Alexander Wood somehow or another thought that instead of using the intravenous injection of which Orfila was so fond, if he injected into the subcutaneous tissue he would get the same result; and he tried it, and it answered; and that is how it came about. I do not know whether Dr. Alexander Wood published definitely this, but what I am giving you now is the result of what I used to hear when I was a student, when I knew Christison, and slightly knew Wood.

6973. (*Sir William Church.*) Of course the improvement in chemistry had something to do with it?—Yes, undoubtedly.

6974. But it would not have been possible for Orfila to have made use of many drugs which are now used hypodermically, because their active principles had not been extracted in the shape that they now are?—No,

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but at first when Wood introduced the syringe it was employed almost entirely for morphin.

6975. Now, of course it is used for many other drugs besides morphia, and do you think with advantage, for instance, quinine in cases of malaria?—Yes.

6976. And also mercury in cases of syphilis?—I do. In the cases of malaria especially one knows that there are certain cases that do not yield to quinine given by the mouth, and do yield to quinine given by the hypodermic syringe. In regard to syphilis I have very little experience, because I do not see many of those cases, and I have no experience whatever in the case of mercury hypodermically given.

6977. In certain maniacal cases it is sometimes useful when you would have great difficulty in getting the patient to take the drug by the mouth?—Yes. In such cases it is of the utmost service, because the injection of a very small dose, even the hundredth of a grain of hyosin will probably quieten them down.

6978. Also apomorphia in certain cases of poisoning?—Yes, in such cases you can get active vomiting in a very short time, quicker than you possibly could do it otherwise.

6979. (Sir Mackenzie Chalmers.) By subcutaneous injection?—By subcutaneous injection; and then, of course, in the case of a bit of gristle just sticking in the throat and gradually choking the patient, if you can inject hypodermically apomorphin and get him to vomit, the gristle will be ejected.

6980. (Chairman.) Then with regard to experiments for the purpose of teaching, what is your opinion about that?—I believe that in order to thoroughly teach youths physiology and pharmacology in the way they ought to know it, you must have experiments. I have had both kinds of training. When I was a student no experiments whatever were shown, and I learnt my physiology from text books and from diagrams. I then went across to Germany and learnt it there by demonstration; and the difference between the two is just as great as learning how to put on a splint by reading an account of it in a book and doing it yourself. You get a practical notion of your physiology from demonstrations that you cannot possibly do from lectures.

6981. It cannot be done by anatomising?—No, it cannot be done, because anatomy deals only with structures, and physiology with the living functions.

6982. You approve of that course being followed?—Assuredly. If there was any pain involved to the animals, I should say that we had better not do that; but as it is a question where there is absolutely no pain involved to animals and a great deal of advantage to be gained by students and thereby a great deal of pain, I believe, saved to patients afterwards, I think it is the bounden duty of the teacher to give demonstrations.

6983. You think that anaesthetics ought always to be used?—Surely.

6984. (Sir Mackenzie Chalmers.) You would not allow the animals to recover, as I understand; you would limit it to operations where the animal was not allowed to recover?—I am not sure of that.

6985. (Chairman.) I believe you yourself were a witness before the 1876 Commission?—Yes.

6986. So far you are the only witness before that Commission who has appeared before us?—The others are dropping off.

6987. (Mr. Ram.) You say in your *précis* that when you were carrying on your investigations you were cut short by the difficulties occasioned by the Vivisection Act?—Yes.

6988. What was it in the Vivisection Act that squeezed your experiments out of existence?—The difficulty of getting a licence for a number of experiments without anaesthetics. You see, in order to investigate the action of snake poison, the injection of snake venom was requisite. This was looked upon as an experiment, and an injection of or application of the permanganate of potash by an incision was also looked upon as an experiment without anaesthetics. Of course the amount of pain inflicted was very small indeed; but then it was regarded as an experiment without anaesthetics; and the difficulty of getting leave for a sufficient number of such experiments was great, so great that we had really to give it up.

6989. But you were able to take to it again in 1904?—

Colonel Rogers then did the experiment, but he was able to do it in this way: that in the interval Professor Waller had invented an apparatus by which chloroform anaesthesia could be maintained without interruption mechanically for 30 hours or more; so that Colonel Rogers was then able to do the whole thing under anaesthetics, and of course there was no difficulty in receiving a licence to do a number of experiments under anaesthetics.

6990. I am obliged to you; that answers that point. You say also in your *précis* that it seems to you that the law against experimentation ought to be very much relaxed?—Yes.

6991. Are you able to indicate in what direction you would relax it?—I should allow greater freedom, I think, in regard to experiments in which no actual pain is inflicted, for example, such as the inoculation experiments. It is simply inoculation of a microbe, but there is no actual pain; the experiment itself is really devoid of pain. These are now becoming among the most important of all the experiments; and in regard to them I should allow much greater freedom. I should not regard many of them as experiments at all. Perhaps I may just mention the view of the late Professor Huxley. He said to me one day—he was then a member of the Fisheries Commission: “If I take a live minnow and put a hook right through it, and then spin it round alive until I catch another live fish, and I pull the live fish, after many struggles and a length of time, out of the water, I am perfectly entitled to do that because I do it for sport; but if I scratch the nose of a salmon for the purpose of ascertaining the nature of the salmon disease, I am making an experiment, and if I have not got a licence I am liable to a fine of £50.”

6992. Then as regards your relaxation that you were speaking of, in that direction would your wishes be met if a number of experiments of this non-painful nature were allowed; and would you still retain the law as at present, that that can only be done by a licensed person on licensed premises?—No, that is just where the difficulty arises, where you have a combination of a licensed man and a licensed place. If you license a man you might let him experiment in an unlicensed place. For example, other witnesses can tell you much more about it, but I believe the examination of grouse disease by a licensed man is rendered exceedingly difficult, or almost impossible sometimes, by insisting also upon a licensed place.

6993. We have had that case brought before us?—So I should say practically that if you licensed the man or the place, an experiment done in a licensed place under licensed supervision even by an unlicensed man might be allowed, or done by a licensed man giving a proper report in an unlicensed place.

6994. Would you limit that to experiments strictly non-painful?—No.

6995. The experiment might be painful and you would ask that it should be done by a licensed man, either in a licensed or an unlicensed place, or by an unlicensed man, provided the place was licensed?—Yes, that is the position I take.

6996. Are you aware of the state of things in America to-day?—I know that in many places there is a great agitation being got up, very much the same as in this country.

6997. In favour of what?—In favour of anti-vivisection.

6998. Do you know that there is a large body there of scientific men who are asking not to have no regulation on vivisection, but to have exactly such an Act as we have here?—No.

6999. I think I have only one other matter to trouble you with. In your *précis* you say that it is through experiments on animals also that the nature of the most dreadful of all respiratory diseases, namely, phthisis, has been discovered, and that we have already begun to lessen, and you trust ere long will stamp out the disease. Can you elaborate that a little? What is being done with regard to ascertaining the nature of phthisis?—That has now been ascertained to a very great extent indeed. We now know that phthisis depends upon an invasion of the lung by a certain bacillus, the tubercle bacillus; that in many cases, however, the infection is not a pure infection but a mixed infection. That there are various other microbes associated with the tubercle bacillus, and

which aid its invasion of the body. But if we could simply prevail upon all people to recognise the infective nature of phthisis, to prevent spitting in all public places, to take proper precautions against the conveyance of the tubercle bacillus from one individual to another, in 30 years phthisis would be just as rare in this country as leprosy is now.

7000. Is that true also of all tubercular disease?—I believe that all tubercular disease could be stamped out entirely in this country in 30 years. The chief objection to it is the expense; because in order to stamp out tuberculous disease, you would probably require to slaughter a large number of cattle; and that is the crux of the matter in regard to animals, and in regard also to the tubercle, more especially in the abdomen of children.

7001-3. Is it your view that tubercle in the abdomen of children is contracted from cattle, from milk?—I think so.

7004. (*Sir Mackenzie Chalmers.*) From milk or meat?—I believe it is much more from milk; because the heat that is used in thoroughly cooking, either by boiling or by roasting meat, is sufficient to destroy the vitality of almost all bacilli; and milk is a vehicle in which bacilli can be conveyed, because it is almost always used without being sterilised.

7005. (*Mr. Ram.*) I think I understood you to say that the use of bromid of potassium as a sedative came in, you think, from clinical observation?—I think so; but I am not quite clear about it.

7006. That was after the lapse of centuries of clinical observation?—Bromid of potassium was really only a more or less new product.

7007. Has the use of useful drugs which are now commonly used increased very largely since experimentation came in?—I do not quite know that I follow that question.

7008. What I mean is this: Of course, all drugs in the days before experimentation on animals came in by clinical use?—Yes.

7009. And it took a long term of years to get a certain knowledge of a certain number of drugs?—Yes.

7010. Has the use of drugs which have turned out to be useful been very much more rapid; has there been a large number of drugs added to the repertoire of useful drugs since experimentation on animals?—Yes, the increase in the number of useful drugs added to those which were formerly employed has been very great since the introduction of experiments on animals.

7011. Has experiment upon animals also had this effect, that it has given an accurate knowledge of previously known drugs of which the use may have been mistaken before that?—Yes.

7012. (*Sir William Church.*) With your experience should you say that there was any difficulty in keeping animals under the influence of anæsthetics for lengthened periods?—None whatever.

7013. Not in the case of dogs?—No.

7014. Do you think that physiological experiments, not directly connected with medicine, have been of great value to medicine?—Yes, an enormous advantage. Many of the experiments which have had apparently little or no connection with medicine at first, have turned out really, afterwards, to be of the utmost possible value.

7015. You remember, perhaps, the way in which chloral was discovered?—Yes, I do.

7016. Do you think it is not probable we should have arrived at the use of chloral if it had not been for Liebreich's experiments?—No, we certainly should not.

7017. That was one quite unconnected with practical medicine—his original experiment. I will not trouble you with it, because we have it from Professor Cushny; I only wanted to get confirmatory evidence; but his original experiments had nothing to do with practical medicine?—They were originally intended more from a scientific point of view. It was from the scientific point of view that he started.

7018. And the action of sulphonal in the same way, I think, was not found out by experiments directed towards practical medicine?—No, it was incidental; it came out in a by-issue in some other experiments.

7019. Was not also the same the case with regard to antipyrin and antifebrin?—No, those, I think, were really the definite results of experiments made on animals upon definite lines. The starting point of the connection between chemical constitution and physiological action was really, I think, in modern times made by Dr. B. W. Richardson. Somewhere about 1844, Dr. James Blake, under Dr. Sharpey's direction, made experiments on the relations between physiological action and chemical composition; but then there was a great lapse of about 20 years, and then Dr. B. W. Richardson took up the chemical series of the alcohols, showing that there was a relation between the chemical constitution and the physiological action of the different alcohols. Following upon this came the experiments of Crum Brown, and Fraser, who found that if you alter the chemical constitution of strychnin by the introduction of methyl, its physiological action becomes altered, and instead of becoming a convulsive poison it becomes a paralyzing poison, like curare. Then after that followed a number of experiments with the benzene compounds, and it was found that many of them had the power of lowering temperature. A series was then produced of synthetic chemical compounds of the benzene series. These were tested, and various of them were found to be practically very useful antipyretics.

7020. To interrupt you for a moment, was not the idea of the original experiments of antipyrin that it was antiseptic rather than antipyretic; that it counteracted septic fever in animals and fever produced artificially in animals?—It is quite possible that you may be correct, because the antipyretic and antiseptic actions are very closely combined in the benzene series; so that—I cannot quite recollect it now, I have not looked up the subject—it is quite possible that they were working on antiseptic lines, and found that the antipyretic power was the one most definitely pronounced. But those two lines run quite parallel; so that now we have got a great body of artificial antipyretics and antiseptics produced which are all made by the trained chemists who prepare these substances on definite chemical lines, and then they are tested physiologically upon animals. If they are found to be satisfactory they are then used upon patients.

7021. Could you tell us how cocain came to be introduced into medicine?—So far as I can recollect now, Von Anrep was the first man to introduce it.

7022. Is it not the case that he was experimenting for physiological purposes rather than for a practical end?—Yes.

7023. Up to that time cocain was not known as an anæsthetic, but as a stimulant enabling fatigue to be borne?—That is so.

7024. (*Colonel Lockwood.*) Do you say that that was the case?—Yes.

7025. That he was experimenting for other purposes?—Yes, physiologically. The first experiments upon cocain which might lead to a knowledge of its anæsthetic action really were made by the late Dr. A. Hughes-Bennett and Professor MacKendrick, because they demonstrated that it destroyed the power of sensory conduction in the cord; but for some reason or other these experiments were not followed up, and it was Von Anrep who actually gave the first actual impetus to the introduction of cocain into the practice of medicine.

7026. (*Sir William Church.*) Then he was followed by Koller, who made use of it for ophthalmic purposes?—But if I remember rightly it was used only for ophthalmic purposes in man after it had been previously tested on the eyes of animals.

7027. And now local anæsthesia for which cocain, or eucaïn, or one of the similar bodies is used is a very important addition to medicine?—Yes, it is, and in addition to that purely local use, it is now being used by introduction into the spinal cord, which is a form of use which is rather based, one would say, upon the very old experiments of MacKendrick and Bennett.

7028. With regard to some experiments which have been mentioned in this room by witnesses on both sides, experiments upon digestion and upon the passage of food down the intestines, it has been stated here that all that was of value was learned by Dr. Beaumont from the examination of the stomach of Alexis St. Martin. How far do you think that is true?—That is not correct, of course. Dr. Beaumont's experiments were very valuable indeed, because it was

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a case of experiment upon man. The side of poor Alexis St. Martin was blown away by a charge of duck-shot, so that they could see inside, and Dr. Beaumont made very good use of his opportunities; but that only went a very short way. It showed that the gastric juice had the power of digestion; it showed that when Alexis St. Martin drank too much spirits his stomach got very much out of order, and that was about the end of it. Dr. Beaumont also found that if he stirred up the stomach very much with a glass rod, or something of that sort, Alexis St. Martin might feel a little sick; but the whole of the rest of the practical introduction of such things as pepsin, pancreatin, and all the adjuncts to digestion have resulted from the experiments upon animals, made afterwards by Blondlot, by Bernard, Bedder, and Schmidt, and others, and the most recent have been made by Pawlow.

7029. In your opinion is gastric fistula in an animal or an intestinal fistula in an animal a condition in which it suffers great pain?—Certainly not.

7030. You say that confidently?—I say that confidently.

7031. Why do you speak so confidently?—Because years before the Act I made a gastric fistula in an animal, in a dog, which never showed the slightest sign of pain, and when I wanted to examine inside the stomach it showed great delight—just like a dog that has been sitting about the house and wants to run out for a walk. When it saw that I was going to look into its stomach it frisked about in the same way as if I was going to take it out for a walk.

7032. (Colonel Lockwood.) How did it know what you were going to do?—Because I did it day after day. I made the fistula and the fistula healed up. Then I had a cannula in the stomach and corked it up so that I could look in.

7033. I confess that I am rather aghast. If I understand you rightly—I do not want to do you injustice—the dog showed great delight when you were about to perform an operation on its stomach?—No, after the operation had been done and the thing was healed up, when I wanted to examine it it seemed delighted.

7034. Because it afforded it relief, I suppose?—No, it was because it liked to be made much of and petted and shown round.

7035. (Sir Mackenzie Chalmers.) It liked to be taken notice of?—Yes.

7036. And there was no pain?—There was no pain. Practically, one knows there is no pain afterwards. For example, just two days ago a celebrated surgeon did an operation upon a patient whom I had seen because the patient was then suffering from cancer of the œsophagus and could not eat. The patient was almost dead when he came to the surgeon, who then injected into his veins a quantity of saline solution. He was brought round sufficiently far to do the operation, and then, having got it done, he poured into his stomach a lot of nutriment. The patient is now very comfortable and has no pain after the operation. I have no doubt, if you are at all doubtful about it, that I can get a certificate from the patient.

7037. I will take it from you, of course.

7038. (Sir William Church.) As a matter of fact, in men, is the opening of the stomach a very painful operation?—No.

7039. In fact the actual opening of the stomach is usually done without any anæsthetic at all being administered. The attachment of the stomach to the wall is done under anæsthetics?—Yes.

7040. And after the stomach has been opened the patient seldom or never experiences pain in the passage of the tube into it for food?—That is so.

7041. You answered some questions about myxœdema, but it is stated now by anti-vivisectionists that iodine used to be given before any experiments were made on the thyroid, and that it was a cure for the condition of myxœdema or cretinism, or, at least, that it has acted beneficially. I should like to know your opinion upon that?—It is quite possible that it might act beneficially, but it certainly was not a cure.

7042. And now the same effects that were obtained by feeding with thyroid gland can be got by a substance which is called thyroïdin, which is, of course, obtained from the gland. Do you know anything about it; or would you rather not say anything about it?—I do not quite see how that bears upon the ques-

tion one way or another; because the use of thyroïdin is as much derived from experiments upon animals as the use of the thyroid gland itself.

7043. It is really not a true distinction; it is a mere play upon words?—Simply a play upon words.

7044. Have you turned your attention at all to the value of experiments on animals in connection with preventive medicine?—No, that is such a very large subject.

7045. I meant, rather as a means of diagnosis, have you turned your attention to that. We have been told here by an authority that the surest and the best method of diagnosing the presence of certain diseases that may be imported at our ports is by inoculation experiments in plague, cholera, and such like?—I cannot give you any definite information about that. It has not come in my way.

7046. Do the answers that you gave to Mr. Ram cover all that you would like to say with regard to the restrictions that the Act imposes upon experimentation upon animals, or would you like to add anything more? I gather from your answers to him that you are in favour of allowing persons who hold a licence to experiment where they like?—Yes.

7047. And, on the other hand, that persons who did not hold a licence might experiment in licensed laboratories?—Yes.

7048. But I suppose you are in favour of the present law, that no one should do experiments upon animals excepting the holder of a licence or in a licensed place?—Surely.

7049. Do you think there are no restrictions further that you would like to see removed? Are you satisfied with the present licences and certificates?—Perhaps, in regard to that, I am rather at a disadvantage, inasmuch as I have not been able to work under licence or certificate for some time, so that those in more active work than I am could probably give a better answer. So far as I am concerned, I think the licence and certificates are all very well. I do not know that I should advise very much alteration except in the way I have mentioned.

7050. You do not think that the having to get special certificates such as are now required interferes seriously with the work in scientific laboratories?—To some extent it does, as I found in my own case when, some two years ago, or rather more, I gave a course of lectures on the therapeutics of the circulation at the London University. I did not allow sufficiently long a time for obtaining a licence and a certificate to show experiments under anæsthetics. The consequence was that when I began to give my lectures I found that I had got over a considerable extent before I got any licence or certificate, and the result of that was that I was unable to go on in the regular sequence. That disturbed me very considerably. The delay in that case was of very considerable importance, and disturbed very considerably the lectures I should have given. If I had been able to get a certificate and licence at once, say within a few days, it would not have mattered, but, as it was, I was kept waiting a considerable time—I forget exactly how long; I think between a fortnight and three weeks—and it was very inconvenient.

7051. But still you would not press for such a point as this, for instance: that a person holding a licence should be freed from the necessity of getting certificates for experimentation on certain classes of animals and for keeping animals alive after experimentation?—I think if you have got a licence as a recognised good worker it might be at once endorsed: "This licence carries with it the liberty to work upon such and such things." That would greatly free a man, for this reason, that every now and again, in every research, a question crops up, and you wish to get that question answered; if you have to wait to get a special licence before you can tackle the question the opportunity for answering it has sometimes gone; so that if, instead of requiring special licences on every occasion, the licence simply carried with it, "This licence allows the operator to do so and so," at the very beginning, that would greatly lessen the hampering of research.

7052. (Colonel Lockwood.) How long do you suggest that licence should last?—I think a year.

7053. (Chairman.) Then you dispense altogether with the intervention of any medical body or authority who

has to approve the certificate as being for a proper reason?—It would not be necessary then.

7054. It would not be necessary, because you would have dispensed with it by the licence?—I take it that this certificate would not be granted without previous inquiry into the candidate's qualifications.

7055. Yes, but you make it simply a question of qualification, and having got a man who is qualified to hold a licence you would have him qualified to perform operations which now require a certificate—that is to bring the animal to life again without killing it?—Not necessarily. You might have a simple licence, and then if the experimenter thought that he foresaw the possibility of requiring something more, he might at once indicate beforehand the probability of his having to do certain experiments, which could be then submitted to the proper body, and with their sanction a certificate could be given.

7056. That is qualifying a good deal what you said before?—That is really what I mean. I did not think of a licence being granted simply to everyone, with the idea of his doing everything he pleased. I did not think that a licence carrying the power to do everything should be given to every applicant, but that when the applicant had a plan before him, involving the probability of various extensions, he should put down, as he is required to do now, I think, what he wants the licence for, and that in the event of his probably needing extensions, these could be at once granted beforehand to him in the licence, instead of his waiting until the need arose, and then applying for a special certificate.

7057. You would add to the present Act, without repealing the present provisions, a clause granting a special licence in a particular case, with special privileges as regards operations that might arise out of a particular leading operation?—Yes.

7058. (*Dr. Gaskell.*) You meant rather, did you not, that this special licence should only be given to well-tried men?—Surely, to trained men to be approved by somebody, as at present.

7059. (*Sir William Church.*) I only want to ask you one other question, and that is this: Do you think there is the slightest ground for saying that the witnessing, or the performing of experiments on living animals in any way affects the character of the spectator or the operator?—No, there are no grounds whatever for it. I may say that there are no more grounds than for believing that looking at operations on patients blunts the susceptibilities of man. My own belief is that, whereas surgeons who have been accustomed to see and to do surgical operations, have less of the hysterical element about them, and do not faint or become helpless at the sight of the suffering and pain of others; they have become on the contrary much more tender-hearted and much more willing to put themselves to a great deal of inconvenience, and to sacrifice themselves in order to save pain to others; and I believe that there is no more injury done to the moral faculties of the students who see experiments performed simply in order that they shall learn how to give relief to their patients than there is when they see the surgical operations done to give relief to their patients.

7060. (*Sir Mackenzie Chalmers.*) To follow that up, I suppose that when a man faints at seeing an operation, it is really because he is sorry for himself, and not because he is sorry for the patient?—Yes.

7061. And when he gets used to it, it ceases to affect his own nerves, but it does not affect his feeling for the patient?—That is so; that is very truly put.

7062. You are a Fellow of the Royal Society as well as a Fellow of the Royal College of Physicians?—I am.

7063. How long have you been a Fellow of the Royal College of Physicians?—About 30 years, and I have been a Fellow of the Royal Society, I think, for 33 years.

7064. In this inquiry we are anxious to get evidence from both sides. Can you give us the name of any Fellow of the College who could give us evidence against the view that you have been taking to-day?—No, I do not know of any; of course, there might be such.

7065. But you do not know any one whom you could suggest to us?—No.

7066. Now I want to go more into general questions. There is a great deal of feeling, naturally, about dogs.

If experiments on dogs were prohibited would medical science suffer?—Yes.

7067. You think they are essential?—It is essential that dogs should be used.

7068. Have you read Professor Starling's evidence on that point?—I have read it, but I have not looked at it recently.

7069. However, speaking from your own experience, do you think that it would be possible to dispense with them?—No, I do not.

7070. Would you give us your reasons?—Because other animals are too small. For many of the experiments guinea-pigs, rabbits, and cats are too small, and other animals of the same size, such as sheep, goats, and pigs are too expensive.

7071. Is there any importance in the fact that the dog is carnivorous?—I think there is. I think that in experiments, for example, on the composition of chyle or digestive experiments they are very important.

7072. We have had some evidence given by two or three witnesses who are against experiments on living animals, to the effect that an animal differs so materially from a human being that only false deductions can be drawn from experiments on animals; do you agree with that view?—That is quite untrue.

7073. There are differences, of course?—Yes.

7074. But allowing for those differences, can an experimenter get trustworthy results?—Yes. If I may give an example, if you give nitrate of amyl to a man it quickens his pulse very greatly; if you give it to a rabbit it very often does not alter the pulse rate at all: the reason of that is that the pulse rate in a rabbit is very quick to start with. If you give nitrite of amyl to a dog it has the same action as upon man—it quickens the pulse rate; but if before you give it to a dog you cut the vagi in the neck so that the pulse rate becomes as quick as it is in the rabbit it does not quicken it any more than it does in the rabbit.

7075. The engine is running at its maximum speed?—That is just it, but when you get those two experiments together you can at once see what is the action of nitrite of amyl.

7076. (*Dr. Gaskell.*) You would not draw any limit as to how low you might go down in the animal kingdom and still get advantage?—No, I do not know where you could draw the limit, because we are now going lower and lower in the animal scale and still get an advantage.

7077. (*Sir Mackenzie Chalmers.*) You yourself have not used curare since 1875, I understand?—To tell the truth I do not recollect when I last used curare.

7078. Would any detriment to science result if curare was prohibited, or if further restrictions were put upon its use?—I do not know. For my own part I should not be inclined to allow the use of curare as an anæsthetic.

7079. The Act prohibits that?—I think that is quite right, but if an animal is anæsthetised I do not see why you should not use curare. It will not increase any pain—it will do the animal no harm, and it might possibly be of service in keeping the animal quiet. There are certain cases which one can conceive where the employment of curare might be very useful. For example, if you wish to find out the effect of any stimulation of a sensory nerve upon the circulation, apart from any movement induced by a motor nerve, you might find the employment of curare advantageous, so that I think I should certainly not prohibit its use.

7080. (*Dr. Gaskell.*) Would it not also be of use in the case of peripheral stimulation of nerves to find out the question of circulation of blood in the muscles?—Yes.

7081. That could not be done without curare?—No.

7082. (*Sir Mackenzie Chalmers.*) Taking your evidence generally, am I correct in saying that you think that the continuation of experiments on living animals is indissolubly bound up with advance in medical science?—Yes.

7083. I take it broadly that the distinction between observation and experiment is that in the case of an experiment you deal with conditions artificially produced, and in the case of observation you have to wait until Nature or some external cause produces those conditions?—Yes.

7084. And you may have to wait an indefinite time?—Yes.

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7085. And you cannot isolate the factors?—That is the chief difficulty.

7086. For instance, in the testing of drugs by observation, you only get an empirical result?—Yes.

7087. And by experiment you get a scientific result?—Yes.

7088. You get at the cause, and you can isolate the various conditions one by one?—Yes, that is so.

7089. Now, coming to particular cases, two witnesses have referred to your own experiments, which I may call baking and freezing experiments, and you have explained the baking experiments. May I take it that the temperature to which the animal was exposed was in no case higher than that to which an ordinary human being goes in a Turkish bath?—Certainly, in no case.

7090. So that if the animal had not been anaesthetised no more pain or discomfort would have been experienced by that animal than if it had been put into a Turkish bath?—Surely.

7091. (Dr. Gaskell.) So far as temperature goes?—Yes.

7092. (Sir Mackenzie Chalmers.) I am speaking only of temperature. You did not give us any evidence about what one of the witnesses before us called the freezing experiments. I suppose you have tried the effect of reducing the temperature?—Yes.

7093. Do you happen to know to what extent the temperature was reduced?—In one of the experiments on the action of aconite, the pigeon was shivering as if it was very cold, but its temperature was 103.1.

7094. What was the temperature of the air to which it was exposed?—The temperature of the air was 100 Fahrenheit.

7095. That is practically ordinary blood temperature?—Yes; but then it was low for a pigeon.

7096. Did you in any of these experiments bring down the temperature below freezing?—I do not think so, so far as I can recollect.

7097. May I put it in this way: As regards low temperature, so far as you remember, did you expose any animal to any temperature to which it would not be subject in its wild state on a winter's night?—Certainly not, and then, of course, every precaution was taken that there should be no local irritation, either from excess of heat or from excess of cold applied, for example from a portion of the cold metal touching the skin. The boxes were always padded, either with cotton wool or with paper, so as to prevent any local irritation.

7098. Have you followed lately the experiments on snake poisoning with Calmette and Fraser's sera?—Yes.

7099. Have any improvements been made lately?—I do not think so. I do not think that Calmette has made any further improvements.

7100. They were tested some years ago by Dr. Douglas Cunningham, I think?—I think they were, but I had forgotten it.

7102. You have not followed out that particular branch lately?—No.

7102. How many years is it since you first made experiments on animals?—I first experimented in 1865.

7103. And you have practically held a licence up to quite recent years, have you not?—No, my work has been very much interrupted. Practically I have not done a great deal of work myself since about 1877, because at that time I found that my practice was getting in the way, and the only plan of doing experiments was to get an assistant whom I paid, who should carry out the experiments which I devised.

7104. But still you have been connected with people who have been experimenting on living animals for many years?—Yes.

7105. Have you ever known of any case of inhumanity or want of consideration for causeless pain; I am speaking of England, of course?—No; in England certainly not, and I do not remember any case abroad, because, as I said before, most of my experiments were done in Professor Ludwig's laboratory in Leipzig, and he was always most careful of the feelings of the animals that were experimented upon.

7106. In those that he saw?—He would not allow anything that approached to cruelty in his laboratory. So careful was he, that whenever a frog was killed by cutting off the head, he would not allow the student to throw away the head; he said, you must destroy the brain, because you do not know whether that brain may not have the power of sensation still.

7107. Have you seen a very convenient presentation of the case against vivisection drawn up by Surgeon-General Sir James Thornton?—No, I do not think I have seen it.

7108. He has collected his statements in alphabetical order?—No, I have not seen it.

7109. And on some of these statements you yourself have given evidence. May I put to you some of his statements and ask you for your comments as to how far you agree with them or not?—Surely.

7110. Speaking of abdominal surgery, he says: "Abdominal surgery has been seriously retarded by experiments on living animals. The following statement was made by Sir Frederick Treves in his 'Address on Some Rudiments of Intestinal Surgery,' delivered before the Midland Medical Society at Birmingham, and reported in the 'British Medical Journal,' November 5th, 1898:—'Many years ago I carried out on the Continent sundry operations upon the intestines of dogs, but such are the differences between the human and canine bowel, that when I came to operate upon man I found I was much hampered by my new experience, that I had everything to unlearn, and that my experiments had done little but unfit me to deal with the human intestine.'" How far do you agree with that statement?—Sir Frederick Treves has, of course, more knowledge of his own condition than anyone else; but I believe that he has since modified his statements so as to show that the impression that it appears to have conveyed to Surgeon-General Thornton is incorrect.

7111. Apart from Sir Frederick Treves' opinion, what is your opinion?—My opinion is definitely that but for experiments on animals abdominal surgery would not now exist.

7112. The next thing he discusses is anthrax. I do not know whether you have paid special attention to that or not?—No, not special attention.

7113. I will only take those heads which I think seem to refer to yourself. Another head is the Calabar bean. He says there: "The most conflicting testimony is given by different physiologists as to the action of Calabar bean upon men and animals, and it appears that no positive conclusion can be reached as to its action upon the vagi nerves. Dr. Harley declares that it does not affect the arteries when applied locally. Dr. Fraser contradicts this, and says he has demonstrated that the local application of the drug produces dilation of the arteries." That is brought forward to show that experiments on Calabar bean have been futile. What is your comment upon that?—Practically, Calabar bean is not used for its action upon the vessels, but it is used for its action upon the eye, and that was discovered first of all by Fraser in his experiments upon animals.

7114. This passage therefore, you think, is a little irrelevant to the question whether experiments on living animals with Calabar bean have been useful or not?—I think so. With regard to the vagi, that is a question of dose. With a small dose you will get a stimulation of the vagi; but if you give an enormous dose you get paralysis.

7115. You get different results, I suppose, even if you take common salt?—Yes, with a little common salt and water you get an appetite; with a lot of salt and water you get the exact opposite.

7116. Then he goes on to chloral hydrate. He says: "Experimenters contradict each other about the physiological action of this drug in the most bewildering manner. Chloral hydrate has not sustained in the case of human beings the conclusion derived from its use upon animals; indeed, it is a question whether it has not proved an evil rather than a benefit in treating disease. Of its injurious effects on the human mind there is now no doubt, though it was at first recommended expressly on the ground that it had no such evil mental results. The error naturally arose from experiments being made with it on animals with no mental constitution analogous to that of man." What is your comment upon that?—There, again, it is a

question of dose and a question of amount. What was made out in animals was the question of the soporific action, and, to a certain extent, an analgesic action upon animals. Both of these are applicable to human beings. The effect upon the mental power of human beings is that if you give chloral hydrate in proper doses it will produce quiet sleep and comfortable sleep, followed by no bad results, for a long time together; but when people get into the habit of drugging themselves with chloral hydrate they may bring on mental aberration, and I have even seen a patient who was confined in a lunatic asylum because no one knew she was in the habit of drugging herself with chloral hydrate until she was confined, when the whole thing came out, and then she became cured. But there has been no fallacy introduced at all by experiments upon animals. The experiments upon animals were restricted to finding out certain points about chloral hydrate which were definitely ascertained by the experiments on these living animals. You cannot give an animal chloral hydrate for months and months together to find out the effects upon its mental condition.

7117. The next subject that Sir James Thornton goes on to deal with is chloroform. I think you have made experiments on chloroform yourself in Hyderabad and elsewhere?—Yes, very many.

7118-9. After pointing out that chloroform was not discovered by reason of experiments on animals, Sir James Thornton proceeds to say: "No experiments were ever made in connection with the discovery of the anæsthetic properties of chloroform." Do you agree with that statement?—So far, it is right in that the anæsthetic action of chloroform was discovered by experimentation upon man, unless the story be true that it was accidentally discovered in the voluntary experiment of a dog upon itself. The story is—I do not know whether it is true or not—that a chemist had got hold of some of this new stuff, which he put in a little jar in a corner of his shop; and one day a lady came in with a little lapdog. The little lapdog went smelling all round, and smelt at this jar, when it fell over insensible; and that was what started Professor Simpson on his experiment. But the definite experiments in regard to its anæsthetic action were really made by Professor Simpson upon himself and on Dr. Matthews Duncan and one or two others.

7120. But, as regards the safest way of keeping it and the best way of administering it, has that been investigated by experiments upon animals?—It has been investigated by experiments on animals, but a good deal of it has been made out upon man. The number of experiments upon man, as compared with the number of experiments upon animals, is simply enormous.

7121. (*Colonel Lockwood.*) Then Sir James Thornton is right in that?—Yes; I think he is so far right. I should not contradict that.

7122. (*Sir Mackenzie Chalmers.*) Then he says generally, under the heading of "Cruelty of Vivisection": "It is unnecessary to enlarge upon this point, as the cruelty of experiments on living animals is self-evident, and is admitted by most of the experimenters themselves." What is your comment upon that?—It is not true; it is not admitted. The cruelty of experiments upon living animals is not great, and it is not admitted. There is very little pain inflicted in experiments upon living animals, and I should say that there is no cruelty at all—that any pain that may be inflicted is either inadvertently inflicted, and there is very little, if any, of it; or it is because they think that by, possibly, putting an animal to a certain amount of inconvenience (I would hardly say pain), they may save pain to their patients.

7123. The next point he deals with is diabetes. Is that a question you deal with—I will read you what he says upon it—or would you rather not express an opinion?—I am quite willing to answer any question about it.

7124. "It has been claimed," he says, "that the experiments of Claude Bernard, Schiff, Pavy, and others have thrown much light upon this disease, but, however interesting from a scientific point of view, these investigations have not enabled physicians to cure the disease. As Dr. Althaus observes, glycosuria which is experimentally induced is not diabetes such as we observe it clinically, but only a passing physiological storm, which is certain to disappear in a short time,

and which, moreover, reproduces only one of the symptoms of diabetes. The experiments referred to are therefore of no value as regards the cure of this disease." What is your comment upon that?—It is simply this—that the experiments of Claude Bernard and others have taught us a great deal about the nature of diabetes; that they have not taught us all yet, and because we have not learnt everything it is simply useless to decry the utility of the things we have learnt. It seems to me that in any physiological or vital problem we deal with our materials as a man does with a bridge. A man goes on building the piers of a bridge on both sides for a length of time, a couple of years perhaps; there is no sort of good. Nobody can pass across the stream or the valley until at length they are so far completed that the keystone is put in, and then over the whole thing they go. But unless the piers are built first you will never get the keystone in. With diabetes we have been getting the piers built up, and before long I think we shall get the keystone in; but I am quite willing to grant that we do not know anything like the whole pathology of diabetes yet.

7125. As the result of experiments, can you alleviate diabetes in any way?—Yes, very greatly indeed; because before Bernard's experiments we did not in the least know that the amount of sugar that was passing was due to the conversion of starch into sugar, whereas now we know that, and we limit the diet of the diabetic to proteid foods, cutting off the starches and the sugar as far as possible, and giving them only proteid and fatty foods. In this way we can lessen his sufferings and can prolong his life.

7126. The patient remains in a chronic state, instead of the disease becoming acute; is that it?—Not always. There are many cases where there is sugar in the urine and where, if the sugar were allowed to remain, the patient might perhaps pass on to a condition of true diabetes. These cases we now term glycosuria only, and by simply putting them upon a proper dietary for a length of time we stop the sugar. We can then gradually give them back the farinaceous food, and even the sugar, without any sugar appearing in the urine; in fact, we cure them permanently, although if the disease were allowed to go on it would probably have turned into chronic diabetes.

7127. His next statement relates to digestion, and Sir James says: "The most valuable contributions in this direction are those of Dr. William Roberts, of Manchester, and the experiments necessary for them were made in laboratory vessels, not on animals at all. In fact, all experiments upon digestion can be made outside the living body with greater accuracy and ease than in it." What is your comment on that?—My comment on that is that we can experiment with the juices (pepsin, pancreatin, and so on) outside the body, but until we had learned by experiments upon living animals the relationship of these with different organs, we could not have carried on these experiments, which can now be carried on in the laboratory—that all the foundation has been laid by experiments on living animals, and now other experiments can be made without any further use of animals.

7128. He then goes on to deal with diphtheria and antitoxins. I do not know whether you have any special opinion on that. We have had evidence from Sir Douglas Powell about it?—My own experience is that the diphtheria antitoxin is an extraordinarily valuable thing, that it practically robs diphtheria of much of its terror. Before, diphtheria was a frightful disease. You got a sudden call to see a child affected by it, but you could do little or nothing for it, and the child died; whereas, with the antitoxin you simply find that the membrane sometimes will melt away like butter.

7129. At any rate it is a powerful remedy, not a cure?—It is a powerful remedy, and a most useful one.

7130. Referring to the antitoxin Sir James Thornton says: "It may cause paralysis, kidney disease, heart mischief, and other disorders, and in some instances, when used on healthy persons as a prophylactic, it has caused sudden death like a dose of deadly poison." What is your comment upon that?—I should think in relation to the alteration in the circulation, to which he refers, as well as the paralysis are much more likely due to the diphtheria disease than to the antitoxin—to the toxin, and not the antitoxin. So far as regards these cases of sudden death, I do not know anything about them; but what one does know is that

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many people are very liable to faint from very comparatively slight operations.

7131. Like vaccination?—Such as vaccination. I was told that on board the troop boats taking young, healthy officers out to South Africa, the number of men who fainted on simply being inoculated for enteric fever was extraordinary, and sometimes in certain peculiarly nervous people, instead of getting simple syncope they get fatal syncope.

7132. You would attribute the deaths then rather to what I may call sudden syncope than to the toxic action of the serum?—Yes.

7133. But you do not know the cases to which he refers?—No, but it is quite evident that it was not due to the serum, because the toxin does not act instantaneously like that.

7134. You think that that observation of his is unfounded, or made under a misapprehension?—People may have died from the disease, but it is not from the serum.

7135. You disagree with his statement that "when used on healthy persons as a prophylactic, it has caused sudden death, like a dose of deadly poison"?—Yes, it may be so, but I should say that the operation of inoculation has caused it, but not the serum.

7136. Then the next heading he takes is drugs generally, and Sir James says: "It has been asserted that 'nearly all the facts definitely known regarding the action of remedies have been gained by experiments on animals.' A direct contradiction is the best answer to this preposterous claim, experiments with drugs and animals having not only been futile, but also misleading. Dr. Rutherford was misled by his experiments on animals so far as to declare, contrary to universal experience, that calomel diminished the secretion of bile. The most contradictory opinions have been put forward as to the action of drugs on animals, showing that the experiments were quite useless, and only calculated to cause error and confusion"?—Well, if a man says that the moon is made of green cheese it is very hard to convince him that it is not, if a man makes a statement like that.

7137. You would say that that is too general a statement for you to deal with?—Yes, it is too general a statement to deal with. That is a statement made in pure absolute ignorance.

7138. Then I will pass over various cases which I think more or less surgical, and his next heading is myxedema. He says: "It has been stated that a cure for this disease was discovered through vivisection; but this is not the case. Dr. Murray, of Newcastle, discovered, though not by vivisection, that an injection of the extract of the thyroid gland of a sheep into myxedemic patients would improve their health and avert death, but the treatment must be persisted in regularly, or relapses occur." Do you agree with that or did Dr. Murray experiment on living animals in fact?—It is said that a partial truth is worse than a lie. Sir James Thornton is perfectly correct about Dr. Murray, but he should go a step or two further back, and ask how Dr. Murray came to use this treatment. If he does that he will find that the whole treatment that Dr. Murray adopted was founded upon experiments on animals.

7139. He then speaks of nitrite of amyl. I think you have also mentioned that?—Yes.

7140. He says: "The claim that nitrite of amyl was discovered and brought into use through vivisection is quite unfounded. This substance was discovered in 1844 by the French chemist Balard, and was introduced to the medical profession in 1865 by the late Sir B. W. Richardson for the relief of angina pectoris, the action and properties of the drug having been by that time well ascertained. Some years later certain experiments on animals were performed by Mr. Gamgee with this drug, but nothing was discovered that was not known before." What is your comment upon that passage?—First of all he has got his dates wrong. It was not introduced as a remedy for angina pectoris by Sir B. W. Richardson in 1865. It was recommended by him in 1864 for syncope and for tetanus, but he never did come to discover its use in angina pectoris. The experiments upon animals performed by Gamgee were performed in 1866, and showed the action of the drug in lowering the blood pressure, a fact which had not been ascertained before; and it was this fact which led to the use of the drug in angina pectoris.

7141. As you have explained to us in an answer in a previous part of your evidence in the case of a St. Bartholomew's patient?—Yes.

7142. There is another remedy, is there not—nitroglycerin?—Yes. I made the physiological experiments with that in 1876, and I published them in the St. Bartholomew's Hospital Reports. I am unfortunately very liable to headache, and when I experimented with it it gave me such awful headaches that I hesitated to test it upon my patients, because I was afraid of giving them headaches such as I had got but in the meantime a few months after I had done my experiments Dr. Murrell used it.

7143. Your experiments were on animals?—Yes. He introduced it, and it has proved perfectly satisfactory. Mr. Tate and I showed that the action was just the same as that of nitrite of amyl.

7144. But you give it in a different way, do you not, in which it is more convenient to give it?—Yes. It acts rather more slowly, and its action is more prolonged.

7145. How is it given?—It is usually given in little tablets mixed with chocolate.

7146. I need not trouble you perhaps with the details, as you have answered generally?—Here it is (*handing in a book*).

7147. Then Sir James goes on, I think, to deal with Pasteur's antirabic inoculations, and he contests the value of those inoculations. Have you any practical experience of the Pasteur treatment?—Yes. I was a member of the Commission sent across to investigate the utility of Pasteur's treatment; I went over perfectly convinced that Pasteur was wrong.

7148. What was the date of that Commission?—About 1884 or 1885, but I came away perfectly convinced that Pasteur was right, and there is no doubt whatever, I think, in the mind of any unprejudiced person that Pasteur's treatment has produced a complete revolution in the treatment of hydrophobia. Hydrophobia was a most frightful disease: Just before I went over I was a member of a Committee of the British Medical Association to investigate hydrophobia, and I went throughout all the country examining cases; and the cases I saw were simply heart-breaking. As the result of it I got inoculated, and had to burn it out.

7149. Your own finger got inoculated?—Yes, unfortunately, with rabid poison when making a post-mortem examination.

7150. What treatment did you pursue yourself?—Caustic potash; there was nothing else. I took a stick of caustic potash and rubbed it in, and scraped it out and rubbed it in again.

7151. Did you go through Pasteur's treatment?—No; it was before Pasteur's treatment; that was the only treatment available at the time, but it does not improve the fingers.

7152. You satisfied yourself on that Commission that Pasteur's treatment saves life?—Absolutely.

7153. Have you followed recently the results of the Pasteur treatment in the new laboratory at Kasauli in India?—No.

7154. A laboratory has been established there to enable Pasteur's treatment to be applied in India, where there is a great deal of rabies?—Yes.

7155. You cannot give us any information upon that?—No.

7156. Who could?—That laboratory is under Hankin, is it not?

7157. I do not know whom it is under. I did not know where we could get any information?—You could easily get it from the India Office. Sir Benjamin Franklin could possibly provide you with all you want.

7158. Then Sir James Thornton speaks very strongly in favour of Buisson's treatment by baths; was that in any way investigated by your Commission?—No, we did not deal with that.

7159. Have you any opinion about it?—It is a very old treatment, but, like other old treatments, it was quite unsatisfactory.

7160. You think experience has proved it to be unsatisfactory?—Yes. It was tried, of course, without any definite results.

7161. Has it been tried with reference to animals as well as man?—I do not think so, but I cannot be sure.

But you could get information about it, I daresay, from Dr. C. J. Martin, of the Lister Institute.

7162. Then the next heading is Poison Tests, and Sir James says: "Poisons act in such different ways on men and animals that deductions from experiments on the latter are fallacious and misleading. Rabbits can eat belladonna with impunity; dogs, aloes; apes, strychnin; and goats, hemlock; while birds in general are little affected by morphia. Even in human beings the effects of certain poisons are greatly modified by race. All this tends to show how vain a thing it is to expect to find out remedies for our own diseases by experiments upon animals, which are not constituted as we are, and which frequently find their food in things which would be fatal to mankind." How far do you agree with that general statement?—A number of the instances are quite correct. The general conclusion is quite erroneous. For example, rabbits eat belladonna with impunity, just in the same way (as I mentioned before) as, if you cut the vagi in a rabbit, you find no change in the pulse rate. If you cut the vagi in a dog you see its pulse rate going up at once. If you give a dog belladonna, at once you will get the pulse rate go up too; the reason being that in a rabbit the vagi act ordinarily hardly at all, whereas in the dog they are constantly acting. When you paralyse the vagi in a rabbit with belladonna there is no difference. If you paralyse the vagi in a dog you get a great difference at once, just as you get a difference at once in a dog's pulse rate when you cut its vagi. When he says that birds in general are little affected by morphia, they are very much affected by morphia, but in a different way. For example, pigeons are not much affected apparently by morphia in the way of its causing drowsiness, but it acts upon them as an antipyretic—it lowers their temperature as much as if you put them in a very cold place—so that they are affected, though in a somewhat different way.

7163. But the point of Sir James Thornton's criticism is that these substances act so differently on animals and man that you can draw no true inference from one to the other. What is your comment upon that?—The same may be said of the action of the drugs upon different men. A dose of aloes, which he says may be swallowed by dogs with impunity, may be swallowed by certain men with perfect impunity; and yet we know that in the great majority of human beings aloes will act as a purgative, notwithstanding the failure to do so in those individuals. The same with opium. Sir Robert Christison used to tell us of a particular family who were not affected by opium. Sir William Hamilton, the Scottish metaphysician, would swallow an ounce of laudanum with perfect impunity—it had no action at all upon him; but it was not that he was an opium-eater or accustomed to it.

7164. It was a pure idiosyncrasy?—Yes; and it was inherited by his eldest son. But still the majority of people are affected by laudanum; so that what holds good in all he says in regard to animals applies in the same degree to man: that we can only tell from observation on man the effect of drugs upon the majority; that we will find exceptions just as we find them in animals.

7165. And by experiments is it possible to make the necessary allowance?—Yes.

7166. (*Chairman.*) I do not quite see how that is in the instance you gave. If pigeons generally are made cold by taking morphia, and it has not that effect on humanity, does not that show that you cannot experiment on pigeons with regard to the effect of morphia as an anæsthetic or soporific?—No, because you see I have dealt with that very question in this paper on the effect of heat and cold, upon the action of aconite on pigeons.

7167. I am assuming that what you say about it is quite correct as a fact. But after knowing what the effect of it was on pigeons, you would not experiment on pigeons with morphia with a view to obtaining information for the treatment of man, would you?—Yes, certainly; and for this reason, that there are differences in men. There are certain people who are called gouty who tend to pass out their nitrogenous waste in the form of urea or uric acid. Other people tend to pass out their nitrogenous waste in the form of urea, and what Dr. Cash and I were very anxious to find out was whether we could trace any relationship between the action of morphin on pigeons, which, like other birds, passed their nitrogenous waste out in the form of urea or uric acid and gouty people; because

there are certain classes of people on whom morphia or opium does not act as a satisfactory soporific; instead of causing sleep it causes excitement. We thought that very likely by experimenting on animals we should find out whether this bears any relationship to the mode of excretion of nitrogenous waste. We find, for example, that morphia will not act upon pigeons and other animals that excrete uric acid, and we thought perhaps that we might find by going to work at the bedside, that men who excrete uric acid would not be satisfactory subjects for morphin. We were not able to continue the experiments, but that was the line of them; and if they are continued on those lines I think we shall get very valuable information indeed in regard to the action of drugs on account of the very variations in their action upon different animals, which will enable us to isolate the different factors which determine the kind of action of a drug upon man, by finding out one factor or another on the different animals upon which we experiment.

7168. (*Sir Mackenzie Chalmers.*) The next heading is, perhaps, too wide a one to discuss at this time; it is "Preventive Inoculation," and that raises too wide a question to go into?—It is not one to which I have paid particular attention.

7169. Then Sir James Thornton discusses Snake Venom, and states that Sir Joseph Fayrer in 1904 pointed out the effect of permanganate of potash?—Yes, but it was before then; it was long before that. He mentioned it first of all in his book, "Thanatophidia of India."

7170. I presume it is, at any rate, from experiments on animals that you can test the proper mode of administering a drug like permanganate of potash?—Yes.

7171. You can only test the best way of dealing with snake bite by inoculating an animal efficiently, and then noting the different conditions under which the drug operates successfully or unsuccessfully?—Yes, that is so.

7172. There is an enormous loss of animal life as well as of human life in India through snake bite, as you know?—I believe they are beginning to use those little lancets for animals as well.

7173. That is an improvement that has been made in recent years as the mode of administering the antidote?—Yes.

7174. Then he takes the heading "Rheumatism," and he says: "Lactic acid is found in the bodies of rheumatic patients, and its injection into animals produces rheumatic symptoms; but this information has not resulted in the discovery of any means of preventing or curing the disease." That I imagine is true?—I think that is true.

7175. Have you anything to say for those experiments? Are they likely to be useful or useless?—I do not know that they are any use as yet, although one does not know what the result may be. As I said before, in all those things we have to accumulate a good deal of information before we get any practical application.

7176. I have only one more point to put to you. Under the heading "Temperature" Sir James Thornton says: "No alteration in the treatment of fever has resulted from the experiments of Claude Bernard on raising the temperature of animals till death occurred. These experiments therefore were totally useless." What is your comment upon that?—That is not so. I think perhaps the best comment I can make, if you will allow me, is to give you a case that I once saw. When I was a student, a man with pneumonia came into the hospital in Edinburgh, and was dying. Professor Bennett, who was a clinical physician, was very much disgusted because this was going to spoil his average. He said that he had had over a hundred cases of acute pneumonia, and he had never once lost a case. However, this man was really dying; he was moribund, so Professor Bennett gave a grunt and passed on. A Swiss named Skolberg, who was visiting the clinique, asked what did Professor Bennett think about the man. "He is going to die." "May I treat him?" "Yes, you can do what you like with him." So Skolberg pulled off all the bedclothes; he got a sheet, dipped it in a tub of water, and wrapped it round the man, pulled it off again in five minutes, and then repeated the dose. I went away, but about half an hour or three-quarters of an hour afterwards I returned, expecting to find the bed empty. To my

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astonishment, I found the man lying there, apparently perfectly well and sleeping comfortably. That man was dying of hyperpyrexia; but that was just before the introduction of the clinical thermometer, and we did not know what he was dying of. It was a bit of pure empiricism. But the experiments on animal temperature now allow any student to know what is happening and to know the time when he is to apply the cold douche and so to save the lives, not of one, but of many. What was then a mere accident is now the routine practice.

7177. You have explained the reason by experiment?—Yes.

7178. And therefore you can apply the treatment intelligently rather than empirically?—Yes. Of course, the effect of the treatment is 1,800 years old. Musa, the physician of Augustus, saved the Emperor's life by cold baths, but, not knowing how to apply them, he killed the Emperor's nephew, the next patient he had to treat.

7179. (Dr. Gaskell.) I just want to ask you one question with reference to a question put to you by Sir William Church. We have been told that there is a difficulty about anæsthetising, dogs especially, because the anæsthetic is so liable to cause death, and that the margin between death and complete anæsthesia is so small that it is impossible to keep the animal under complete anæsthesia. Do you consider that dogs are more liable than other animals to have stoppage of the heart through vagus action in consequence of the administration of chloroform?—No, I do not think that they are.

7180. You do not think that dangerous symptoms come in in the case of dogs early in the anæsthesia?—No, I do not think so. I think that there is very little

danger with dogs, if you are watching what you are doing. All the time we were in Hyderabad the only fatalities we had were when somebody was not attending to his work. All the others went quite smoothly; we had no difficulty whatever.

7181. Is it not frequently the case that when you give chloroform to dogs you have what you speak of as vagus pulse?—Yes; but I do not think that that makes any difference. It does not kill the dog in the least. And you get it in man too.

7182. You do not think, then, that there is any more difficulty in keeping a dog under chloroform or that it is any more liable to death than in the case of cats or other animals?—Certainly not; and less than in the case of rabbits. I should put the dog upon very nearly the same plane as a man; I should say that the difficulties with a dog and man are just about the same. But with a rabbit, if you do not do it rightly, you do get death. There are two entirely different ways of chloroforming rabbits. One is to do it very gently, with a bell jar, and allowing the chloroform to increase very gradually until the animal becomes insensible. The other way is simply to cram the chloroform on to a cloth and stick it over the rabbit's nose and keep it there until it goes under. Either way is quite safe; but a combination of the two is unsafe. If you begin gently, and then cram it on, and then do it gently again, it does not do.

7183. You have had experience of chloroform given to a vast number of dogs?—Yes.

7184. And you have entirely convinced yourself that they have been under anæsthesia?—Yes, entirely.

7185. And complete anæsthesia?—Complete anæsthesia; as complete as any patient could possibly be under on the operating table.

SEVENTEENTH DAY.

Wednesday, 1st May 1907.

PRESENT :

The Right Hon. the Viscount SELBY (Chairman).

Col. the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MACFADYEAN, M.B.

Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (Secretary).

Miss L. LIND-AF-HAGEBY, called in; and Examined.

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7186. (Chairman.) You are a native of Sweden, I believe?—Yes.

7187. And you resided there and were educated there, until you came over to England for the purposes of attending lectures?—Yes; and I have been living in England a great deal. I was at school in England, but I have been living here since 1902—practically residing here.

7188. Before you left Sweden, I understand that you took a great interest in this question of vivisection?—I did, a very great interest since 1900.

7189. And you took an active part in the agitation against vivisection?—I have taken an active part in the agitation for some seven years.

7190. Since 1900, you mean?—Yes.

7191. Did you receive any medical education in Sweden?—No, I did not. The medical education I received I received here; but it was never our intention to take up the study of medicine—it was simply to study physiology.

7192. For the purpose of assisting your action on the question of vivisection?—Yes.

7193. You were a confirmed anti-vivisectionist when you came over to England?—We were.

7194. You and Miss Schartau were joint authors of a book called "Shambles of Science"?—Yes. Although we were confirmed anti-vivisectionists in a way, we were quite open to conviction, and were anxious to find out what vivisection really was and to get more knowledge. We found that we had not sufficient knowledge to fight against it as we were doing, and it was out of a desire to get more knowledge and get more thoroughly acquainted with it that we decided to take up the study of physiology.

7195. You had had no previous experience of physiology or of attending lectures?—No, but it was a visit to the Pasteur Institute in Paris that made us interested in the whole question; it was the things we saw there which made us really care for the question.

7196. The principal thing we wish to have your evidence upon is the cases that you refer to in your book, in which it is alleged or suggested that there were breaches of the law and cruelty; and perhaps it would be more convenient to take them just in the order in which you refer to them in the book?—May I be allowed to read a statement which I have prepared? It is very much in connection with the book and very much in connection with my whole point of view.

7197. I am afraid we should have things twice over

in that case. It would result in our hearing a good deal twice over, because it would be necessary to ask you as to the precise facts you there speak to on each of those experiments. I propose now to go through them, and if, when I have done, that there is anything you wish to add, you will have an opportunity?—In my statement I have prepared certain information in connection with those experiments which have been already brought before the Commission; but before that I have some evidence which to me it is absolutely necessary to give in connection with “Shambles of Science,” and in connection with statements that have been made already before the Commission; and I therefore shall be very grateful if I may be allowed to read it, as to me it is very closely connected.

7198. We find that when witnesses read statements they very often have not got present to their minds what the object of this Inquiry is so much as what their own views are, and we find that those statements very often go into matters which are not really relevant to our Inquiry?—I am afraid that my evidence will not be of the same use if I am not allowed to read my statement, as other witnesses have been allowed; for instance, Professor Starling, Mrs. Cook, and others have been allowed to make statements; and I have prepared my statement very carefully. All my statement is composed of corroborative facts which to me are absolutely essential if I am to present my defence of “Shambles of Science” from the point of view of the accusations which have been made; and I am afraid if I am not allowed to present it in this way, I must beg leave to withdraw as a witness. I do not feel justified, in the interests of the cause that I represent, in defending my book in a way that would not do justice to the cause.

7199. You must remember that it is not your book that is on trial here; it is the experiments that you speak to. You say that it is a statement of facts that you wish to read?—I wish specially to speak to those facts in immediate connection with my book; but also I understand, if the Commission wish to hear my observations, that I have the same right to speak on the general question as Mrs. Cook has done, and Dr. Arabella Kenealy, and other witnesses who have come from the anti-vivisection side.

7200. Yes; I am not disputing that you are at liberty to speak on every possible relevant question?—I do not intend to do that. I have limited myself to physiology. I should infinitely prefer to be allowed to read a statement, as other witnesses have done.

7201. You have sent us a *précis* of your evidence?—Yes. May I be allowed to say that I have endeavoured to give in my *précis* the very same points as I have in my statement.

7202. I was going to follow your *précis*, in which you begin by stating that you wish to give evidence especially in connection with your book “Shambles of Science” and passages contained therein, which the Commission have already dealt with, and the experiments on the so-called “brown dog” and other facts bearing upon the basis and progress of the anti-vivisection cause?—Yes, but if you read the next point in my *précis*, there are certain things I wish to bring out before I come to that.

7203. I was going to follow on with that?—I have a short statement on the ethical side of the question, and I thought I should be allowed to read that first.

7204. Very well; will you give us that?—The “Shambles of Science” was inspired by moral indignation, and written as a protest against the cruelties of vivisection, the callousness of vivisectioners, and the demoralising effect of vivisectional demonstrations before students. Before I proceed to lay before the Commission facts which corroborate the accusations made in the book, I wish to plead the priority and the ultimate persistence of the moral objection and revolt against experiments upon living animals. There can be no doubt that the vivisection of human beings would at present be of the utmost scientific value to the same section of scientists who claim the right to vivisection animals, but the majority of vivisectioners shrink from expressing such desires, though there are exceptions. We have advanced so far in our moral conceptions that a general clamour for human beings for vivisectional purposes would meet with disapproval and resentment. The question of utility or uselessness is here rightly deemed to be immaterial in comparison with the moral issue, and in the same way a further moral development will prohibit the vivisection of animals. The history of the evolution of morals affords

repeated proofs of such moral victories. The use of instruments of torture for the purpose of extracting especially coveted information has been abandoned in most civilised countries. Religious as well as scientific torture of human beings is condemned as immoral. There can be no doubt that, were the arguments of the vivisectioner to be considered a safe moral guidance in other walks of life, the artist and the sculptor might crucify in order to further realism in their art, and the writer of drama might order the infliction of pain and suffering on the helpless for a similar purpose. Few would deny that humanity is as much in need of art as of science, and yet divorced from morality they both become scourges instead of blessings. From the purely physical point of view it might be advantageous to kill off a few thousands of our incurable lunatics, imbecile children, incorrigible paupers, worst criminals. But we refrain from acting in accordance with such a view because we are restrained by a moral consciousness which was non-existent in our savage state. Moral evolution has forced us to include the sub-human races in the circle of our compassion and our sympathy. Kindness to animals, gradually passing into a conscious acknowledgment of their rights and our duties towards them, is the last and finest product of our developing altruism. From the moral point of view the elimination of the unfit means the elimination of the cruel and the selfish. In the light of progressive morals anti-vivisection is not an isolated fad or the expression of uncurbed sentiment—it is part of a great whole, of a humanitarianism which includes every sentient creature in its protective embrace, and which condemns every form of cruelty to man or beast. The abolition of vivisection, then, becomes an unavoidable moral necessity of the future; for the growth of sympathy and the demand for justice cannot permanently be stopped by any appeals to our cowardly fears or to purely egotistic considerations.

I now come to my evidence in immediate connection with the “Shambles of Science,” and everything I am going to say now is closely bound up with the book. I have brought some copies in case anybody would like them.

7205. Now you are going to speak with reference to the experiments which you saw and your description of them?—Yes. I am going to try to give my own experience of vivisectional demonstrations and vivisectional anaesthesia, as well as certain evidence in corroboration of that experience.

7206. Do you mean other experiments which are not mentioned in your book?—No, I shall not speak to any experiments except those I have seen—not generally, of course. I have some corroboration, of course. I am afraid I do not quite understand the question.

7207. I thought from what you said that you were proposing to give evidence of breaches of the law in experiments in operating rooms and laboratories on other occasions than those referred to in your book?—No.

7208. This surely would be more convenient to take by question and answer. We are now coming to the first of them?—Yes, I am now coming, if I am allowed, to what experiments are “absolutely necessary,” if I may read a very short statement. I have not taken it from the point of view of going through the whole book chapter by chapter; I should never think the Commission would have time to listen to it if I were to give lectures on each chapter; and I have simply taken certain facts.

7209. What we want to get at is the facts. When you speak of “painless experiments” you give what took place on a certain occasion in a lecture room with reference to a frog?—Yes.

7210. What really took place is what we want to know?—Yes.

7211. Because you know very well that some of the physiologists differ from you?—Yes, I do.

7212. And some who were present on the occasion differ from you and give different accounts?—Yes.

7213. Then in order to get at the facts we must pursue the ordinary method of doing it—that is, to ascertain what your statement of the precise facts is to which the other witnesses speak?—If the Commission would perhaps find it possible to follow the *précis*, the next point is that the “Shambles of Science” throws light upon the manner in which the provision of the Act of 1876, that only experiments which are “absolutely necessary for the due instruction of the persons to whom such lectures are given” is interpreted. That

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is a point which runs through the whole book rather than one single chapter.

7214. You are speaking now of experiments for instruction and demonstration?—Yes, and I am very anxious to bring out a point here, that point being that the Act of 1876 demands that only experiments which are absolutely necessary shall be performed in illustration of lectures; the principle underlying this restriction evidently being that too much vivisectional demonstration before students would have a demoralising effect—at least it must be some notion like that which underlies that Section of the Act. I wish to bring out now that our experience as recorded in the “Shambles of Science” shows that a great many experiments are performed before students which cannot be justified as being “absolutely necessary for the due instruction of the persons” (those are the words of the Act) “to whom such lectures are given, with a view to their acquiring physiological knowledge or knowledge which will be useful to them for saving or prolonging life or alleviating suffering.” As an instance, I have mentioned in my *précis* the injection of diseased spinal cord matter taken from a lunatic into a dog. I have mentioned that as an instance of an experiment that cannot very well be said to be absolutely necessary.

7215. That is on page 75 of the book?—That is the page.

7216. You want to prove from what you saw then that it was unnecessary?—Yes. I refer to that experiment especially as one that cannot be said to be one that was absolutely necessary; and my contention is that all the experiments are more or less unnecessary. May I proceed on this point?

7217. If you will tell us what you find in this chapter which supports your argument?—In this chapter my argument is supported by the fact that this experiment has no therapeutic value whatever; further, that it was not absolutely necessary to demonstrate it from the physiological point of view. I do not know whether I am to mention the names of the physiologists who performed these operations.

7218. Certainly?—I do not wish to mention their names with any desire to libel them. The Commission wish me to mention their names?

7219. Certainly?—I want then to bring out that Professor Halliburton, who conducted this demonstration, in his “Handbook of Physiology” refers to the test for cholin; and he says the cholin may be tested in two ways—by a purely chemical test and by a physiological test; but—these are not his words, but it is my opinion—as there are the two tests it was not absolutely necessary to choose the test of injecting it into a live animal; he might just as well have chosen the purely chemical test. Further, as I said, the experiment is of no therapeutic value whatever, and Professor Halliburton himself does not seem to assign great value to this experiment, for he says in his Handbook, on page 177: “It is possible that such tests may be of diagnostic value in the distinction between organic and so-called functional diseases of the nervous system.”

7220. Was Professor Halliburton one of those engaged in this operation?—Professor Halliburton was the lecturer upon that occasion; and my contention here would be that when the lecturer himself does not know or is very doubtful as to whether there is any diagnostic value, even diagnostic value, in such an experiment, that particular experiment should not have been one which is tried before students. Further, I wish to state that the choice of what is absolutely necessary seems to depend upon the individual taste of the physiologists who are lecturing and demonstrating. Thus Dr. Bayliss, during the Bayliss v. Coleridge trial in 1903, referred to his particular experiment upon which the action was based as one that was “absolutely necessary”; whilst Professor Gotch, of Oxford, during the trial said that that particular experiment was not included amongst those which he showed to his students. That shows, in my opinion, that the choice of what is “absolutely necessary” depends on the individual taste of each vivisector, and that one might ask why there is not a certain curriculum, a certain number of experiments which are agreed upon as being “absolutely necessary” and performed before students, but that there is unlimited freedom in the choice of that which is “absolutely necessary.”

7221. Are there any other of the experiments which you refer to in this book which bear upon that point

of demonstrations?—My point—from my point of view—of course I do not expect physiologists to share it—is that none of those experiments can really be said to have been absolutely necessary for the due instruction of students.

7222. You select this one as what you consider a strong instance?—I select it as a strong instance; that is all. May I now go on to my second point in connection with the book?

7223. Yes?—The descriptions given in the book in connection with the subject of vivisectional anaesthesia and the lack of humaneness on the part of lecturers and demonstrators who, whilst they are anxious to impart knowledge to their pupils of the theories of experimental physiology, neglect altogether to inculcate any ideas of mercy or carefulness in the treatment of the animals, are corroborated by the tone and instructions which characterise most modern textbooks and handbooks on physiology, theoretical and practical. Vivisectors have lately laid claim to humaneness in the execution of their pursuits, and have disdainfully repudiated connection with the old vivisectors, who were cruel, or foreign vivisectors, who may be cruel now. Thus there was a letter in the “Times” of March 12th, 1902, by Sir Victor Horsley, in which he referred to foreign vivisectors in these words. He was speaking of a representative of the anti-vivisection agitation, saying that he “confuses what may be done in Germany in the direction of operating on animals without anaesthetics with what is actually done in the United Kingdom under anaesthetics, and terms it all vivisection.” There are many other instances of similar statements, which I do not think I need read to the Commission. But the veracity of their claims is disproved by their own teachings. If everything were done to teach students the moral obligation of being merciful to the victims of scientific research these textbooks and handbooks would show some trace of it. They would contain special and serious warnings against the infliction of pain, and special and detailed instructions as to the care of animals from the humane point of view, both during the operative and the observational periods of experiment. The following well-known books contain no such warning or instructions. (I have brought all these books with me to-day.) “Directions for Class Work in Practical Physiology,” by E. A. Schäfer, 1901; “Exercises in Practical Physiology,” by Aug. D. Waller, Part II., 1905; “Essentials of Experimental Physiology, for the Use of Students,” by T. G. Brodie, 1899; “Practical Physiology,” by Beddard, Edkins, Hill, Macleod, and Pembrey, 1905; “Halliburton’s Handbook of Physiology,” 1905; “Elements of Human Physiology,” by E. H. Starling, 1905; “Outlines of Practical Physiology,” by William Stirling, 1902; “An Introduction to Physiology,” by L. A. Hodgkinson Lack, 1905; “Essentials of Human Physiology,” by D. Noël Paton, 1905; “Essentials of Physiology for Veterinary Students,” by D. Noël Paton, 1905; “A Manual of Physiology,” by G. N. Stewart, first edition 1895, fifth edition 1905; “Textbook of Physiology,” edited by E. A. Schäfer (for advanced students), 1898. The following books are by four American professors of Physiology, but have English or Anglo-American publishers, and are widely known and studied in England:—“Textbook of Physiology,” by W. S. Hall, 1900; “Handbook of Physiology for Students and Practitioners of Medicine,” by Austin Flint, 1905; “A Textbook of Physiology for Medical Students and Physicians,” by Will. S. Howell, 1905; “Laboratory Manual of Physiology,” by Fred C. Busch, 1906. I have not said that directions as to the administration of anaesthetics are absent from all these books, but I wish to point out that, although some are less callous than others, on the whole they lack all signs of any dominating wish to attempt the humane teaching and practice of experimental physiology. I now come to my next point, and that is that incomplete anaesthesia is frequently advised in these textbooks for students. One of the most important lessons to be drawn from a study of these textbooks is the utter inadequacy and unreliability of vivisectional anaesthesia as a means of abolishing pain. “The sleep of anaesthesia is a pathological condition due to the action of a poison,” says Professor Halliburton in his “Handbook of Physiology,” and experience of vivisection as it is taught and as it is carried out shows that experiments frequently require partial or perfect freedom from that condition.

7224. From what condition?—From the condition of anæsthesia. Instead of warning students against cruelty, some textbooks deliberately advise light anæsthesia, and warn their readers against employing anaesthetics in a manner which would bring about deep anæsthesia.

7225. What was Professor Halliburton speaking in reference to in that passage?—Professor Halliburton was there merely speaking as a scientist, and not with any special reference to demonstrations. I simply quoted his words as being an authority, and saying that the sleep of anæsthesia is a pathological condition due to the action of a poison; and I brought it in here to show the likelihood of physiological experiments being frustrated and injured by anæsthesia.

7226. (*Colonel Lockwood.*) Have you got Professor Halliburton's book?—I have it here.

7227. (*Chairman.*) Would you just read the passage again?—"The sleep of anæsthesia is a pathological condition due to the action of a poison."

7228. (*Sir Mackenzie Chalmers.*) If you can give us the page we can see for ourselves?—I am afraid I have forgotten the page.

7229. (*Chairman.*) You only read part?—The rest was my own.

7230. What followed a little surprised me?—It was badly read by me. I have said that these physiological books for students, as I am going to show, frequently require perfect or partial freedom from that condition—the condition which Professor Halliburton refers to as a pathological one.

7231. We should like to see the passages, if necessary?—I have got all the passages marked.

7232. I do not want them read, but you will give us the references so that they may be examined, because so much depends upon the context?—In support of this I beg to draw the attention of the Commission to Professor Brodie's book "Essentials of Experimental Physiology for the use of Students." Professor Brodie was lecturer on physiology at the London School of Medicine at the time of my attendance there.

7233. (*Sir William Church.*) The London School of Medicine for Women?—The London School of Medicine for Women. In this book Professor Brodie expresses his preference for light anæsthesia and curare in the following terms, at page 176, dealing with stimulation of the sciatic nerve: "Often, too, struggling movements are produced, especially if the animal be only lightly under the anæsthetic, when with each convulsion the blood pressure rises considerably. The only completely satisfactory method of obtaining a pure pressor effect is to previously curarise the animal"; and at page 202, dealing with the superior laryngeal, he says: "We may stimulate this nerve in two ways. We may imitate the normal method by introducing into the larynx a curved probe whose end has been wrapped in cotton wool. In this instance, and, indeed, for all these nerve stimulations, the rabbit must not be too deeply under the anæsthetic."

7234. (*Chairman.*) Do you understand that to mean that he is advocating that the rabbit should still be sensible to pain?—I do not mean to suggest that Professor Brodie is advocating that, but I mean to suggest that vivisectional anæsthesia is exceedingly unreliable, as the deeper stages of surgical anæsthesia are not compatible with physiological research. On page 206, dealing with stimulation of the central end of the vagus, he says, "This result is much better obtained if the anæsthesia be not too deep." Professor Stewart's "Manual of Physiology" shows the usual fear of too deep anæsthesia, though the last edition is better in this respect than the first. In the practical exercises attached to the various chapters which give full directions for vivisectional work, students are advised only to give ether in addition to morphia or chloral "when necessary"; and on page 156 of the first edition, dealing with stimulation of the depressor nerve in the rabbit, we read: "Anæsthetise a rabbit with $\frac{1}{2}$ gramme chloral hydrate, and, if necessary, with ether. In blood pressure experiments only small doses of chloral hydrate or chloroform can be given, as they affect the vasomotor centre." Dr. Waller gives a similar caution in his "Exercises in Practical Physiology" (Part II., pages 46 and 47).

7235. There, again, do you mean to suggest that in the doses he recommends he is recommending a dose which would not produce insensibility to pain?—I am not at the present moment dealing with doses.

7236. Well, quantities?—I am not at the present moment dealing with doses; in fact, I do not know that the doses are given in these cases.

7237. I am not speaking of this specific dose. But he says there should be a light anæsthesia?—Yes, I mean to suggest that all these (and there are one or two or more that I want to bring out on this point) imply the induction of an anæsthesia which from the humane point of view is unreliable because it is too light, and I am going to endeavour to prove by these facts that that is generally practised in physiological laboratories.

7238. That is the real point we know, whether it is generally practised; and this is really a little outside our inquiry, as to what doctors write in books and recommend. It is what is done that we are concerned with, because they may not be recommending it but saying that certain effects will not be produced under heavy anæsthesia?—I do not think it is outside your inquiry when in the "Shambles of Science" I have described what I have seen done. These are the theories which would be responsible for the doings I have seen. It is corroboration.

7239. We might have to go into the education of every doctor who performed an operation, at that rate. We are not so much concerned as to what doctors have written on the question, and what the effect of light or heavy anæsthesia would be upon animals, as upon what is done?—But if I were to come before the Commission and simply say I have seen this, and I have seen that, I have every reason to believe that the Commission naturally would not look upon me as an authority on the subject, but would probably take the opinions of physiologists, who hold entirely different opinions, as more valuable; but if, at the same time as I bring my own experience, I corroborate that experience by theories as they are put down in every current and modern text book, I consider that I strengthen my case considerably.

7240. (*Colonel Lockwood.*) You are arguing that the anæsthesia as recommended by the authorities you quote is so light as not to render the animal insensible to pain?—I mean that it is very possible that the animal may not be insensible to pain.

7241. (*Sir William Collins.*) Are you quoting from the handbooks of physiological laboratories?—I am quoting from handbooks all of which I have brought with me.

7242. (*Sir John MacFadyen.*) Would you admit that there is any point in the argument if the authors of these books tell us that when they said the anæsthesia was not to be too deep they never for a moment intended it to be so light that the animal was to be conscious?—I quite admit that the defenders of vivisection would say that, but my point is that vivisectional anæsthesia is unreliable, because the scientific requirements do not tally with the humane requirements, and because these scientists would always rather follow the scientific requirements than the humane ones.

7243. May I point out that the train of your argument really seems to involve a belief on your part that they intended to suggest that the anæsthesia should be so slight that the animals might be conscious; because it arose in connection with a question of cruelty?—My whole evidence is in connection with cruelty.

7244. So that you do wish us to believe that these authors deliberately intended to advise that in certain cases the anæsthesia should be so slight that the animal would probably be conscious?—I have never said that or suggested it. I have said that they advise light anæsthesia, and I have not suggested anything as to what their views are on the animal feeling pain or not; in fact, I think in most cases they hold no views on that subject when they write these books. But I mean to suggest from the anti-vivisectionist point of view that it is very likely that the animal is insufficiently and improperly anæsthetised, and therefore we anti-vivisectionists have a very strong reason to complain.

7245. (*Colonel Lockwood.*) Why do you say it is "very likely"?—Because I know, and I think everybody knows something, of the difficulties of anæsthetising generally, what a science it is, what an art it is; and from my experience of vivisectional anæsthesia I know how that is practised, too. And if the dominating idea is to have a light anæsthesia, the dominating idea cannot be to have profound anæsthesia; and we know that anæsthesia is always fluctuating between deep and

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light, and that therefore the tendency, the weight of balance would be towards the light, towards the return of consciousness, rather than towards the total and permanent abolition of consciousness.

7246. (*Chairman.*) It all depends upon whether those are really instructions which would be read by any medical student as instructions for the purpose of allowing animals to be too lightly anaesthetised, so that they still should feel pain—whether that is the intention?—May I read again Professor Brodie's words: "This result is much better obtained if the anaesthesia be not too deep." This book is written for students.

7247. "Is not too deep"; but yet it may have to be deep enough to prevent pain being felt by the animal?—Professor Brodie would no doubt say that; but the whole fight over this question is on the different points of view. There are different opinions, and I present one side of the case.

7248. But you are citing a book and suggesting that it has a certain meaning, and then you say that therefore it is advocating to a certain extent cruelty—it is encouraging cruelty?—No; I say that it may lead to cruelty.

7249. (*Dr. Gaskell.*) Do not you know that in this kind of experiment of which you have quoted there is a difference in the result according to the amount of anaesthetic given?—That is just what I am suggesting.

7250. That it is not a difference of pain; it is a difference in the pressor or depressor effect, and also in the action of the nerves on the vasomotor centres?—Yes?

7251. And if there was any pain, the experiment could not be carried out at all?—I do not think that is so. I shall bring further evidence to show that experiments can be carried out under pain. Dr. Waller gives a similar caution in his "Exercises in Practical Physiology" (Part II., pages 46 and 47) in these words: "Stimulation of the Central End of the Lingual nerve. The lingual nerve is ligatured near its entrance to the mouth, and so stimulated. Unless the animal is too deeply anaesthetised, salivation is accelerated"—in other words, that is another caution that too deep anaesthesia would interfere with the experiment. In Professor Halliburton's Handbook there is another caution in these words: "It is essential, when the experiment of stimulating the cortex of the brain is being performed, that the animal should be anaesthetised, otherwise voluntary or reflex actions will occur which mask those produced by stimulation. If, however, the animal is too deeply under the influence of a narcotic, the brain is inexcitable." This statement of Professor Halliburton is supported by many similar expressions in the literature of vivisection. Thus Dr. David Ferrier, in his book "The Functions of the Brain" (2nd edition, 1886) says, in speaking of electrical stimulation of the brain, at page 224: "That which will cause intense and indefinite action in an animal non-narcotised will excite only moderate and definite action in an animal sufficiently narcotised to abolish all sense of pain, and no effect at all on an animal deeply anaesthetised." And on page 232: "Deep narcosis, however, abolishes the excitability both of the grey matter and the subjacent medullary fibres, so that no reaction whatever occurs in the application of currents otherwise effective." On page 166 in Professor Busch's "Laboratory Manual of Physiology," dealing with the subject of excretion, students get the following advice: "Narcotise a rabbit, lightly, with morphin. Anaesthetise with ether just sufficiently to keep the animal quiet." There is no attempt to introduce any moral consideration into these words—"just sufficiently to keep the animal quiet."

7252. (*Chairman.*) Is that your observation?—That is my observation—but instructions are then given for the utilisation of this animal for a number of other experiments. The Commission do not probably want to hear about them, because this is an American book; but it has an English publisher and is very well known in England. I have only a few more remarks about it.

7253. I think you might confine your evidence to English books. You have read a number of passages, and the real question is, What is the fair meaning of those passages? I do not think you add anything by going to American books. We may take it that a great number of books do contain expressions such as you have quoted. The question is really what is the fair meaning of them?—My next point is that there are cases where even no anaesthesia is spoken of as a condi-

tion that is quite likely to occur to be necessary. It is certainly not only light anaesthesia that is advocated in these textbooks; sometimes they calmly describe the course of experimental results in unanaesthetised animals, or point out that better results are obtained if no anaesthetics are employed. The following instances illustrate this. From "A Manual of Physiology," by G. N. Stewart (5th edition, pages 137-138)—

7254. Is that a modern book?—All the books are modern. The 5th edition was published in 1906.

7255. By "modern" I really meant after the Act of 1876?—Yes, all my books, I should say, are within the last 10 years. He is dealing with the depressor nerve in the rabbit, and he says: "Excitation of its central end causes a marked fall of blood pressure, accompanied by, but not essentially due to, a distinct slowing of the heart. If the animal is not anaesthetised there may be signs of pain, and for this reason the depressor has sometimes been spoken of, somewhat loosely, as the sensory nerve of the heart."

7256. What do you suggest is the suggestion made to students there?—The suggestion made to students is that experiments have been performed on animals that are not anaesthetised. "If the animal is not anaesthetised there may be signs of pain" when the depressor is stimulated; and my suggestion here is that experiments on unanaesthetised animals productive of pain do not seem to be an impossibility to teaching vivisectioners—nor do they withhold information.

7257. But do you really infer from that that Mr. Stewart is practically admitting that since the Act of 1876 licensed persons have performed these operations without anaesthetics?—I do not mean to infer anything more than what is said.

7258. But does it say that?—I do not think that writers on physiology have the Act of 1876 in front of their eyes when they are writing these textbooks for students. I think they are purely considering the scientific aspect of the matter.

7259. But when you are asking us to draw such strong inferences against a writer, is it not quite consistent that he is describing the result of observations that he has made when he has seen such operations performed in France or Germany?—I cannot possibly say what Professor Stewart meant to infer, but I shall come later on to similar statements by Professor Starling about the effect of pain, and I only quote them as being material to my side of the case; I cannot draw any inferences.

7260. I only suggest that if they only go so far as that, they do not seem to me to assist you at all?—I am afraid I must leave it to the Commission to draw inferences. I am not here to do that.

7261. (*Sir John MacFadyean.*) But you read this evidence to us with a special object, which is, to get us to draw certain conclusions?—Certainly.

7262. Do you wish us to conclude from it that the writers have actually performed, or that any other licensed person has performed painful experiments without anaesthetics?—I wish the Commission simply to draw the conclusion, first, that experiments are desirable under light anaesthesia, which I have already shown; and, secondly, that there are cases where no anaesthesia is scientifically preferable; and without committing myself to saying what was done here or there, or anywhere where I do not know, or suggesting anything personal or definite in connection with breaches of the Act. I only bring this scientific wish and scientific desire and scientific necessity before the Commission, and I must leave it to the Commission to draw their own inferences. Professor Flint says, in his "Handbook of Physiology for Students and Practitioners of Medicine," page 373: "If the abdomen of a dog, perfectly quiet and not under the influence of an anaesthetic, is opened, and a portion of the liver excised, rinsed in cold water and rapidly cut up into boiling water, the extract will show no reaction with Fehling's test for sugar." From the same book, page 374: "A notable experiment on the influence of the nervous system on the liver is the one in which artificial diabetes is produced by stimulation of the floor of the fourth ventricle (Bernard). The operation is not difficult. The instrument used is a delicate 'stilet' with a flat cutting extremity and a small projecting point about $\frac{3}{8}$ of an inch (one millimetre) long. In performing the operation on a rabbit, the head of the animal is firmly held in the left hand, and the skull is penetrated in the median line, just behind the superior occipital

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protuberance. This can easily be done by a few lateral movements of the instrument. Once within the cranium, the instrument is passed obliquely downward and forward, so as to cross an imaginary line drawn between the two auditory canals until its point reaches the basilar process of the occipital bone. The point then penetrates the medulla oblongata between the roots of the auditory nerves and the pneumogastrics, and its projection serves to protect the nervous centre from more serious injury from the cutting edge. The instrument is then carefully withdrawn and the operation is completed. This experiment is almost painless, and it is not desirable to administer an anæsthetic, as this in itself would disturb the glycogenic process."

7263. (*Dr. Gaskell.*) Would you mind mentioning to the Commission that Professor Flint is an American?—Professor Flint is an American, but this experiment is one that is frequently referred to in all textbooks on physiology, and I have here an English book describing the same experiment—it is "Recent Advances in Physiology and Bio-Chemistry," edited by Leonard Hill (page 340)*—which likewise describes those experiments, also without mentioning any anæsthetic.

7264. (*Chairman.*) What Professor Flint, I understand, said was that it was better to perform it without any anæsthetic?—Professor Flint distinctly says that anæsthetics must be avoided in experiments on the glycogenic function.

7265. Then it is very material to know that he was an American, because his book was written in America (and I suppose originally for Americans) where there is no vivisection law I understand in most of the States; therefore it is a very different case from that of England?—I wish to bring out, too, that an English book, "Recent Advances in Physiology and Bio-Chemistry," edited by Leonard Hill, describes the same experiment, also without mentioning the use of anæsthetics.

7266. But Professor Flint does mention the use of anæsthetics. Does the English author use the same phrase about anæsthetics?—No; he says nothing about it at all.

7267. But that is a most material difference?—I quite appreciate the difference, and I quite appreciate, too, why the English author says nothing about it.

7268. You think that by not mentioning the use of anæsthetics in a country where the law is that anæsthetics must be used, the author is implying that they ought not to be used?—I should think that as a scientist, as a physiologist, Dr. Hill knows quite well, what all physiologists know, that these particular experiments have to be performed without anæsthetics. I cannot form any opinion as to what he means by not saying anything about them.

7269. Are you citing this for the purpose of bringing home to our minds the action taken by those who advise the students in England?—Yes.

7270. I want to know whether you are reading that passage to show us that they refrain from advocating the use of anæsthetics, and therefore encourage illegal operations by such a passage as you have just read to us in an English author's book?

7271. (*Sir Mackenzie Chalmers.*) You know that that experiment would be illegal in England?—Yes. I am not at present dealing at all with how the law is carried out or administered.

7272. (*Chairman.*) It is the interpretation of what the author means that we are dealing with; we have to interpret his meaning, and one wants a great deal more than that. Do you mean to suggest that he is inculcating the illegal performance of operations in his book?

7273. (*Sir William Collins.*) Does Dr. Hill say that glycogenic experiments can be successfully performed in England without anæsthetics?—No, he does not say that, but he would know as a physiologist that it would be impossible from a purely physiological point of view. I now come to "Practical Physiology," by Beddard, Edkins, Hill, etc.

7274. (*Sir Mackenzie Chalmers.*) What Dr. Hill is that?—The same Dr. Leonard Hill. On page 156 we read, "The gastric secretion is obtained by making an

incision in the stomach, as in Figure 138, and then reflecting and suturing the mucous membrane of the stomach so as to make a separate secreting sac which is still in muscular and nervous continuity with the rest of the stomach. The mouth of the sac is sewn to the opening in the abdominal wall. The vagus is exposed and divided." I must say in fairness, that there is a note in Mr. Beddard's book saying "This experiment cannot be demonstrated." That note, I should think, evidently refers to the English law; but these are the words: "Three days later the peripheral end of the vagus is excited in the unanæsthetised animal, and the juice collected. Anæsthesia or operative procedures easily inhibit the gastric secretion."

7275. (*Chairman.*) Is Dr. Leonard Hill an Englishman or an American?—An Englishman. This is an English book, "Practical Physiology." This experiment is one typical of the methods of the Russian physiologist, Professor J. P. Pawlow, Director of the Physiological Department of the Institute for Experimental Medicine, St. Petersburg.

7276. (*Colonel Lockwood.*) But it does not refer to experiments done in England?—Yes, I am coming to that. Professor Pawlow has been much quoted lately by physiologists as a vivisector who is able to work under "perfect physiological conditions"—that is, no pain and no anæsthesia. Professor Starling has described those conditions as "an ideal" which has been attained by Pawlow; and in his admiration for these methods he used the following words in his evidence (Question 4119): "You get an animal and you alter its anatomy in some way or other so as to make some process accessible to simple observation, so that you can see what goes on without interfering in any way with the animal's comfort or with the animal's normal condition." In the Harveian oration for 1902, Dr. Ferrier refers to these methods as "novel and luminous." And there are many expressions of admiration for his methods in the literature of physiology. But a direct reference to Professor Pawlow's lectures published in England under the title "The Work of the Digestive Glands," shows that his methods are far from "ideal" or perfect from the physiological point of view, and that even those vivisectional methods speedily become pathological instead of physiological, and that disease and suffering are by no means absent. On page 183 Professor Pawlow says:—"I had by no means the intention at first to set up conditions of disease; I operated solely for physiological purposes, and kept my animals alive for months or years. How many and how profound pathological processes have under these circumstances come into existence before my eyes! I have seen in connection with a disturbance of the functions of the liver an enormous ascites develop, at another time an ascending paralysis of the central nervous system, or, again, a general lacerability of the blood-vessels, and so on." On page 171 he speaks of a dog which got an ulcer in the artificial gastric cul-de-sac, which gave rise to violent bleeding and produced a secondary peritonitis. And in describing pancreatic fistulæ he tells us (on page 7) that only one in every four or five dogs will be found which tolerates the operation without any nursing, meaning thereby that it is a difficult operation, which takes a great deal of nursing, or the animal will die. I suppose that is what is referred to. I have brought this before the Commission, although it may seem irrelevant, because Professor Starling, in quoting Pawlow's experiments, has at the same time brought forward a plea for more experiments in the future under Certificate B, that is, the certificate which allows the animal to recover from the anæsthesia and to be kept alive, and under which several operations are at present performed legally on one and the same animal. But Pawlow shows clearly himself that he has not attained any ideal conditions by any means. He uses the very words here in contradistinction to Professor Starling. Professor Pawlow himself says: "Our solution of the problem"—that is of these fistulæ—"is evidently by no means an ideal one" (page 8), and he tells in this book which I have brought with me how these fistulæ—pancreatic and other fistulæ—frequently produce corrosion and various skin troubles, which must give a great deal of discomfort and pain to animals. He tells a story of one of the dogs in his laboratory, and I am relating this because these are the vivisectional methods of the

* The chapter on "The Metabolism of the Carbohydrates," in which this description occurs is written by J. J. R. Macleod, M.B.—L. L.-af-H.

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future. Professor Starling has referred to them repeatedly before the Commission. Professor Pawlow tells the story of a dog in which a pancreatic fistula had been established, one of those animals to which Professor Starling refers as just having had the anatomy altered a little, so that the animal is "quite happy," says Professor Starling; there is no inconvenience. I do not remember his exact words, but he repeats several times that the animals are "quite happy." Professor Pawlow says that one morning he found that one of these dogs had torn down part of the wall in the laboratory, to which it was chained, and he found the animal was lying on the pulverised wall, evidently to try and take away the pain and the suffering that came from the corrosion which was caused by the constant trickling of the pancreatic juice; and the next morning he found that this dog had again, in its efforts to save itself, torn down another part of the wall to which it had been chained. And then Professor Pawlow found that he could devise certain means of taking away part of its pain and discomfort by allowing the animal to lie on sand. But I wish specially to point out that these experiments are by no means free from pain or discomfort.

7277. (*Chairman.*) Are you giving us the account now of an experiment which can be done in England without anæsthetics, or, at least, of a case in which a dog may be kept alive after the experiment?—I am quoting this to show one instance of experiments which can be done, and are done, in England now under Certificate B, and which have been specially brought out by Professor Starling as the "ideal" experiments of the future.

7278. But what you have been telling us happened in Russia?—Yes, because Professor Pawlow has been referred to.

7279. (*Sir John MacFadyean.*) Was that an exceptional case that you read, or was it the normal result of the experiments?—Professor Pawlow in several pages here relates the great difficulties he has had in making the state of these animals as normal as possible, but he by no means pretends that they are in a normal state.

7280. But would this be an average case or a special one?—This is a passage on page 6 in Professor Pawlow's book. It says: "In a very short time the abdominal wall becomes macerated by the escaping juice, and in places even fairly large bleeding patches appear. These continuously irritate the animal, and prevent the collection of pure juice by means of the funnel." Then, further on, he says that even now, when he has succeeded by many new methods, and after much trouble in preventing this corrosion and maceration of the skin, he seeks better methods, because the continual escaping of the pancreatic juice puts the animals in an abnormal condition, which interferes with their general state of health.

7281. But perhaps it was the exceptional painless cases that Professor Starling had in his mind when he spoke about the ideal condition. How do you know that he referred to the bad cases?—I wish specially to point out that Pawlow himself, after having quoted all his difficulties and improvements, says: "Our conditions are yet far from ideal."

7282. (*Sir Mackenzie Chalmers.*) These were early experiments?—No; these are the latest. I have the book here.

7283. What is the date?—1902. A new edition is being prepared, and, I hear, will be out in three weeks.

7284. (*Sir John MacFadyean.*) But Pawlow lectured in this country within the last two years, did he not?—I believe he lectured here last autumn.

7285. May he not have described the improved methods in those lectures?—I think he described the latest improvements, as he terms them, there; but it is after describing his improvements that he says of his methods: "Our conditions are yet far from ideal."

7286. But it was prior to 1902?—I do not think there has been any change since 1902, but I am not competent to answer that. I was not present at the lectures.

7287. You wish us to infer that Professor Starling's remark about Professor Pawlow's operations being ideal refers to those which you have quoted from the book?—Yes; that is what I understand.

7288. That is what you offer to us?—Yes, that is it; and may I also say that those conditions which are

desired to be ideal, but which are not in reality ideal, are exposed by another scientist—Paulesco, of Bucharest—who has written in the "Journal de Physiologie et de Pathologie générale," Paris (January, 1906), that he, too, has found that the establishment of biliary fistulæ leads to many abnormal conditions in animals which interfere with and frustrate the experiments. That anæsthetics interfere with the results of certain experiments is pretty generally acknowledged. That all physiological experiments are disturbed by the use of anæsthetics is freely admitted by the school of Pawlow. It is true that this school adds a theoretical and aspirational condemnation of pain, too; but in practice it is shown that pain is considered a lesser evil, and whenever there is a real conflict between moral and scientific claims, the vivisector chooses adherence to the claims of his science.

7289. (*Chairman.*) That is yours—you are not quoting?—That is my own.

7290. But, when you say that, what you are saying is that you apply it to English vivisectors. I suppose when you say the vivisector you mean the English vivisector?—I apply it to all vivisectors, including English.

7291. Then do you mean that the English vivisector, when he can get a better result with pain, defies the law?—Yes; I assume that. And I have a right to assume it at the present moment, because Professor Starling has said before this Commission that were there no such things as anæsthetics he would yet admire the man who performed experiments without anæsthetics—that is to say, the man who tortured animals for the sake of science.

7292. That is quite a different thing?—No; that is the same thing, I believe.

7293. That is quite a different thing from saying that he disobeys the law?—I only bring that forward in support of what I have just said—that if there is a real conflict between moral and scientific claims, the vivisector would choose adherence to the scientific claims.

7294. That is to say, that he would disregard the law, and perform an operation which caused pain because he got a better result?—I am not at present suggesting that anybody breaks the law.

7295. (*Sir John MacFadyean.*) Is there ever such a conflict in your opinion as you suggest there?—There is sometimes such a conflict as I have shown, and on the Continent no secret is made of such a conflict.

7296. Then you do mean to suggest to us that English vivisectors do break the law when that conflict arises?—Am I obliged to say again that I have not come here to suggest that British physiologists break the law.

7297. (*Chairman.*) But you use an argument which has no force whatever unless you are suggesting that?—I have come here to bring evidence of the existence of pain even in England.

7298. The sooner we come to that the better pleased I shall be. What we want is not suggestions that by a certain interpretation of a passage in a book such operations are encouraged, but real facts to show that they are actually performed?—I believe I am coming to that too, if I am allowed to proceed.

7299. The sooner we get to that the better?—With vivisectors and the defenders of vivisection cruelty is generally interpreted as the unnecessary infliction of pain and suffering, whilst in their opinion the supposed necessities of science at once justify the infliction of pain and suffering. This touches the very root of the unreliability of vivisectional anæsthesia. Foreign textbooks show great frankness in connection with the priority of scientific considerations to moral ones. For instance, Professor Robert Tigerstedt, a renowned physiologist, Professor at the University of Helsingfors, Finland, writes in his "Textbook of Human Physiology"—a book which is the standard textbook of German students, and which has now been translated into English, in 1906 (page 5): "Only if the purpose of the experiment makes it necessary is the operation performed on unanæsthetised animals"; and he adds: "Furthermore, it is many times of advantage in the experiment to suppress the voluntary movements of the animal, and this is done by administering the American arrow poison curare." A digression from textbooks into the records of experiments given by vivisectors in their journals and writings shows, if possible, still more conclusively their dislike of anæ-

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thetics. A typical instance is afforded by a paper on "The Pathology of Asthma," by T. G. Brodie and W. C. Dixon, reprinted in "Collected Papers from the Research Laboratories of the Royal College of Physicians, London, and the Royal College of Surgeons, England" (edited by Brodie), Paper 19, Part I., 1903: "A considerable difficulty we met in all our early experiments, and which we only learnt to explain after much trouble, was due to the action of the anæsthetic. A large number of our early experiments gave negative results, which we ultimately found to be caused by the use of chloroform or ether as the anæsthetic. These, we found, paralysed the nerve endings in the bronchial muscles, and we only succeeded in obtaining constant results on vagal stimulation when their use was reduced to a minimum. We therefore adopted the practice of anæsthetising the animal with the smallest quantity of chloroform and then destroying the medulla and brain. No anæsthetic was then required." And further: "Thus slight reflex constrictions have been described as following excitations of the sciatic, superior laryngeal, central end of the vagus, the cornea and the nasal mucous membrane, but the tracings given in support of these statements are far from satisfactory. When working upon anæsthetised animals our attempts to produce these reflexes were almost entirely negative. It therefore became necessary to avoid the use of an anæsthetic altogether, or only to administer it for a very short time. As it was also necessary to keep the medulla uninjured, we worked upon decerebrate animals; but the conditions were still far from ideal, for the decerebration involves a rather lengthy operation under anæsthesia, and the injury produced in the majority of cases appears to set up a constant inhibitory effect upon the medullary centres. When working in this way we therefore decerebrated the animal and then kept it for some three or four hours before proceeding with the experiment, in order to allow ample time for recovery from the anæsthetic. But even under these conditions the results obtained have not proved as successful as we had hoped, and while giving the reflex effects we have at present succeeded in recording, we feel that there are still many undecided points to be worked out before we can understand the normal working of these centres." I have quoted that to show the difficulty that the physiologist meets with when he endeavours to employ anæsthetics, and how anæsthetics interfere with experiments.

7300. That is to say, that carrying out the law presents certain difficulties to physiologists. That is what they say?—Yes. In a book entitled "The Physiology and Pathology of the Cerebral Circulation," published in 1896, by Leonard Hill, M.B., who says he has opened "scores of living animals," there are clear indications that chloroform and ether are distasteful to the vivisector. The method of the experiments was the following: "A cannula was placed in the central end of the carotid artery. A second long cannula was passed down the external jugular vein, and on same side into the right auricle. The torcular heriphihi was trephined, and a third cannula was screwed into the hole thus made. The cannula in the carotid artery was placed in connection with a mercurial manometer" (pp. 40 and 41). "My object," says Mr. Hill, "has been to make the method of research complete by simultaneously recording the arterial pressure, the general venous pressure, the intracranial pressure, the cerebral venous pressure." On page 40 we read: "The animals employed for this research were in most cases dogs. Morphia was the anæsthetic generally used; but the results have been controlled by the use of other anæsthetics. In no single case has morphia been found to obscure results, while ether and chloroform frequently do so." And on pages 58 and 59 we read: "Administration of chloroform produces a fall of arterial pressure and a rise in general venous pressure. . . . Ether produces a slighter fall of arterial pressure and a slight rise of pressure in the right auricle. . . . Morphia produces no peculiar effect on the cerebral or general circulation. . . . Thus morphia is far the best anæsthetic for researches of this nature. . . . the vaso-motor mechanism is in no way damaged by morphia, as it is by ether and chloroform." Sensory nerves were stimulated. We learn that: "Stimulation of any sensory nerve causes a rise of general arterial pressure and intra-cranial or cerebral venous pressure passively follows the rise." Curare was used, absinth, brain extract, and strychnine injected.

7300A. Is the object of reading that, to suggest that morphia is not an anæsthetic?—That brings me to my next point in connection with vivisectional anæsthesia and the "Shambles of Science," i.e., that the use of morphia as an anæsthetic for vivisectional operations, especially when combined with curare, constitutes cruelty, and that such use of morphia is illegal under the present Act, as morphia is not, in the words of the Act, an "anæsthetic of sufficient power to prevent the animal feeling pain." Even in experiments before students morphia is used. I have seen severe experiments performed under morphia, for instance one described in the "Shambles of Science," under the title "An experiment that is not supposed to be useful." This experiment, which involved opening the chest of a dog widely, removing the ribs, setting up artificial respiration, enclosing part of one of the lungs in an oncomotor, and stimulations of various kinds, was, according to the lecturer himself (Dr. Brodie), performed under morphia. Other severe experiments described in the book were, in the words of the lecturers, performed under "morphia, and scarcely any anæsthetic at all," and under "morphia and a little chloroform."

7301. (Colonel Lockwood.) Where is that described?—In the "Shambles of Science."

7302. (Sir Mackenzie Chalmers.) Are these the lectures that you attended?—These are the lectures described in the "Shambles of Science."

7303. Did you see this experiment?—Yes, I did.

7304. (Colonel Lockwood.) Under what head is it described?—The experiment that was performed, in the words of the lecturer, "under morphia," is described on page 55 of the first edition.

7305. (Sir John MacFadyen.) Was the animal curarised?—Nothing was said about that.

7306. Is that the experiment of which you say in the "Shambles of Science" that "we can be almost certain the dog has been also curarised"—that occurs on page 60?—Yes, that is the experiment.

7307. A minute ago you said that nothing was said about its being curarised?—I said that nothing was said about it.

7308-9. But you think it is almost certain that it was?—What we thought at the time was that it was certain it was curarised. I have already given instances of the vivisectors' preference for morphia and their dislike of anæsthetics. It is only natural that they should attempt to prove that morphia has the power of abolishing pain in vivisected animals. During the trial of Bayliss v. Coleridge, Mr. Bayliss said on the second day that morphia is an effective anæsthetic. I do not think I need go into the many statements to that effect that have been made. But these attempts to prove that morphia is an anæsthetic are futile. Vivisectors can have no greater authority than their own Claude Bernard, whose special work on the action of anæsthetics and narcotics is universally known and quoted, and whose name at the present moment appears in almost every textbook, and whose illustrations of vivisectional operations are reproduced in English textbooks. Claude Bernard quite appreciated the utility of morphia from the vivisector's point of view, though he was honest enough to acknowledge its utter uselessness as an effective anæsthetic for vivisected animals. These are his words: "The animal still remains sensitive, a touch on the cornea induces the closing of the eyelids, but he is quite still, and lends himself without a movement to the most delicate operation. . . . He feels the pain, but has lost the idea of self-defence."

7310. (Chairman). Do you suggest that the great majority of physiologists consider that morphia is not an anæsthetic?—I have not formed an opinion as to whether the majority or the minority do or do not; but I can bring forward a great amount of evidence of those who do not consider it to be an anæsthetic.

7311. When you used the expression about vivisectors, you have no better authority. There are a great many authorities who differ, as you know?—But Claude Bernard is very much appreciated, if one reads Sir Michael Foster's "Life of Claude Bernard," how he looks upon him as a father, and asks to be allowed to look upon the physiologists of France as his brethren.

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7312. We have heard a good deal of Claude Bernard since we have been sitting here?—I am coming to modern physiologists, if I may, in a moment.

7313. (*Sir William Church.*) Would you be good enough to tell us whether that statement, which we have had frequently before us, and which we have just had from you, by Claude Bernard, was spoken of morphia generally, or of a particular quantity of morphia in relation to an animal?

7314. (*Sir Mackenzie Chalmers.*) Have you the quotation there?—I have the quotation; but I do not think I have got the exact dose of opium that he refers to. I shall be pleased to send it, because I have the exact reference at home.

7315. (*Sir William Church.*) Will you please give us the exact reference to this passage of Claude Bernard?—I will.

7316. You are not yourself aware whether he made that statement of morphia in any quantities?—He made it about the action of opium on animals generally. These are his words: "The animal still remains sensitive, a touch on the cornea induces the closing of the eyelids, but he is quite still, and lends himself without a movement to the most delicate operation. . . . He feels the pain, but has lost the idea of self-defence." That is taken from Bernard's "Leçons de Physiologie Opératoire," page 155. Prominent veterinary surgeons who, above all, should have experience in this matter, deny that morphia is an efficient pain-destroyer. Among them I may mention Mr. Sewell, who has said that, in his opinion, morphia is not an anæsthetic for a dog in any sense. And Professor Hobday, who, I feel sure, is the best authority on animal anæsthesia which I can quote—

7317. (*Chairman.*) You are going a little too far in stating what witnesses said at the trial. If they are alive they can be brought here to give the evidence?—Then I will leave out the trial. I now refer to Professor Hobday, and I am very anxious to bring this before the Commission, because Professor Starling said that Professor Hobday is coming to give evidence; and Professor Starling evidently thinks that Professor Hobday will give evidence in illustration of the general ideas in connection with vivisectional anæsthesia prevalent among vivisectors; but I do not think that Professor Hobday will give evidence of that kind, for in his book, "Surgical Diseases of the Dog and Cat," published last year, there is an important chapter on the art of giving anæsthetics to dogs and cats. In contradistinction to Mr. Leonard Hill, and many other vivisectors, he considers chloroform the ideal anæsthetic to choose for the average adult dog. Instead of supporting the vivisectors' contention, that morphia is an anæsthetic for dogs, he says that, "compared with man, the dog is extraordinarily insusceptible to morphia" (page 46). He also says on pages 45 and 46 that "It (morphia) is classed as a narcotic rather than as an anæsthetic; and although it certainly deadens pain for minor operations, it does not do away with it altogether, as is the case with chloroform and ether. It is distinctly a useful aid in that the dazed, stupid condition in which the patient is put, renders it more tractable and easily managed. Professor Cushny has appeared before the Commission, and, in answering questions 5150 and 5270, has said that morphia is an effective anæsthetic for dogs, and that a large dose of this drug acts as an anæsthetic. These statements are of special interest, because in his "Text Book of Pharmacology and Therapeutics or the Action of Drugs in Health and Disease" (the fourth edition, 1906), Professor Cushny expresses entirely different opinions—opinions which make his statements before the Commission strangely contradictory, or which show that Professor Cushny holds one kind of theories when he speaks as a scientific pharmacologist, and another kind when he speaks as an ardent defender of the doings of vivisectors. On page 208 of this book, he says of the action of morphia on dogs: "The sensation of pain seems to be much lessened, but not entirely abolished, and reflex movements are difficult to elicit. After larger quantities an exaggerated sensibility to external stimulation seems present, for the animal starts convulsively at loud sounds and on pinching, but when left undisturbed lies in profound sleep."

7318. Do you suggest that Professor Cushny there is offering an opinion that the animal was feeling pain?—My position is this: I had prepared this statement of Professor Cushny's in my evidence which I was to have

given before the Commission when I was coming in March, as very strong and weighty evidence in favour of my view that morphia is not an effective anæsthetic in dogs. To my great surprise, on reading Professor Cushny's evidence before this Commission, I found that he entirely contradicted what he had said in his own textbook, where he says that larger quantities in the dog produce an exaggerated sensibility, and also that pain seems to be lessened, but not entirely abolished.

7319. (*Sir John MacFadyean.*) Are these not doses which are still non-poisonous?—I do not think that is inferred. He says that larger quantities produce an exaggerated sensibility.

7320. Is it possible, I suggest, that they are still non-poisonous?—That argument cannot be used here, because Professor Cushny says that larger quantities produce an exaggerated sensibility—the more you give the more exaggerated sensibility to external stimulation seems to be present.

7321. You think that follows?—I think it follows. I have the book here. He says at page 208: "The sensation of pain seems to be much lessened, but not entirely abolished, and reflex movements are difficult to elicit. After larger quantities an exaggerated sensibility to external stimulation seems present, for the animal starts convulsively at loud sounds and on pinching, but when left undisturbed lies in profound sleep."

7322. (*Sir William Church.*) But do you wish us to infer from what you have read and stated of morphia or opium that it is impossible, first of all, to stupefy a dog so that it cannot feel pain; and, secondly that it will not die afterwards from the effects of opium?—I think morphia is as little an anæsthetic for dogs as it is for human beings; and I am entirely supported there by Professor Hobday, who does not consider morphia in any way an anæsthetic.

7323. I wish you would give me an answer, please. Do you wish the Commission to take evidence from you that morphia, if given in large quantities, will not produce complete anæsthesia?—Yes.

7324. I mean in quantities that are fatal?—Yes.

7325. That is your statement, that it will not produce anæsthesia?—What I wish the Commission simply to take as my evidence is that I do not consider morphia to be an anæsthetic at all.

7326. You say that you do not consider it; but do you state that in your opinion an animal cannot be made perfectly unconscious with morphia, so deeply unconscious that it will not continue to live after a certain time?—I suppose you can poison it?

7327. Do you deny that you can do it in man?—I suppose you can poison a man to death with morphia, as you can poison a dog to death with morphia, I am not denying that, but at present I am speaking of the anæsthetic properties of morphia, and I say that it has none.

7328. Do you mean that a man, when unconscious from morphia, feels pain?—No, I do not mean that.

7329. Do you mean that when an animal is unconscious from morphia it feels pain?—There is a very great difference between an anæsthetic and a narcotic. If morphia were an anæsthetic it would be included in the numerous textbooks on anæsthesia.

7330. Never mind about the textbooks. I wish simply to get your opinion before the Commission. Do you think that when a man is under the complete influence of a sufficiently poisonous dose of morphia or opium he feels pain?—That case is somewhat apart from my case here. I must still maintain that I am speaking of morphia as an anæsthetic, not as a poison.

7331. (*Chairman.*) How long may a person or a dog live after it has received a poisonous dose, a dose that will ultimately kill it, without its coming to?—That question I think had better be asked of some physiologist.

7332. Is it not rather material in answer to what Sir William Church asked you? Supposing that a dog lived for half an hour under a dose that was really poisonous—supposing it had resisted that poisonous dose, could it not be operated upon during that half hour?—It is very difficult for me to answer. Witnesses often answer questions in a few words which give quite a wrong impression because they are not

allowed to state their whole point of view, and I do not wish to give an erroneous impression.

7333. (*Sir Mackenzie Chalmers.*) May I take it that your point is that if a large dose of morphia is given the animal may die, but that up to the point of death it may feel pain?—That is my point; and that is brought out by the evidence of Claude Bernard and many others.

7334. That is your point?—Yes.

7335. (*Sir John MacFadyean.*) Is the contrary impossible?—May I also state that large doses of morphia have been known to act as a violent stimulant?

7336. (*Sir William Church.*) I beg your pardon, but the question I asked you was in the case of a dose which is perfectly fatal, when the animal is not excited, but when it has become perfectly unconscious. Do you think that animal feels pain?—Then my answer is that we do not know, and that the evidence is overwhelming on the side that that animal may be feeling pain although it may be lying perfectly still.

7337. Do we or do we not know whether a man in that condition feels pain?—I think we know that a man fatally poisoned with morphia does not feel pain.

7338. Then on what grounds do you infer that an animal in a similar condition feels pain?—Because morphia, as Professor Hobday points out, acts differently on dogs and men. He says that dogs are extraordinarily insusceptible to morphia.

7339. Then your answer to me is that Professor Hobday says so?—Yes.

7340. (*Sir John MacFadyean.*) Do you know whether Professor Hobday has ever administered a poisonous dose of morphia to a dog? Are the experiments that he has carried out not cases in which he has employed non-poisonous doses? He dare not employ a poisonous dose on his patient?—That I do not know.

7341. But that is very material if you quote his evidence as being conclusive?—May I also say that such terribly poisonous doses of morphia would interfere entirely with physiological experiments? There again I quote Claude Bernard and various other authorities. For instance, experiments on the secretion of the salivary glands are frustrated by too much morphia or opium; and I think if a dog were poisoned to such an extent with morphia the object of the experiment would be defeated.

7342. (*Sir William Collins.*) Do you quote Professor Cushny to show that if it be true that a dog under morphia betrays an exaggerated sensibility, at any rate in that stage, it is not in an anaesthetised condition?—Yes, that is why I quote Professor Cushny; and I have one more sentence from Professor Cushny's book relating to morphia upon cats. May I also say that the opinions of Professors Charles Richet and Dastre in France, and Professor Sollmann in America, are that morphia is not an effective anaesthetic? I quote Professor Charles Richet especially on the point of anaesthetising animals in laboratories.

7343. (*Sir William Church.*) Do you mean by these gentlemen saying that it is not an anaesthetic that if it is used in large quantities it does not prevent the animal feeling pain?—I mean to say that if large quantities would induce anaesthesia, I am sure that these gentlemen would not say so definitely that it is not an anaesthetic. Professor Cushny gives his experience of the effect of morphia on cats and other animals and we learn that "In the cat and in all the other *felidæ*, and in the horse and ass morphin seems rather to increase than to diminish movement. The animal runs about the room, or in a circle, and seems unable to remain at rest for a moment. At the same time a depression of the intelligence and of the power of perception makes itself manifest, for no definite attempts at escape are made, and obstacles are not avoided so carefully as by the unpoisoned animal. Eventually, convulsions similar to those seen after strychnin are developed" (p. 207). In connection with that, but I think the Commission will not care to hear it, I have brought considerable evidence to show that a great many experiments are performed on cats in this country under morphia; but I will leave that as there is no doubt thrown upon it, I should think.

7344. (*Colonel Lockwood.*) Under licence, I suppose?—Yes, under licence.

7345. (*Chairman.*) At any rate, if they are we should have the witnesses who were present at them to tell us

what took place?—But it is not illegal in England, of course, to use morphia as an anaesthetic, and my point of view rather is that it should be illegal.

7346. (*Sir Mackenzie Chalmers.*) It is not a prohibited anaesthetic?—No. My point of view is that under the present Act of 1876 it should not be counted as an efficient anaesthetic. That is why I have brought all this evidence forward.

7347. We have been told that morphia is very often given as aiding other anaesthetics which are always given. You do not object to that?—I think one can have no objection to it when merely preparatory to anaesthesia, but I object to experiments which are done only under morphia or under morphia and curare, and especially on cats. Professor Hobday I know said to me that morphia is not an anaesthetic for dogs, and still less so for cats.

7348. (*Sir John MacFadyean.*) In non-poisonous doses?—Professor Cushny says that the larger the dose the more exaggerated the sensibility.

7349. But Professor Hobday did not say that?—But Professor Cushny does. I did not discuss that particular point with Professor Hobday, but I have no doubt that he would say the same. If it acts on cats in that way, so that they are unable to remain at rest for a few moments, it cannot surely be an anaesthetic. I should now like to be allowed to draw the attention of the Commission to the difference between vivisectional operations and surgical and veterinary operations under anaesthesia, as attempts are often made to identify the two.

7350. Have you yourself attended at some veterinary operations?—I have attended at some veterinary operations, and I have also attended some vivisectional operations, and surgical operations too, and therefore I am anxious to draw a distinction. Veterinary operations are not disturbed, but aided, by deep surgical anaesthesia. Vivisectional operations, dealing as they do with the delicate measuring of functional activities, are often disturbed by deep anaesthesia. Suffering after veterinary operations can be mitigated in every way. Suffering after vivisectional operations often forms part of the experiments, its causes and development being carefully recorded. Through the courtesy of Professor Hobday I was some time ago allowed to be present at an abdominal operation on a dog which was in a state of perfect surgical anaesthesia, in which every sign of consciousness or pain was entirely absent, and the dog lay in an absolutely limp and motionless condition through the action of chloroform administered by a special anaesthetist who devoted his whole time and attention to this, and who kept the anaesthesia uniform by constant and careful doses of the anaesthetic. The difference between this anaesthesia and conditions which I have seen in the physiological laboratories is very great. The whining and struggling dog which I saw on February 26th, 1903, at the Physiological Laboratory University of London, in which the abdomen had been opened, the intestines pulled over to the right side, and the kidney placed in an oncometer, the neck opened, and which was subjected to the usual stimulation of nerves, compression of veins, and injection of foreign substances, is an instance of a victim of the light anaesthesia which the vivisector prefers.

7351. (*Colonel Lockwood.*) That you saw yourself?—Yes. It is described in the "Shambles of Science" under the title "A Troublesome Dog"; it was troublesome because it whined.

7352. (*Sir John MacFadyean.*) When you say it was a victim, you intend to suggest that it was conscious of pain?—I intend to suggest that so far there were signs of consciousness.

7353. What do you wish us to believe, because there is no point in it if the animal was quite unconscious?—The difficulty of proving this is that nobody, neither the Commission nor I, nor any vivisector, can say whether that particular dog felt pain. I simply say that the dog whined and cried and seemed to be conscious, and we never can go by any other signs. How can we know whether an animal feels pain; we can only go by signs?

7354. I merely asked you whether you wished us to believe that the animal was conscious?—I wish to point this out as a typical instance of the kind of anaesthesia that is used in vivisectional experiments, and the great difference between that anaesthesia and the anaesthesia that I saw in Professor Hobday's surgical room. The object of the veterinary surgeon is generally the alleviation of pain and suffering; the object of the experi-

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mental physiologist and pathologist is sometimes the production of pain and suffering. And this brings me to my next point—namely, that books intended for students do not even avoid referring to the production of pain in animals as part of the ordinary routine of experimental experience. In his book "The Elements of Human Physiology" Professor Starling speaks of the stimulation of the (sensory) afferent nerves of the heart (page 249) in these words: "We may investigate their functions by stimulation of the central ends of the divided nerves. They may produce one or more of four effects: (i.) Pain as evinced by the movements of an animal not fully under the influence of an anæsthetic (we should be more correct if we said that stimulation of these nerves produced reflex movement)." Professor Starling no doubt speaks from personal experience here, and yet he has told the Commission, in answer to Question 3451, that though he has been engaged in the experimental pursuit of physiology for the last 17 years, he has never seen pain inflicted in any experiment on a dog, cat, or rabbit. On page 373 Professor Starling enlarges upon his knowledge of the effect of painful stimulation, and tells us that "any painful stimulation of a sensory nerve, especially in the abdomen, excites a reflex inhibition."

7355. (Chairman.) I do not know enough of these terms, but when he uses the word "reflex," does that imply that they do not feel pain?—That has really very little to do with it. A reflex movement may be painful or it may not.

7356. It may be painful?—Yes, but he speaks of the production of pain specially.

7357. (Dr. Gaskell.) On what page?—On page 373, "painful stimulation of a sensory nerve"; and on page 249. Another book which refers to the production of pain in a similar way is Mr. Hodgkinson Lack's "An Introduction to Physiology" (published 1905), and written for the student and future medical practitioner. This book is certainly not a very important one, but it is written in a way which is characteristic of the spirit with which even the younger generation of vivisectors treat experiments upon live animals. Anæsthetics are not mentioned, and animals are written of in this style (page 68): "Take a healthy frog and dissect out the steadily beating heart." On page 73, on afferent nerve fibres (vaso constrictor): "Dissect out the cervical sympathetic in a rabbit. Section it, and the arterioles in the rabbit's ear relax, and the ear becomes flushed with blood." Or (vaso dilators): "Dissect out the chorda tympani in a dog, and section it, no marked changes will be noticed in the submaxillary gland" (page 74). Or page 124 (the function of the renal tubules): "An animal is taken, and the medulla is sectioned below the level of the vaso-motor centre, and therefore there is a fall in general blood pressure. . . . The animal is kept alive by means of artificial respiration." On pages 52-53-54 stimulation of a spinal nerve and its roots is dealt with, and the author gives a diagram with indicatory letters, and points out in a table the various effects of stimulation, pain being several times included among the results. I wish specially to emphasise that the indifference or the callousness which generally characterises these books must tend to lessen the humane feelings of, and to demoralise, all those who take up the study of physiology, and also that the conspicuous absence of special warnings against cruelty in a field of research, manifestly open to abuse, even in the eyes of those who do not consider it inseparable from cruelty, brings proof that the boasted humane-ness of the modern vivisector is an attempt to quiet the public, and to allay the steadily-growing opposition to vivisection.

7358. (Chairman.) Do you read that last passage as an instance of callousness?—I read the last passage and refer to the last book as an instance of callousness.

7359. Why? It is written, I suppose, for medical purposes?—It is written for the student and future medical practitioner, and is meant to serve as an incentive to further study.

7360. But what is there that is callous about it if he is actually describing the facts? He is not, as I understand, using any brutal expressions about it; he is describing it in the ordinary way?—The callousness would come in in the fact that this is a book which is supposed to be an educational work, and that, whilst he has time to write of all the details of the experiment, the curare and artificial respiration, he does not even mention anæsthetics throughout the book.

7361. He does not need to mention them at that time?—Also the style in which the animals are spoken of: "Take an animal." "Take a healthy frog, and dissect out the steadily-beating heart."

7362. Supposing you were describing the action of a cook, or of somebody who is recommended to kill a fowl in a particular way, you would not expect the cookery book to give directions to the cook to be particularly careful over it. I cannot see where the callousness comes in?—It is all, of course, a matter of opinion. What seems callous to some people does not seem callous to others. I can say confidently that to the humane section of the community this book would seem callous because of the omission of any reference to anæsthetics.

7363. It does not seem callous to me; and everyone who does not agree with your view of that passage is inhuman, I suppose?—No.

7364. (Sir John MacFadyen.) Might I ask the name of the author of that book?—I have said that this book is not among the most important ones. It is Mr. Hodgkinson Lack. His name appears in the Medical Directory for 1905, but I cannot find it in the Directory for 1907. However, he was a licensed vivisector in 1905. I believe he lives in Edinburgh, but I do not know much about him. I only know that the "British Medical Journal" in a review some time ago did not seem to admire the book, but said that it was written for cramming.

7365. I was just going to ask you, was it not a cram book when students are expected merely to get up facts with regard to results?—Even if it is —

7366. Do you suggest that it is an invitation or incitement to students to go and do the things which are described in it?—I suggest that, because the author himself says that the book is meant to serve as an incentive to further study, and as an incentive to further study I maintain that it could have been accompanied by a little humane counsel or a few words at least about anæsthetics.

7367. (Sir Mackenzie Chalmers.) Is there not a distinction between study and research?—Yes, there is, but thoughts generally lead to action, and therefore I would say that humane thoughts ought to prevail in these books, as actions are the fruits of thoughts.

7368. (Sir John MacFadyen.) Do not you think that the author was much less ambitious, that he merely expected his book to serve as a cram?—His ambition did not prevent him speaking of curare and artificial respiration. I wish his ambition had led him to talk of anæsthetics, too, which it might easily have done.

7369. (Chairman.) However, you quote that as a strong instance of the callousness of the medical profession?—No, not of the medical profession. I was speaking of vivisectors.

7370. (Sir Mackenzie Chalmers.) Physiologists?—Yes, licensed vivisectors.

7371. (Chairman.) Who are supported by a large majority of the medical profession?—It remains to be seen, of course, how many they are supported by.

7372. I did not know that it was disputed that they were supported by a large majority?—No, perhaps not.

7373. (Sir William Church.) All these text books and various other books that you have quoted you quote to show that they do not suggest humanity to students, but if anything you say that they would rather lead to callousness on the part of students, because, in your opinion, they are calculated to incite students to do these operations for themselves, and also to cause them to be callous as to the pain that they may inflict?—I have not quoted them for the purpose of showing that they may incite students to take up illegal vivisection; that has not been my point of view; but I have quoted them to show that the general tendency in them is one of disregarding altogether all moral or humane considerations, and that the study of physiology at the present moment cannot but be entirely indifferent to the humane and moral claims.

7374. But then I want to ask you further whether it is not the fact that anybody who wishes to experiment or to witness these experiments has to either perform them or see them done in a licensed laboratory?—Yes.

7375. And do you mean us to infer that the tone of the directors of these laboratories is one tending towards cruelty?—In quoting these text books I have criticised the tone of them, but I am quite prepared, if I am asked the question, to say also that in my personal experience I have found nothing in the teaching of physiologists which justifies the claim that is made that vivisectioners nowadays do everything to further humaneness. I have seen nothing which justifies the claim of vivisectioners that the modern vivisectioners are endeavouring to inculcate humaneness. That is the red thread in that part of my evidence.

7376. The object of your having quoted all these textbooks to us this morning is to show that the directors of the physiological laboratories in general are regardless of the sufferings of animals, and do not impress upon their students the necessity for humanity at all?—That cannot have been my object, because the authors of these textbooks are not all directors of laboratories—at least, I have not put that forward; but I have put forward these books, and have quoted from them to show the lack of any signs of a wish to inculcate humaneness in the theory and practice of experimental physiology.

7377. Why should they, because all this vivisection to which you allude goes on in laboratories where, of course, the experimenters come into communication with the directors of those laboratories, and generally—at least, I believe so—they receive advice as to their action with regard to animals?—That claim might have been put forward if vivisectioners had not put forward this other claim on which I base my statement, that everything is done to inculcate humaneness. And then, further, everything is also done to make young students interested in experimental physiology, to take it up. Professor Starling has advocated his views before the Commission that students should first attend vivisectional demonstrations, and further take up vivisection themselves for the purpose of acquiring manipulative skill, and under those circumstances, as vivisectioners are so anxious to spread their teachings and to reach the younger generation, I maintain that their indifference from the point of view of humaneness and mercy and kindness and all such things shows that they are indifferent on that point, an indifference which sometimes amounts to callousness.

7378. You would not go so far as to say that the directors of physiological laboratories generally teach their students that they should study by means of scientific experiments, perfectly irrespective and regardless of pain, that they need not consider that at all. That is rather the effect of your remarks this morning on my mind?—I do not think I have suggested that. In fact, my suggestion is not that there is any deliberate encouragement of cruelty, but that there is an entire indifference to the humane side of the question.

7379. (Chairman.) Will you proceed?—I wish specially to emphasise that the indifference and callousness which generally characterise these books must tend to lessen the humane feelings of and to demoralise all those who take up the study of physiology, and also that the conspicuous absence of special warnings against cruelty in a field of research, manifestly open to abuse, even in the eyes of those who do not consider it inseparable from cruelty, brings proof that the boasted humaneness of the modern vivisectioner is an attempt to quiet the public and to allay the steadily growing opposition to vivisection. Among such demoralising literature, a book just published by Messrs. Macmillan and Company, in London, is conspicuous. It is "Collected Papers on Circulation and Respiration," by Sir Lauder Brunton. In 1907 Sir Lauder Brunton calmly publishes a paper (included in this volume) on "Experiments on the Influence of Digitalis on the Pressure of Blood in the Arteries" (page 103). (I have that book here, too) which, it is true, records experiments performed before the present Act, but which, nevertheless, are of a peculiarly brutal nature, and couched in words which cannot but give the direct lie to the assertions of vivisectioners, that the old-fashioned experiments would be met with disgust to-day. These experiments, which involved opening the neck of dogs and sheep, exposing the carotid arteries, and inserting the nozzle of a hæmadynamometer, and injecting the digitalis through the open jugular vein, have, I believe been referred to before this Commission by Professor Cushny as having been done under anaesthetics (Q. 4655). Professor Cushny also asserted that had no anaesthetics been used the

experiments could not have been performed owing to the movements of the animals. That is at numbers 4660A and 4663, and also that the animals were not allowed to recover. The following extracts from Sir Lauder Brunton's own paper show the inaccuracy of Professor Cushny's statements: Experiment I., November 25th, 1865—

7380. (Chairman.) But really it is extremely immaterial whether an operation before 1876 was or was not performed under anaesthesia; it was not illegal at the time, and it is taking up time unnecessarily. You said something about Sir Lauder Brunton alluding in brutal terms to some operations before 1876. Will you read what those brutal terms were?—I say my point of view is not that these experiments were performed in 1865. My point of view is that the republication in 1907 proves that vivisectioners are not ashamed.

7381. I quite understand that, but you said that it proved that he was callous and brutalised on the subject of operations, because he spoke of these operations which were before 1876 in brutal terms?—Yes.

7382. What are the brutal terms?—I am coming to them.

7383. Would you come to that—because that is the only material thing?—I said that these experiments were nevertheless of a peculiarly brutal nature, couched in words which cannot but give the direct lie to the assertions of vivisectioners that the old-fashioned experiments would be met with disgust to-day.

7384. Will you refer to the terms in which he speaks of them?—May I refer to their peculiarly brutal nature? The experiment was performed on a pointer. "It was tied down to a table and chloroform administered while the carotid was being exposed." This animal is described as struggling (p. 105). The same animal was used for Experiment II. on November 29th. The experiment began at 1.58, and among the author's remarks I note the following: "Time, 2.53.—Dog appears to be sick, but does not vomit, the muzzle preventing him from opening his mouth at all. Dog moans. Time, 2.55.—Groaning much increased and louder. 2.56.—Appears to be endeavouring to vomit. 2.59.—Convulsions. Stoppage of heart's action. Dog dead." The post-mortem revealed that "The veins and right side of the heart were engorged and swollen, stuffed like sausages, as Caspar describes it." On the same day, Experiment III. was performed on a full-grown sheep. The experiment began at 8.35 p.m., the sheep being tied down on the table. No chloroform was given. Extract from remarks: "9.22—

7385. (Sir William Collins.) Does Sir Lauder Brunton say that no chloroform was given?—No chloroform. I have the book here.

7386. (Sir Mackenzie Chalmers.) What is the date of it?—1865.

7387. Were they in England?—Yes.

7387A. (Sir William Church.) Was it not in Scotland, to be quite correct?—They were in Britain; they may have been in Scotland. "9.22" (this is a sheep).—"Animal excited by examining the wound. 9.45.—Sheep struggling. 9.47.—Sheep struggles." The next morning, on November 30th, the sheep "seemed still much depressed," and between 1 and 2 p.m. it was again tied on the operating table. This time chloroform was administered, and the other carotid opened. Extract from remarks on this experiment: "2.11 p.m.—Sheep appears completely free from the effects of the chloroform. 2.14.10.—Violent struggles take place. 2.16.15.—An apparently convulsive movement of the head. 2.17.10.—Violent struggles. Another struggle. 2.22.10.—Animal violently struggling. 2.33.—The sheep was allowed by all to be dead." On December 20th the fifth experiment was made on a large dog.

7388. (Chairman.) Is this the same sort of thing that follows?—Yes. I will not read it all; I will just give an extract. On December 20th the fifth experiment was made on a large dog, a kind of bloodhound. Chloroform was given, but Sir Lauder Brunton explains that after he had exposed the carotid, which lay very deep, the dog was seized with convulsive tremors. Then he describes the course of the experiment in these words: "Howling loudly, as if suffering much" (p. 111). Then he speaks of the eye, and says: "Pupil less contracted, seems normal, sensitive." Another experiment—Experiment VII.—

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7389. I do not think that multiplying instances is any good. Surely three are enough?—It is characteristic that in describing another experiment he states that "the dog appears to be free from chloroform, and seems to feel pain, as he groans" (p. 121).

7390. That is exactly the same thing; but those are operations that cannot be performed now; the Legislature thought they were improper and cruel, and put a stop to them; therefore it does not help us much now to describe those experiments. But, supposing they had to be described, how would you describe them in different language? Do you say that it is inaccurate language?—I would never publish them now. I should have been thoroughly ashamed of them.

7391. Do you mean to say that in an article to be read by scientific men, you think that a scientific man is doing something brutal in publishing it?—I think so, if his book is likely to be read by a very large number of students. And, further, the Commission has heard these same experiments described from the scientific point of view. That scientific view has been put before the Commission by Dr. Cushny, and therefore I beg leave to put our view before the Commission. I put them before the Commission from the moral point of view; they are the same experiments, and from the moral point of view I say that they are repellent, and I care not whether they are useful or not; from the humane point of view they are utterly worthy of—I will not use a term.

7392. They are illegal now. The law has said that they are illegal on the ground that they are cruel, and there is an end of it. I do not see the object of reading out an account of them. I thought you read it for the purpose of showing that Sir Lauder Brunton, as a leading physiologist, rather gloated over things of this kind when he described them. I can see nothing except an ordinary description. If these things are to be described at all, they seem to me to be described in the ordinary language that you would expect from an educated man?—I read them simply as being material to my point of view, to show that the modern literature of vivisection and physiologists tends to demoralise those who read it, and I brought, as the very strongest instance I could bring, the republication in 1907 of such terrible experiments in a book which can be bought for 7s. 6d. by any medical student.

7393. (*Colonel Lockwood.*) And to destroy the opinion that vivisectionists wish to get abroad that they are humane in their ideas?—Yes. It has often been said that the old-fashioned experiments would never be allowed; that they would be met with disgust and hatred to-day.

7394. (*Sir William Collins.*) Are these the same series of experiments in regard to which Professor Cushny answered the noble Chairman, at question No. 4660A, by saying, "I am quite sure Brunton would use anaesthetics"?—Yes.

7395. (*Sir John MacFadyen.*) Is your train of argument this: that, assuming that in the past painful experiments, which would not now be allowed, had really contributed useful knowledge with regard to the cure and prevention of disease, all reference to those experiments and to the facts they disclosed should be blotted out of published books?—My point of view is that the vivisectionist who is responsible for them would certainly not republish them in 1907 if he really had any feelings with regard to humaneness, or want of humaneness, in carrying them out.

7396. So that you would really establish a sort of Press censorship to obliterate all record of any past painful experiments, although they had contributed very useful knowledge?—I think that certainly from an educational point of view it would be much better and more in accordance with the claims made by vivisectionists.

7397. (*Chairman.*) That, I understand, ends that part of your evidence?—That ends that part.

7398. And we come next to your own experience of experiments?—Yes.

7399. (*Chairman.*) Coming to these experiments described in the "Shambles of Science," it struck me that it would be a convenient course (but if it is not you will tell me so) if I asked you—to take as an instance the "Painless Experiments" whether as regards the facts stated in your book you adhere to the

statements you made there, or whether you wish to alter anything?—No, I adhere to everything.

7400. Then I will take it in that way, and any questions can be asked you upon it with a view to making you modify or explain anything?—If you please.

7401. I will take it very shortly then. You have already told us that you proceeded to attend a series of lectures in 1902, which you afterwards described in the book called the "Shambles of Science"?—Yes.

7402. The first of them, I see, is called "Painless Experiments," at page 12; I have got the older copy?—Yes; page 11, I think.

7403. That was an experiment on a frog, and I think what you say there is that the frog was not properly pithed, or, at any rate, that sufficient was not done to destroy its nervous sensation—its feeling?—It was one of the ordinary experiments on frogs which are not properly pithed; and I have some evidence that I am very anxious to bring out in connection with that very subject, if I am allowed to do so.

7404. If it is what somebody else says about it on that particular case, I am afraid we must have the witness before us. We cannot have your account of what another witness may say about it?—It is a theory that I wish to bring forward in support of what has been said in this chapter on "Painless Experiments." It is a theory of my own, but it is one which is supported by other people.

7405. As to what happened to the frog?—No, not as to what happened to this particular frog, but as to frogs generally in physiological laboratories.

7406. We shall get into some very wide questions if we go into that. We had better treat it simply. You can hardly call that in support of your own account of what took place?—It is in support of what took place, and it is solely in support. It is in support of my account of what took place; but I can very well, if it is the wish of the Commission, wait and answer all questions first.

7407. You say that it applies to this particular case?—It does.

7408. Then we will take it now?—This experiment describes certain very ordinary experiments upon frogs which are often performed in physiological laboratories, and which form the very ABC, if I may use that expression, of experimental physiology. These frogs were, in the opinion of orthodox physiologists, incapable of feeling pain because the brain had been destroyed. The theory I wish to bring forward is my own firm belief that it has not been proved that a frog is incapable of feeling pain so long as it is capable of these complicated purposive movements which show sensation and volition, and several other qualities, which, at any rate, cannot be associated with any animal that is said to be dead—because very often these frogs are talked of as being "dead"; and my contention is that these frogs are not dead. That is my first point. And the second is that it is not proved that they are incapable of feeling pain.

7409. You have not, I presume, tested your theory by any experiments on frogs?—No, I have not. Then, in connection with that, I think that Professor Starling's evidence before the Commission brings out very clearly that students are at the present moment allowed to experiment upon frogs. I have the exact reference here.

7410. (*Colonel Lockwood.*) Without licence?—Without licence—because students do not generally hold a licence. It was in answer to Question No. 3747 that Professor Starling said of students: "At present they have to get a large extent of their practical experimental knowledge from a frog; a frog is a long way from man." And then he goes on to advocate that students should be allowed to experiment on a rabbit. This statement seems to show clearly that Professor Starling does not consider it a contravention of the Act that students should at present be allowed to perform experiments themselves on frogs. I find similar statements in other books, especially in a book called the "Physiological Action of Drugs," by M. S. Pembrey and C. D. F. Phillips, in which there are statements in the preface which show that physiologists at the present moment look upon experiments on frogs as legitimate for students to perform. Now, the Act applies, so far as I understand, to all vertebrate animals, and I am

not going to repeat the mistake which I believe the Editor of the "British Medical Journal" made in 1897—I suppose it was a slip*—of saying that the frog is not a vertebrate animal. The frog is supposed to be a vertebrate animal, and as such I maintain that the frog should come within the influence of the Act of 1876.

7411. I daresay you will be asked some questions about your account of what you saw by various Commissioners; I do not propose to ask you any other question myself?—May I say further in connection with this that the phenomena that we describe in this chapter are quite ordinary phenomena? There is nothing extraordinary in them. Brainless frogs are capable of jumping and croaking; they can feed by catching passing insects, and the longer they live the more do they resemble intact animals. The whole question whether brainless animals are capable of feeling pain is an exceedingly important one in connection with antivivisection and the whole vivisection question. The frog is so low down in the zoological scale that the spinal cord is capable of taking up the functions of the brain when the brain has been taken away; and we find this in a lesser degree the higher up we go in the zoological scale; but even dogs can live without their brains. Goltz had a dog which lived more than a year and a half without its cerebral hemispheres. He believed that this dog had lost understanding, reflection, and memory, but not sensation—that it was capable of emotions and voluntary power, that hunger and thirst were present. These dogs of Goltz experienced satisfaction when fed, became angry when attacked, saw a very bright light, avoided obstacles, heard loud sounds, and could be awakened by them. Professor Sherrington, in a well-known book, called "Schäfer's Text Book of Physiology," on the subject of cutaneous sensation, refers to the dogs that were experimented upon by Goltz in these terms: "The dogs from which Goltz has removed the cerebral hemispheres reacted to 'painful' stimuli in a way which was interpreted as evidence of physical pain." I have some further extracts from books by Dr. Ferrier and Professor Starling to show that rabbits deprived of their brains are capable of feeling pain.

7412. (*Sir Mackenzie Chalmers.*) When deprived of the brain, or a portion of the brain?—The whole brain. Dr. Ferrier says, on page 146 of his book, "The Functions of the Brain," a very well-known book: "Animals deprived of their cerebral hemispheres are still capable of exhibiting, in response to various forms of sensory stimulation, special and general, reactions, more or less complex, which do not at all differ in character from those which we associate with feeling or emotion." . . . Rabbits scream piteously, and exhibit the various signs of agitation characteristic of intense pain if their toes are pinched, or any sensory nerve severely stimulated." And Professor Starling, in his "Elements of Human Physiology," though not dealing with it from the psychological point—

7413. (*Chairman.*) We are getting rather a long way from the frog in the second experiment?—It all bears upon the question.

7414. I think it is getting rather far from that question, because I presume there is no doubt that physiologists would agree that dogs and rabbits come under the Act?—Yes.

7415. And any operation of taking out their brains would be something done under the Act?—May I just bring one instance? There is a tendency at present, just because anæsthetics are so distasteful to vivisectors, which I have already referred to, to perform experiments on animals that have been deprived of their brains; and the vivisectors would then consider that those experiments are absolutely painless, and they would no doubt be performed under licence alone—in other words, counted as equal to employing an anæsthetic. Thus Dr. T. G. Brodie and Miss Winifred C. Cullis performed some experiments on the secretion of urine (they are described in the "Journal of Physiology" of May, 1906), and they say: "We therefore" (that is because anæsthetics were for some reason not suitable) "worked upon animals (dogs) which have been killed by pithing." Pithing there does not include, evidently, the destruction of the spinal cord; because I admit that an animal that has had its brain and spinal cord destroyed is incapable of feeling pain; but my point is that it is

not proved that animals which only have had the cerebral hemispheres removed are incapable of feeling pain. Dr. Brodie, after saying first, "We therefore worked on animals which have been killed by pithing," continues: "Such animals recommence to breathe within a few minutes. . . . Such animals survive in this condition for some four to eight hours." As they had been "killed" in this case, that expression at least seems somewhat incongruous.

7416. (*Colonel Lockwood.*) What page is this?—"Journal of Physiology," Volume XXXIV., number 3, 1906, page 234.

7417. Have you got it here?—Unfortunately I have not brought it, but I can send it to the Commission. I believe that Miss Winifred C. Cullis, who was demonstrator of physiology at the London School of Medicine when I attended lectures there, held no licence; and I should think that if she holds no licence this experiment would simply be counted at present as an experiment not within the Act, because the dogs are supposed to have been killed. Yet they survived for from four to eight hours.

7418. (*Chairman.*) Were experiments tried upon the dogs during those four or five hours?—Yes.

7419. (*Sir William Church.*) Would you tell the Commission how they were enabled to carry on a certain amount of vital action?—They were enabled because according to Goltz and other observers a dog can be kept alive for 18 months without its brain.

7420. But how were these particular dogs that were experimented upon kept so that the vital action could go on during the four to six hours during which the experiments were done?—I think that vital action goes on perfectly if the cerebral hemispheres have been removed.

7421. This has nothing to do with the cerebral hemispheres. You tell us that these dogs were pithed, and then Professor Brodie and Miss Cullis carried out some experiments during four or six hours, I think you said, on the urine?—Yes.

7422. Would you tell the Commission how these dogs were enabled to go on performing certain vital functions?—I do not think anything is said in that chapter as to how they were enabled to do so.

7423. You have not tried to inform yourself of the reason why these dogs differed from any other dog that is pithed and then kept alive. Are you not aware that pithing is an immediately fatal operation in an animal if you leave it alone. You understand what pithing is?—Yes, of course.

7424. Cutting across the spinal cord?—No, the cutting may not be across the spinal cord. That is just another point that I wish to bring out before the Commission; that pithing is a word which covers so very many things. Real pithing, I maintain, means destruction of the cerebral hemispheres and the spinal cord.

7425. Will you tell us what it meant here in this particular case; that is what we want to know?

7426. (*Sir Mackenzie Chalmers.*) Do you know how this dog was pithed?—No information is given as to how it was pithed, and I cannot tell the Commission. I used the words which were used by Dr. Brodie and Miss Cullis.

7427. (*Chairman.*) Does Dr. Brodie say the dog was pithed?—He uses the word "pithed" exactly as I read it. What I wanted especially to bring out was that pithing is used for a great many different things. It is used by some vivisectors and physiologists in the sense, which I believe is the accurate and proper sense, of the total destruction of the brain and spinal cord. It is used by others merely to convey destruction of the cerebral hemispheres.

7428. (*Sir William Church.*) Would you kindly tell us the different physiologists who use the term in those different senses?—I can if you will allow me time to look it up in the books which I have brought.

7429. I do not mean now, but will you send it to us afterwards?—Yes; I thought of preparing a list of them, and then I thought it would not be required.

7430. (*Chairman.*) Perhaps you will give us a note of it later on?—Yes; and I will also look up that par-

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* See "Zoophilist" of July, 1897, and June, 1902.—L. L.-af-H.

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ticular point as to Dr. Brodie and Miss Cullis's special form of pithing here. I will endeavour to get it for the Commission.

7431. You mean what is referred to in the chapter on painless experiments?—Yes.

7432. Then the next chapter is: "An Experimental Production of Blood Clotting by Injection of a Nucleoproteid." The suggestion there, I think is that the rabbit which was being operated upon gave signs which satisfied you that it was not being adequately anaesthetised?—Yes, Mr. Thane, the Inspector under the Act of 1876, has referred to this chapter before the Commission, and he has characterised our suggestion contained in this sentence: "And would not the tetanic spasms that followed counteract the effect of a possible anaesthesia?" as ridiculous; and he then goes on to quote from a paper by Professor Halliburton, published in the "Journal of Physiology," Volume XVIII., page 138, an account of the very same processes, though couched in more scientific, and, of course, non-sympathetic language. What we call struggles is there interpreted as "slight stretching movements." We say "The eyes become bloodshot and start from their sockets"; whilst the scientific terms are: "Extreme exophthalmos and dilation of the pupils." We describe that the animal fell back dead, as it was supposed, and that it resumed its struggles after having received a stimulus; whilst the scientific paper admits the very same thing by saying that "sometimes after ceasing to breathe for a minute, the animal gives a few breaths more, and sometimes it resumes breathing normally." If Mr. Thane had taken the trouble to quote other paragraphs from the "Journal of Physiology," describing similar experiments with blood-clotting by Professor Halliburton,* he would have found the phenomena which we describe and insist on describing as struggles, referred to as "pronounced stretching movements of the fore and hind limbs which occur immediately before the death of the animal"; and in the case of a large black dog, weighing 30 lbs., experimented upon at 4.44, the result was stretching movements and spasms. The struggling movements are referred to as becoming more pronounced at 4.57, whilst at 5 o'clock an additional injection resulted in extreme stretching movements. An absolute line of divergence between extreme stretching movements and struggles might be difficult to find. The spasms described by Professor Halliburton in his paper tally with the spasms described by us. Mr. Thane considers the suggestion that tetanic spasms might counteract the effect of a possible anaesthesia ridiculous. I maintain that it is not impossible. Our suggestion is intended to convey that the presence of convulsions indicated that the animal was not under the influence of an anaesthetic. The fact that anaesthetics are constantly used as anti-spasmodics and known to allay convulsions generally, indicates that deep anaesthesia and convulsions are two incompatible conditions. In the "British Medical Journal" for June 29th, 1901, there is a description of an experimental inquiry into the pathology of gastric tetany by Professor Halliburton and Dr. McKendrick. Tetany fluid, prepared from the vomit of a patient suffering from gastric tetany, was injected into a cat under anaesthesia, and the writers remark that "no spasms or convulsions were produced; whether these would have occurred had not the animal been anaesthetised it is impossible to say." These words show that Professor Halliburton does not consider it impossible that the anaesthesia was responsible for a suppression of spasms, which leaves room for the counter suggestion.

7433. No doubt questions giving an opportunity of making that statement would have been asked you by other members of the Commission. I think perhaps as you have told us that you adhere to your accounts of these experiments, that will be the shortest way of dealing with them. I put the question to you in that way so that there should be no mistake as to what is meant by them. That deals with the experiments in the chapters beginning with "painless experiments," and ending with "a grand demonstration"—that is the last of the pure descriptions of experiments. You say that those accounts are correct?—Yes.

7434. Then I think I will not ask you any further questions, but other members of the Commission may desire to do so, and if, when they have asked them, there is something that you wish to add in explana-

tion you will have an opportunity of doing so?—If you please.

7435. (Colonel Lockwood.) You maintain that the quotations given in the books you read tend to show that science, and not humanity, are inculcated in the physiological books of which you have given us extracts?—Yes, that is what I intend to convey.

7436. That is what you gave us those extracts for?—Yes.

7437. Have you any reason to believe, from personal knowledge, that the law as it at present exists is evaded in the laboratories?—Of course, several of the accounts in the "Shambles of Science" would tend to show that the law, if not evaded, as it stands admits of cruelty.

7438. We have had some conversation about light anaesthesia, and you read from some books on the subject of light anaesthesia as, I suppose, opposed to complete anaesthesia. But would you acknowledge that light anaesthesia may produce complete insensibility as well as deep anaesthesia?—I would not acknowledge it, and I do not believe that surgeons as a rule operate under light anaesthesia. There were some very interesting letters on that subject, published some time ago (I have the references if they should be required) to the effect that surgeons as a rule in our great hospitals do not operate under light anaesthesia; and one surgeon in particular, Mr. Edmund Owen, in referring to a certain number of operations which, from their nature, cannot be performed under deep surgical anaesthesia—that is operations on the throat, on the palate, and certain other operations—ends his letter to the "Times" by saying: "Why should it be necessary to spread out these hideous things before the public gaze?" (March 15th, 1902). And another of these surgeons said that operations under incomplete anaesthesia would be "as embarrassing to the operator as painful to the patient."

7439. If done without anaesthetics?—If done under light, incomplete anaesthesia.

7440. Therefore, what you maintain is that operators on mankind would never use light anaesthesia; they would always work under complete anaesthesia?—They would always endeavour to get complete anaesthesia except for certain operations where such anaesthesia would be dangerous.

7441. But they are not averse to using light anaesthesia on animals from the fear that their experiment might be spoiled if they used complete anaesthesia? That was your point, was it not?—Yes, that is my point.

7442. You have seen some operations on animals?—Yes.

7443. Have you ever seen a distinct breach of the law that you could point out to us?—Of course, I do not know if you can go over the whole trial again, but there was, in my opinion, a distinct breach of the law.

7444. As stated in the "Shambles of Science"?—As stated in the "Shambles of Science."

7445. (Chairman.) The question that was asked was whether you had been present when any such operation had been done?—My answer to that must be, that I have been present at several experiments at which the anaesthesia was not complete or satisfactory from a humane point of view; and then I must leave it to the Commission to infer whether that is a breach of the law or not; I have not taken it from that point of view.

7446. (Colonel Lockwood.) Could you verify the names of the operators?—I can give the names of all the operators.

7447. Where you saw animals which you believed were not under complete or satisfactory anaesthesia?—I can give you an instance.

7448. Can you give us the reference?—That of "A Troublesome Dog."

7449. You give that as one instance?—I give that as one.

7450. Who was the operator?—Dr. T. G. Brodie, assisted by Dr. Alcock.

7451. And have you any other?—All the experiments in Professor Starling's laboratory—that is, all the ex-

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periments performed by Mr. Bayliss, did, in my opinion, give very little guarantee that the animals were insensible, especially the one of which an account is given under the title of "The Struggling Cat."

7452. Who performed that operation?—Dr. Bayliss, in Professor Starling's laboratory.

7453. In "A Long Experiment," is that one you witnessed yourself?—Yes, all these I witnessed myself.

7454. And in all those, or most of them, you believe that the animal was sensible to pain of some sort?—More or less. I believe, to make a general statement, that the anaesthesia was not satisfactory from the humane point of view. And there were certain circumstances connected with the experiments which led me to infer that—notably, in some cases the omission of the constant renewal of the anaesthetic, which always has to be done; and in other cases the admission of the lecturer or demonstrator that morphia had been given, morphia and a little chloroform; and in other cases the behaviour of the animal, as, for instance, "The Struggling Cat," or "The Troublesome Dog."

7455. May I take it that you object to experiments on living animals altogether (it is only with a view to eliciting your opinion); or that under complete anaesthesia, if you believed the object was necessary to save human life, you would not have objected?—I object on principle to all experiments on living animals, and I do not believe they could be useful in saving life.

7456. You object to all experiments on living animals?—Yes.

7457. Whether anaesthetised or not?—Yes.

7458. As demoralising to the man who operates and to the public who see it?—Yes.

7459. Supposing that the law is continued in some shape or other, could you assist us by any suggestion beyond the suggestion you have made, that opium should never be used as an anaesthetic because you do not think it is one? Could you give us any suggestion as to alterations in the law that would mitigate any sufferings that might happen under the present law?—As a first alteration of the law I should certainly suggest the prohibition of demonstration experiments before students, as being, to my mind, abolition piecemeal, which would take away a very important part of vivisection, and would certainly do very much to help the anti-vivisection cause. Then, though I am not in favour of the exemption of one animal and the leaving out of others, I should say that all experiments on dogs, cats, monkeys, and all the higher animals are especially objectionable, and especially experiments on monkeys. I have a monkey myself, and have personal experience of its extreme intelligence and sensitiveness.

7460. From your experience of what has gone on and what you have read, do you believe it would not be hostile to science if those higher animals were left out?—I think it would be very much objected to by the representatives of science.

7461. That would be another point. Do you believe that it would not be dangerous to science?—I do. I believe that all experiments on living animals could very well be dispensed with by science.

7462. And would you suggest any further safeguarding of animals by way of more inspection?—No, I would not suggest more inspection.

7463. You think that more inspection would be useless?—I think that inspection would perpetuate the practice.

7464. Therefore the addition of inspectors, or surprise visits, or more numerous inspections, would all be useless from your point of view?—Yes. Of course, I think the present inspection an utter failure; but I do not feel justified in saying that I should agree to any steps for further inspection.

7465. It would not help your cause a bit more?—No.

7466. (Chairman.) When you say that you are against all operations, does that include operations for testing the effect or the safe dose of new drugs on animals?—Yes, it does; it includes all inoculations and testing experiments; because I am convinced, in fact, I know, from having seen the effects of such inoculations at the Pasteur Institute in Paris, what suffering they involve. It is not only the prick of a pin.

7467. (Sir William Church.) Would you kindly tell

me what is the date of, I think it was Dr. Halliburton's textbook, which you referred to in connection with your "Dog Injection with a Substance Derived from a Lunatic"?—I referred just now to the "Journal of Physiology." I did not refer to any text book.

7468. I think you quoted Dr. Halliburton as proving that very little was known with regard to the action of choline?—I will give the exact words.

7469. I only want the date of the book?—It is the seventh edition of Professor Halliburton's "Handbook of Physiology," formerly Kirkes' "Physiology," published in 1905.

7470. That is the edition you quoted from?—Yes.

7471. That is what I wanted to know. Thank you?—I have found the quotation. It is page 177.

7472. I do not think I need trouble you to read it again. I think you thought that his experiments which he had performed on cholin were of no practical value at all?—I quoted his own words to show that he himself found so.

7473. I do not think he quite says that?—He says: "It is possible that such tests may be of diagnostic value in the distinction between organic and so-called functional diseases of the nervous system"; and I quoted that experiment as one that could not properly be defined as absolutely necessary, because, for one thing, it has no therapeutic value, its diagnostic value is doubtful, according to Professor Halliburton himself, and its physiological value is not absolute, because on this very page Professor Halliburton himself maintains that cholin could just as well be tested by a purely chemical test. These are his words: "The tests employed to detect cholin are mainly two: (1) A chemical test, namely, the obtaining of the characteristic yellow octahedral crystals of the platinum double salt from the alcoholic extract of the cerebrospinal fluid of blood; (2) a physiological test, namely, the lowering of arterial blood-pressure (partly cardiac in origin, and partly due to dilatation of peripheral vessels), which a saline solution of the residue of the alcoholic extract produces."

7474. (Chairman.) When you said "absolutely necessary" just now, you meant for the purposes of demonstration?—Yes, that was my first point.

7475. (Sir John McFadyen.) I thought you said a minute ago that Professor Halliburton himself alleged that these tests were of equal value? That does not follow from what you read; they might both be tests, and one might be useful in certain circumstances, and the other might not, and one might be better than the other?—He does not say anything about it.

7476. But I think you used language which implied that Professor Halliburton said that they were of equal value; that one could be used in place of the other?—The words of Professor Halliburton imply that they are of equal value. He says there are two ways of testing cholin, (1) and (2); and I maintain that when there is a chemical test which would be equally useful, it is not necessary to include the physiological demonstration.

7477. (Sir William Church.) This lecture that you attended was at the University of London?—This particular lecture was at the University of London.

7478. But it was for advanced students, was it not, not for ordinary medical students?—It was for ordinary medical students; all the medical students from the London School of Medicine for Women who wanted to go to these lectures could go. Any medical student can attend them. They are called advanced classes.

7479. They have to apply for admission?—Yes, you have to apply. I think any student under the University of London is allowed to go. We applied for tickets from the Principal of the London University.

7480. (Colonel Lockwood.) What is the course called?—It is called "Advanced courses of physiology, open to all students within the University of London."

7481. (Sir William Church.) But it is an advanced course for students?—Yes, it is called an advanced course.

7482. Would you look at page 76 of your book. You say: "The lecturer tells us that the animal is anaesthetised with morphia and some chloroform, and adds: 'These unusual circumstances under which the experiment is performed will perhaps interfere with

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the results.' Was it the morphia and chloroform which constituted the unusual circumstances? What do you imply by that?—We imply simply the question. We ask whether Professor Halliburton meant that the unusual circumstances were the chloroform and morphia. We imply nothing more than the question.

7483. Then why do you put in the book: "Was it the morphia and chloroform which constituted the unusual circumstances?"—That is just what we did not know. That is why we put it as a question. We did not put it as a statement; we put it as a question.

7484. (Sir Mackenzie Chalmers.) A question with a meaning attached to it, though?—Perhaps.

7485. (Colonel Lockwood.) That is the experiment with a substance derived from a lunatic?—Yes.

7486. (Sir William Church.) Did you not gather that it was an experiment which he thought was better performed under morphia and chloroform than under A.C.E., or chloroform alone?—The lecturer said nothing about it. He told us that it was performed under morphia and chloroform, but he gave no reasons why he had chosen those two drugs.

7487. (Sir Mackenzie Chalmers.) Might not the quotation have this meaning: that it was unusual for him to give both morphin and chloroform, as he usually only gave chloroform or the A.C.E. mixture?—It might mean that. Of course, I cannot commit myself to say what he meant. It is merely put as a question.

7488. But still it might mean that?—It might. It might also mean that on other occasions only morphia was used.

7489. And if what I suggest was his explanation, should you accept it?—Yes, I think I should accept it.

7490. (Sir William Church.) You know that experimentation with cholin is a new thing; the action of cholin has only lately been found out?—Yes.

7491. That you were aware of when you went to the lecture?—But I think that things which are just found out should not be tried before students immediately.

7492. I am not speaking of students; but you were aware of that?—Yes, that was just why I urged that it is not a necessary experiment, because it was so new.

7493. But you do not suggest that this demonstration is one which would be performed before ordinary medical students?—Yes, the medical students who attended those lectures were ordinary students.

7494. You make no difference between an ordinary medical student who has to attend a certain number of physiological lectures and an advanced student?—No, I think any medical student had the right to attend those lectures.

7495. The demonstration was given specially, was it not, to advanced students; it was headed "Demonstration for Advanced Students"?—When we came to attend these lectures, the conditions were these: There were placards in the London School of Medicine for Women that these courses were given in various places, saying where you could obtain tickets, and anybody who applied was admitted. We applied in that way, and got our tickets, and were admitted.

7496. The chemical test that you have alluded to is a difficult one, is it not?—I have never seen the chemical test. I should not think it is more difficult than many others.

7497. The physiological test is more easily obtained—the effect of cholin on blood-pressure?—I do not know whether it is more easy to try it on a live dog than to try it in a chemical test tube. I should say that the chemical test tube ought to be nearer at hand.

7498. Are you aware whether it has been found that, as the result of the researches that have led to the discovery of cholin and its action, it has ever been used practically or not?—I believe that the discovery of cholin has a certain practical value in the treatment of nervous disease, but I am unable to see that the injection of cholin into live dogs is of any value whatever, because it has no therapeutic value; it is merely a product of disease.

7499. Was not the object of the experiment to show the action of cholin upon blood pressure?—Yes.

7500. And has it been found already of much use,

as Dr. Halliburton says it may be found? I wanted to know whether you were acquainted with any of the investigations which have been made in it from a practical point of view?—No, I am not acquainted with any use that has been derived out of experiments on dogs with cholin.

7501. You are not aware that it has been already used to distinguish between cases in which there is nerve degeneration, and cases in which no degeneration is present?—That I am quite aware of. That is why I said a minute ago that the discovery of cholin would undoubtedly be of great value as a means of distinguishing between various different diseases. But I say again that that does not touch the point of injecting this product of disease into a live dog. That has no value whatever, so far as I can see.

7502. How are you to investigate it and find out what the effect of cholin is and whether it is prevalent in the blood of the patient, unless by experiment?—By the chemical test which Professor Halliburton refers to as one method.

7503. The chemical test is a more difficult one?—I do not think it matters if it is more difficult; chemical experiments are generally difficult. I do not think it is more difficult than others.

7504. Your impression was that the animal was not fully anaesthetised in the case which you describe?—I do not think we make any suggestion that the animal was under insufficient anaesthesia. The suggestion is made that the animal was killed in a very inconsiderate way.

7505. I should like to ask you a question or two about how it was killed afterwards. But I drew myself, perhaps quite incorrectly, the inference from your question at page 76: "Was it the morphia and chloroform which constituted 'the unusual circumstances'?" that you were rather under the impression that morphia having been used, the dog was not under full anaesthesia?—The inference there is whether the use of morphia and chloroform constituted unusual circumstances. The inference seems rather to be what are the usual circumstances, what happened on other occasions.

7506. Then you do not doubt that this dog was under complete anaesthesia?—I cannot say that we were satisfied, but there was nothing special during that demonstration to suggest any bad form of light anaesthesia.

7507. You thought that the method of killing was improper?—The method of killing is really what is mostly criticised in this chapter besides the nature of the experiment—that the assisting demonstrator did not even take the trouble to see that the animal was really dead.

7508. But surely he did. Would you tell us what happened?—It was exactly as it is described in this book.

7509. (Sir Mackenzie Chalmers.) Would you describe it in your own language?—The demonstrator attached a glass tube to the opened jugular vein, and first we were told that the animal was to be killed. Then he blew a little air into the jugular vein and then sat down, so far as I remember, among the audience, and the lecture went on, and the animal was supposed to be dead; but after a minute or two the dog began to struggle and to show signs that it was not dead, and for some time nobody observed it.

7510. (Sir William Church.) In what way did it show signs that it was not dead?—By struggling. "It has begun to struggle, and seems to be in great pain." "At last the lecturer happens to look at the 'dead' dog. He says something to the assistant demonstrator, who leisurely rises from his seat and strolls up to the animal." The suggestion there is rather that the assistant demonstrator ought to have got very upset over his bad killing, and should have rushed to the animal and killed it quickly.

7511. (Chairman.) You said in your book "strolls" to the animal?—Yes.

7512. You say now that he "rushed"?—The suggestion we make is that he should have been in a greater hurry than he was.

7513. (Sir William Church.) You know that blowing air into the vein is a way of killing animals?—Yes, I know that; I have seen it stated somewhere that it has

to be done very carefully—that different animals require a different amount of air.

7514. You do not suggest that blowing air into its veins enabled this animal to feel pain?—No, not that in itself.

7515. It was really that the assistant demonstrator was not successful in his first blow?—And also that he did not care sufficiently that he was not successful; that when the animal struggled his attention was not so directed towards the animal—that he did not notice it at first, but it took some time, and that that implied a certain amount of carelessness.

7516. How long a time elapsed between these two blows, do you think—the first blow and the second?—That I could not possibly say at this time; it may have been three minutes, it may have been four; it may have been five minutes.

7517. You do not remember?—I could not remember now.

7518. Would it be five minutes?—At the utmost five minutes, I should say. It may have been three or four minutes.

7519. Do you think that the struggling of the little dog indicated that it was coming out of its anaesthesia?—Yes, I think it did.

7520. That is why you think it was careless that it was not watched?—Yes, the whole attitude of the lecturer and the demonstrator I think indicated carelessness.

7521. You are quite sure that these struggles did not follow the second blow of air into the veins, that they were before?—They were before.

7522. Did any follow the air being blown into the veins?—They followed the first blowing into the veins.

7523. But the second?—We do not say that he blew in air a second time. That it is not said. It is said that he resorted to his scalpel. I do not think there is anything said about a second blowing in of air.

(Chairman.) No, you say that the scalpel was used?

7524. (Sir William Church.) What I asked was when the struggles took place; whether they took place before or after the second blowing of air into the veins?—They took place after the first blowing; there was no second blowing, so they could not take place after that. They took place after the first blowing.

7525. (Sir John MacFadyen.) But you say that he repeated it?—No. We say "In order to stop any further pranks played by this obstinate life, he resorts to his scalpel."

7526. I beg your pardon; I was reading another passage to you: "He repeats it and then sits down among the audience." I take it that you describe that as one operation; but, nevertheless, you say that he repeated the operation of blowing into the veins?—But that happened before he sat down among the audience. Those were done whilst he was still standing by the dog. I did not count that as two blowings. I understood that the question was whether the demonstrator first blew the air into the animal and then sat down, and then went and blew in air again, and then afterwards resorted to the scalpel.

7527. You count the first two as one?—Yes.

7528. (Sir William Church.) I did not understand that before. Are you quite sure that these struggles which you thought meant returning to consciousness, were not the effect of the air being blown into the vein?—I do not know whether the effect of blowing air into the vein is struggling.

7529. You had never seen it done before?—I had never seen it done before, but I have never heard or read anything to that effect.

7530. You are not aware that struggles and convulsions are the way in which death generally takes place in these cases where air is blown into the veins?—Even if it were so it is no proof that the animal may not have been returning to consciousness.

7531. I am not saying whether it is proof of that or not; I suggest that the explanation of the struggles and convulsions may have been the blowing of air into the veins?—I am perfectly sure that in a deeply-anæsthetised animal the introduction of air into the vascular system would not result in struggles.

7532. What makes you perfectly sure?—The general behaviour of an animal that is under deep anaesthesia—the totally limp condition and absence of struggles.

7533. Have you ever worked at the question yourself? I do not know how far you worked in physiology; have you any practical knowledge? You say that you have never before seen air blown into the vein to kill an animal. Now you tell us that you are quite sure that if an animal is properly narcotised or anaesthetised it could not die with struggles and convulsions?—Yes, I am perfectly sure of it.

7534. Have you any reason for that certainty?—I have the reason of my general knowledge of the effects of anaesthesia. I have never seen, of course, for instance, air blown into the vascular system of a human being, but it is my general knowledge of the demeanour of the patient, whether animal or man, under anaesthesia, which makes me believe that if an animal is properly anaesthetised, it does not struggle when air is blown into the jugular vein.

7535. But may not your inference be incorrect; you are only giving us your inference?—Yes.

7536. Can you give us any facts with regard to it?—I daresay I could get some facts with regard to it.

7537. We are told—at least, I have been told; I may have been told incorrectly—that convulsions and struggles are the way that death occurs in these cases where air is blown into the vein, whether the animals are narcotised or not?—Nothing of the sort is said in this case about the animal coming out of the anaesthesia. It is simply said, "It has begun to struggle, and seems to be in great pain."

7538. (Sir Mackenzie Chalmers.) "Seems to be in great pain"—that is inconsistent with anaesthesia?—It is.

7539. (Sir William Collins.) Have you seen certain observations on the "Shambles of Science," prepared by Dr. Thane, and dated December, 1903, which have been supplied to the Commission?—No, I have not. I shall be exceedingly glad to see them.

7540. (Chairman.) You will understand that this was a confidential report made by Dr. Thane to the Home Office after the publication of your book. The confidential document has been handed to us, and you will be examined upon it; and it is simply because questions will be asked upon it that it can be handed to you for your convenience; but it is not handed to you for any purpose of publication?—I quite understand.

7541. (Sir William Collins.) I notice, in regard to a matter upon which questions have already been put to you about a dog injected with the substance derived from a lunatic, that Mr. Thane states in this paper: "The lecturer gives the following account of the alleged revival of the dog: 'The animal was fully anaesthetised throughout with morphia and chloroform. After the experiment had been successfully performed, I said we should now kill the animal by one of the most rapid methods available—viz., by blowing air into the dog's veins. The vein already previously exposed for the administration of the drug was used, and air blown in by my assistant in the experiment. The first blow was not, as it usually is, successful, because he omitted to clamp the vein when he stopped blowing, and a good deal of the air came back again. So he gave another blow, this time taking the usual precautions to clamp the vein afterwards. The blood pressure fell to zero, the heart ceased to beat, and the dog ceased breathing. This occurred in a few seconds. My assistant returned to his seat, and I said 'The animal is now dead.' Have you any remarks to make upon that?—Yes; that would be the two first blowings, and then the assistant sitting down and Professor Halliburton remarking, 'The animal is dead.' Is there any account of the revival?"

7542. Up to that point, you have no exception to take to the statement?—No, I think it is exactly the same as in "Shambles of Science."

7543. Then he proceeds: "The sudden failure of the circulation and respiration produced the usual asphyxial convulsions. These, which are due to either want of blood or presence of excessively venous blood in the nerve centres, especially in the medulla oblongata, do not indicate any return to consciousness. Fearing, however, that the convulsions might alarm

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some members of the class, I asked my assistant to kill the animal, which he did, and so put a final stop to the movements"?—I think that account is exactly the same, though in different terms, as that given in "The Shambles of Science"—the two blowings, the sitting down, and the movements of the dog supposed to be dead.

7544. You notice that he states that the usual asphyxial convulsions do not indicate any return to consciousness?—Yes, I quite expected that answer to be given.

7545. You think it is a matter of opinion rather than a matter of fact?—Yes, I quite expected that answer.

7546. Would you say that asphyxial convulsions are absent where the narcosis of chloroform has been carried out so as to terminate fatally?—I should think so. I have already quoted some evidence on that point, that, inasmuch as anæsthetics are used so much as antispasmodics and to allay convulsions, I believe that such convulsions are incompatible with perfect anæsthesia, deep anæsthesia.

7547. You are not speaking on that from your own experience?—No.

7548. (Chairman.) When you say you quite expected that answer from Professor Halliburton, do you mean to imply that it was untrue?—I do not mean to suggest that; but I mean to suggest that the point of view of the vivisector is so different from the point of view of the person who looks upon these experiments from the humane point of view, that you can only expect two totally and entirely divergent opinions; it always will be so.

7549. (Sir Mackenzie Chalmers.) You mean that the same phenomena produced two absolutely different views?—Entirely, and they always will.

7550. (Sir William Collins.) In the case of No. 13—"A Grand Demonstration," pages 139 to 148 of your book—I see Mr. Thane, referring to that, says: "The 'struggles' are denied altogether." That appears to be a conflict of fact?—That surprises me, because they were so evident that anybody must have seen them, and if they are denied, I should like to ask why so many efforts were made to anæsthetise these three animals. That is the only reason why the operation is recorded in this book. They failed—constantly failed, showing the great difficulties accordingly—as the chapter ends by saying: "This was an example of careful, complete, and public vivisectional anæsthesia. What about the careless, incomplete, and private?" We looked upon this demonstration as giving an example of real carefulness, so far as carefulness can be practised.

7551. You say that you saw the struggles?—We saw the struggles as they are described in the chapter, "A grand demonstration."

7552. Then as regards page 142, Mr. Thane says: "The dog, it is known, had been fed by the attendant not very long before; it is untrue that it 'had not been kept long enough within the precincts of the physiological laboratory'."—But we say in this chapter that this dog had recently come in, according to the statement of Professor Starling, made before the class. "For this experiment it is best to use a dog in a starved condition," says the lecturer. "This one, however, came in only recently, and his pancreatic juice has been secreted in digestion." Professor Starling said that, thereby suggesting that somebody had made a mistake in feeding this dog. In fact, these experiments are always performed after starvation. In Professor Starling's book, "Elements of Human Physiology," there is a phrase in these words: "If a dog be starved for six successive days and be made to do hard work on the last three, then it will be found that there is no difference in the excretion of urea," showing that starvation is sometimes a scientifically necessary preparation for an experiment. So it was in this case, and Professor Starling merely referred to a thing which is quite usual.

7553. Then you deny that it is untrue that it had not been kept long enough within the precincts of the physiological laboratory?—I deny it, and my denial is based on Professor Starling's words, that he complained that it had not been kept long enough.

7554. As regards page 147, Mr. Thane refers to "the miserable white rabbit." He says the "writer omits to state that it was dead"?—May I ask when it was

suggested that the rabbit was dead? Surely not during the experiment when the intestinal extract was injected.

7555. The only light thrown by Mr. Thane on it was this: "The miserable white rabbit," the writer omits to state that it was dead. At page 145 it was sent out in order that it might be killed by an overdose of ether; and that is what had happened"?—But we do not suggest that the rabbit struggled after it had been carried out. Our description of its struggles is until the moment when it was carried out. Then we say nothing about it until we go out, and we meet the boy carrying the rabbit into the lecture room again, when the rabbit lay quite still. "The little creature lies still on its board." If it be true that it was dead, then I say that there is nothing in the "Shambles of Science" that contradicts that in any way. We say that the animal struggled until it was carried out as described, and then we saw nothing of this rabbit in the laboratory room until we were leaving, when we met the boy carrying in the rabbit again, and then we say: "The little creature lies still on its board, the head is wet, the hairs stick to the skin, the eyes are protruding and look like glass balls."

7556. You think it was superfluous to state that it was dead?—In other words, we met the rabbit in the door as we were going out, and we make that statement there in connection with that particular period, whether it was dead or alive.

7557. (Sir John MacFadyen.) You say, "It lies still"?—Yes, if it was dead it would lie still.

7558. You think that is not calculated to lead the reader to believe that it was still alive?—There can be no wicked suggestion there, because the suggestion would be that it was properly anæsthetised. There can be no harm in that statement.

7559. (Sir William Collins.) At any rate, you thought it was superfluous to state that it was dead?—We had no opportunity of seeing. The animal was carried in again when we were going out, and we saw that it was lying still—which shows how accurately we observed it if it was really dead.

7560. Then on page 9 of his report, Mr. Thane, referring generally to your work, says: "Two suggestions run through this work, expressed either openly or by implication. One is that the animals are not properly anæsthetised. This, in the only case in which it has been possible to test the question, has been proved emphatically to be untrue. I believe there is no better foundation for the insinuation in the other cases. Of the 13 lectures dealt with, eight took place at the University of London. Dr. Waller, the director of that Laboratory, is, one may say, an expert, probably the first in the country, on the physiology of anæsthetics. He has given much attention to this subject, and for the last two or three years has devoted, I believe, his whole time to the study of the action of anæsthetics on the nervous system." Is there any remark that you wish to make on that passage?—May I say, in answer to that, that I entirely and utterly disagree with Mr. Thane there; that in the case of the brown dog, to which he presumably refers, it was proved that the anæsthesia had been satisfactory; and, secondly, as the brown dog has been brought in so much before the Commission, I should like to be allowed to make a statement in answer to what I have seen in the printed evidence of Mr. Thane.

7561. (Chairman.) You refer to the evidence given before the Commission?—Yes, evidence given before the Commission.

7562. You can refer to anything which has been already put before us by a witness.

7563. (Sir Mackenzie Chalmers.) Was the brown dog the case in Bayliss v. Coleridge?—Yes.

7564. The jury heard your evidence on that case, did they not?—Yes, they heard my evidence.

7565. And they did not agree with your conclusion?—No, they did not.

7566. (Chairman.) You are not going to read your evidence given at the trial?—No, not at all.

(Sir Mackenzie Chalmers.) It is the same case?

(Chairman.) It is the same case, but the evidence will be on facts which have been brought before us.

(*Sir William Collins.*) And it is dealt with in the paper by Mr. Thane.

(*Witness.*) I have not put down a single word in the statement I have here in connection with the trial on the brown dog which is not absolutely essential to my answer to the statements which have been made by Mr. Thane and Professor Starling concerning that brown dog, and I therefore ask permission to give the statement.

7567. Will you proceed?—I now come to the evidence which I wish to give in immediate connection with the "Shambles of Science," and with special reference to the statements which have been made before this Commission. The book was published July 10th, 1903, by Mr. Ernest Bell. During a period of barely four months there were more than 200 reviews and notices in daily papers and journals of various kinds, and the book attracted a great deal of attention. Had the book been libellous, this period afforded ample opportunity for action. Previous to the publication of the book, Mr. Stephen Coleridge had asked us to give him a statement describing a certain demonstration at which we had been present. This was in illustration of a lecture by Dr. W. M. Bayliss at University College, on February 2nd, 1903.

7568. (*Chairman.*) Is not this going into the case at the trial?—No, it is not; it is only going into the case of the trial so far as it has been brought before the Commission. Certain facts have been stated by Mr. Thane that the book had been withdrawn.

7569. (*Sir William Collins.*) Which answers of Mr. Thane are you referring to?—The brown dog has been brought up before the Commission, I believe, two or three times, if not four times, already. It begins at Questions 1261 to 1283, including 1282.

7570. I see at Question 1261 Mr. Thane refers to a report which he made to the Home Office, for which he was thanked?—Yes.

7571. That is the same report from which I have been reading extracts to you?—That brown dog has been brought before the Commission in answer to Question 1551, and there are a great many questions up to No. 1569.

7572. (*Chairman.*) I am quite aware that it has been referred to, but I do not quite see how your repeating the statement that you made to Mr. Coleridge has anything to do with it?—It is essential that I should mention it, because I wish to prove that there was no connection between the trial and the book. The book was not libellous. It has been stated that the book was withdrawn. Mr. Thane said at Question 1276: "May I add that the 'Shambles of Science' has been withdrawn from publication?" (*Dr. Wilson.*) Only a particular chapter, I think? (*A.*) No, the whole book is withdrawn, I believe. (*Dr. Gaskell.*) I was told yesterday that a new edition had just come out, and was seen in a shop? (*A.*) Then that is quite relevant. But I should like to look at the letters that were sent in; because my recollection is that a letter was sent from the publishers saying that it was withdrawn." Then follows the whole letter, in which Mr. Bell promised to withdraw the book. As that has been brought before the Commission, I thought I might be allowed in a few words to refer to it, and to show fully and accurately what occurred.

7573. (*Sir Mackenzie Chalmers.*) Yes?—It is not very long; it is a very short account.

7574. (*Chairman.*) Well, we will see?—This was in illustration of a lecture by Dr. W. M. Bayliss at University College, on February 2nd, 1903. The animal experimented upon was a brown dog, which attracted our attention in a special way because it bore marks of previous vivisections, and because of its movements upon the operating board. It was afterwards admitted that we had been present at the third vivisection of this dog, and that the animal had moved, though the movements were declared by Mr. Bayliss and Mr. Starling to be due to a special disease and not to consciousness. Mr. Coleridge read the statement which we had given to him at a large meeting in St. James's Hall in May, and as a result of this Mr. Bayliss instituted proceedings for libel. During the trial, which took place in November, 1903, the connection between that statement and the chapter "Fun," in the "Shambles of Science," became established, and in consequence of this Messrs. Hempson, solicitors to Mr. Bayliss, threatened Mr. Bell with legal proceedings unless he signed an undertaking to withdraw all copies from

circulation, and not to print or publish any further copies. Without committing myself to any criticism of Mr. Bell's action, I wish to point out that we gave no undertaking to withdraw the book from circulation, that we in no way concurred in Mr. Bell's decision, and that the rest of the chapters of the book could in no way be affected by the result of the Bayliss v. Coleridge trial. This, our opinion, was strengthened by a remark made by the Lord Chief Justice during the trial. He said, referring to the "Shambles of Science": "This book is not libellous."

7575. I have never found any report of that taken by itself. Do you mean to say that he said there was nothing in the book that was libellous?—He said, referring to the "Shambles of Science": "This book is not libellous."

7576. Has that been reported?—It has not been reported, but at the time there were so many people who had heard the statement that a special list was circulated and signed by a number of people who had heard it. The reason why it was omitted from the report I do not know.

7577. I am quite sure it was never said.

(*Secretary.*) It is not in the transcript.

7578. (*Chairman.*) I am quite sure that the Lord Chief Justice never said it?—I know that he said it.

7579. If he said it, he said it with qualifications, and in a context which could not give it the meaning which you are giving it now?—He absolutely said those words. I have brought with me the list signed by a number of people who heard it.

7580. I do not care if a thousand people signed it; I should say that it had not got his authority. I am quite sure that the Lord Chief Justice never would confirm it?—I am sure that he said it.

7581. He says in the course of the trial that it is an undoubted libel to speak of a person having committed an illegal act?—He could not say that, because the book itself is not libellous, and cannot be libellous, because no names are mentioned. How could it be libellous when no names are mentioned?

7582. You do not understand enough law, I am afraid, to know that it is not necessary to mention persons' names to constitute a libel?—May I just say that had the book been libellous there was plenty of time to institute actions of libel against us. The book was out for four months.

7583. We are not here to try the question whether the book is libellous or not. What you are entitled to say is that inasmuch as it is made a point against you that you have withdrawn your statements because Mr. Bell withdrew that chapter from the book, that was not done by your wish or authority?—That is all I beg leave to say.

7584. You must confine yourself to that?—I am obliged to answer your questions, my Lord. I did not start this.

7585. You read a long statement?—I am only answering the statements that have been made. If there is anything which goes beyond that in this statement, I shall at once stop reading; but to the best of my belief I have not put anything in which is not absolutely necessary in answer to the statements that have been made.

7586. All I wish you to do is to confine yourself simply to that point. I quite understand that you say that the withdrawal of that chapter was a thing with which you had nothing to do, and which you never authorised?—Shortly after the withdrawal of the book an address of confidence was presented to us —

7587. This is going beyond?—This is just to show how the book came out again.

7588. We must proceed on some regular system?—May I just put this to you? It has been said that the book has been withdrawn, in other words, that the book does not exist any more because it was a libellous production.

7589. We know that the book has been printed since—that a new edition has been printed with your authority?—That is all I wish to get clear.

7590. We have got all that before us?—May I now proceed with my direct answer? Professor Starling has said in his evidence, at No. 3545, that we gained admission to the London School of Medicine for

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Women by stating that we "wished to take up medicine as a serious pursuit." This is untrue and manifestly absurd; for, as Professor Starling should know, one cannot study medicine by merely taking up the subject of physiology, and we made no application for any all-round medical training. Mr. Thane has told this Commission that we have misrepresented the question of anaesthesia in our book. I beg to submit that we have stated nothing which is not amply borne out by other records and experiences of vivisectional anaesthesia, some instances of which I have laid before the Commission. Out of 12 chapters which contain descriptions of various experiments, two record attempts to produce satisfactory anaesthesia; three describe experiments under morphia and a little chloroform, which took place at the Physiological Laboratory, University of London. Of these animals, one is described as being quiet, and one as giving a good deal of trouble by struggling and whining, the third as struggling after it was supposed to have been killed by air blown into the jugular vein. The descriptions of three other experiments which took place at Professor Starling's laboratory, University College, contain no such record, for the lecturer, Mr. Bayliss, gave the students no instructions as to the existence or conditions of anaesthesia. During the trial of Bayliss v. Coleridge, he admitted that it was not his practice to tell the students about the methods of anaesthetising the animals (2nd day, 354). During one of those demonstrations, that described under the title "The Dog that Escaped," Mr. Bayliss stated that the animal was slightly curarised. Professor Starling has already answered questions on this chapter, and I must point out that he grossly misquoted it, and then characterised his own misquotation as "wilful deception."

7591. (*Sir William Collins.*) What is the number of the question?—4278. These are his words:—"The writers say: 'If the dog was under curare, if it had artificial respiration.'" Whereas anyone who takes the trouble to read the chapter can see that we did not use the suggestive "if" in either case, but, on the contrary, stated definitely, on the authority of Mr. Bayliss himself, that the dog had had a dose of curare, and, according to our own observation, that no artificial respiration had been established which was the cause of the early death of this particular victim.

7592. What is the page of your book?—That is called "The Dog that Escaped," page 67 in the first edition.

7593. Where are the references to "If the dog was under curare," which you say Professor Starling misquotes?—Professor Starling says that we said, "if the dog was under curare."

7594. What is the page in your book?—There is no such page; that is just my point. He entirely misquotes it.

7595. You mean that it does not occur in your book?—Yes.

7596. (*Sir Mackenzie Chalmers.*) That he has imagined the quotation?—Yes.

7597. (*Chairman.*) You say that he is misquoting some particular passage; what is the passage?—This is the passage, on page 68: "He struggles as much as he can till the lecturer injects something into the jugular vein; a peculiar stillness follows. 'The animal is now slightly curarised,' says the demonstrator; 'we want to excite the sympathetic and the vagus end.'" We do not say "if it was under curare," "if it had artificial respiration"; but we state definitely, on the authority of Mr. Bayliss, that the dog had had a dose of curare, and, according to our own observation, that no artificial respiration had been established. That is why I say that Professor Starling has misquoted the chapter. One experiment was performed under conditions which led us to infer that the animal had been curarised, although nothing had been said by the lecturer about anaesthetics or curare. Shall I give the reference to that?

7598. (*Sir Mackenzie Chalmers.*) I think it would be convenient if you would?—It is the chapter called "The only completely satisfactory method," page 129 of the old edition. This inference was based on Dr. Brodie's own words in his book "Essentials of Experimental Physiology," to which I have already referred, and his preference for curare which is expressed therein. One description, entitled "Painless Experiments," deals with the common experiments on frogs, for which anaesthetics are out of the question. Of the two remaining, one was performed, according to the lecturers, under

morphia and scarcely any anaesthetic, and the other under morphia alone. Mr. Thane has given as his opinion to the Commission that there is no foundation for our statement that "very often vivisection is received like a kind of entertainment; there is laughter and merriment, and nearly always there is evidence that most of the students find the proceedings exceedingly 'exciting.'" I fear Mr. Thane can never have been present at any of these demonstrations, and at any rate that the 28 experiments out of the total of 19,084 which were witnessed by him in 1903 did not include any of those described in the "Shambles of Science." Medical students are not notorious for dignified seriousness, and though I have no intention of suggesting that the infliction of pain and suffering would in itself give rise to merriment, I think the general taste for fun and rowdiness in medical students is sufficiently well known to justify the sentence quoted. In 1884 the "Medical Times" referred to the London medical student as "a little wild, a little rough, and guilty perhaps of occasional lapses from virtue." Though I believe much improvement may be found in the present-day students, there is certainly still room for more virtue. Our experience that the students find the proceedings exceedingly exciting has been shared by many, as for instance the late Professor Bigelow, Professor of Surgery in Harvard University, who said: "Watch the students at a vivisection. It is the blood and suffering, not the science, that rivets their breathless attention." And the late Sir Benjamin Ward Richardson, who said about demonstrations before students: "Intellectually, I do not think my classes were assisted, in the main, by the experimental demonstration. I am sure it limited my sphere of usefulness, by leading me, in the limited space of time at my command, to omit some parts of physiology of a simpler, less controversial, and more useful kind. I am bound to say, too, that, morally, I did not recall the effect as produced all that could be wished" ("Biological Experimentation," p. 144). A statement occurring on page 152 (first edition) of the book was put to Mr. Thane, and considered by him to be a misrepresentation. The statement is the following: "In the pain-tortured bodies, trembling under the sharp steel bathing in their own blood, and in vain trying to tear the straps that fasten them to their crosses of agony, twitching under the piercing currents of artificial electricity, there is no trace left of the normal life in the nerves, in the blood, in the wonderful co-ordinated mechanism of the internal organs." This passage should be read and quoted in connection with the following: "Or, when the nervous system, with its myriads of delicate cells, the powers and innermost functions of which offer such insolvable mysteries to our outer senses, is injured by curare, morphia, ether or chloroform, how can we under such conditions get the faintest idea of their true nature?" For these two paragraphs were written to show that pain and anaesthesia are equally unsatisfactory physiological conditions, an assertion which the recent attempts of physiologists, notably of Professor Pawlow's school, at producing normal conditions fully bear out. In the work by Professor Pawlow, which I have quoted before, there are such opinions expressed which entirely tally with the opinions expressed in these two pages in the "Shambles of Science." Professor Pawlow says on p. 15 in his book "The Work of the Digestive Glands" (in dealing with operative technique in animal experimentation): "Such a discussion of operative methods appears to me necessary, chiefly because it becomes more evident every day that in the ordinary method of the so-called 'acute' experiment carried out at one sitting, and complicated by free bleeding, many sources of error lie concealed. The crude damage done to the integrity of the organism sets up a number of inhibitory influences which react upon the functions of its different parts. The body as a whole, in which an enormous number of different organs are linked together in the most delicate fashion for the performance of a common and purposive work, cannot in the nature of things remain indifferent to forces calculated to destroy it."

7599. (*Sir William Collins.*) On that point with regard to anaesthesia which you have just mentioned, I do not think you have disputed in your evidence that you regard chloroform as a true anaesthetic?—I regard chloroform as a true anaesthetic if used in the proper way.

7600. Both in men and in dogs?—Yes.

7601. In the lower animals generally?—Yes.

7602. Although you have some doubt as to morphia,

you consider that chloroform may be regarded as a true anæsthetic?—I do not consider morphia an anæsthetic at all.

7603. You regard it as a narcotic?—Yes.

7604. Would you say that narcosis would be an improper term to apply to the conditions of a patient under chloroform?—The two terms are frequently mixed in a very unfortunate way; people often mix the two terms and use "narcosis" when "anæsthesia" should be used.

7605. I am sure that you are well able to give us a good definition of each?—I should say that the difference between narcosis and anæsthesia is that narcosis is an artificial condition of sleep in which pain is abolished to a certain extent but not altogether, whereas anæsthesia is a condition where pain is entirely abolished and consciousness is entirely lost; which is not the case in narcosis—consciousness is not entirely lost then.

7606. Then you include in your definition of narcosis a presumption that sensation is not wholly lost?—Yes.

7607. So that in all your references to narcosis we must read in that presumption?—It is all a matter of words. Narcosis may be pushed perhaps so far that it verges on anæsthesia; but those differences which I have indicated I think hold true and are the real differences.

7608. But when you refer to narcosis or narcotic conditions you imply by that a condition in which pain is not lost?—A condition in which pain is not totally abolished.

7609. Sensation is not lost?—Exactly.

7610. (*Chairman.*) Is not tobacco often called a narcotic?—Yes, but not an anæsthetic.

7611. (*Sir William Church.*) You have just said that, if pushed, narcotics are anæsthetics?—No, I have not admitted that. I have said that the two terms are frequently confused, and that it is thinkable that narcosis may be pushed sufficiently far to approach what we call anæsthesia; but I distinctly see the difference between the two, and I am not prepared to admit that they are identical.

7612. (*Sir William Collins.*) Do you mean that it is not a difference of degree, but a difference of kind?—Yes, it is a difference of kind.

7613. And that the discriminating difference is that in the case of narcosis sensibility to pain is not lost?—Narcosis is induced by drugs which have not the power of producing perfect unconsciousness; it is produced by drugs which have other effects.

7614. I understood when you kindly gave me a definition of narcosis, that you held that it included within it that abolition of sensibility to pain had not taken place?—I said certain pain. I believe I said that pain in narcosis is abolished to a certain extent, but not totally abolished.

7615. But that sensibility to pain in some degree is always present in a condition of narcosis?—Yes.

7616. So that it would not be right to use the term narcosis in speaking of any condition in which sensibility to pain was in any degree retained?—No; but I must add that this would be my definition of the term, because when one reads the literature on the subject one frequently finds the terms confused. People often talk about narcosis by chloroform and narcosis by ether; and according to the definition which I have just given, that use of the term would be inadmissible.

7617. I was rather anxious to know what you meant when you used the term narcosis?—I can only tell you what I believe.

7618. At any rate, you agree that chloroform and possibly other substances are anæsthetics; I suppose ether would fall in the same class?—Yes.

7619. It is possible to abolish entirely sensibility to pain both in man and in lower animals?—Yes, I quite agree.

7620. Presuming that a vivisectional experiment was carried out under chloroform or ether or one of those true anæsthetics and under conditions in which the anæsthetic was faithfully and continuously applied throughout the experiment, what moral or ethical principle in your opinion would be violated by such experiment?—In the first place, I do not believe that vivisectional operations generally can be carried out

under deep surgical anæsthesia, for the reasons I have already stated in my evidence; there are scientific objections, of which I have given instances. Secondly, my moral objection to vivisection is the whole principle of exploiting the lower animals for our supposed service and for our use. I take my stand as an opponent of vivisection rather on that ground than solely and entirely on the ground of pain.

7621. Then your answer, I understand, is twofold. The chief head of your objection is to the exploiting of the lower animals in the interests of man?—The two answers to your question will go together.

7622. I thought you stated that the latter was the more important?—I said that I did not believe but I knew, that vivisectional operations frequently required conditions which cannot be described as perfect surgical anæsthesia; and of that I have given instances to the Commission. And then I said in answer to your question whether I should object to an experiment on a living animal supposing it was unconscious, that I should object to it all the same; because I take my stand against vivisection not only and entirely on the ground of pain, but on the broader humanitarian ground of objecting to the general exploitation of animals for our use.

7623. Do you doubt that operations under what you call surgical anæsthesia are painless?—No; in some cases consciousness may return, but I could not go so far as to say that it frequently or often happens.

7624. That is to say, in a vast number of cases of surgical operations under what you call surgical anæsthesia, the operation is painless?—Yes.

7625. Then I understand that you simply do not believe those who state that vivisectional experiments on animals can equally be carried out under anæsthesia?—I cannot believe it, because I have facts to the contrary based on all the literature on the subject.

7626. (*Sir John MacFadyean.*) You have already made it perfectly plain in answer to previous questions, that you are absolutely opposed to experimentation upon animals in any shape or form?—I have.

7627. I should like to ask whether you are ready to admit, or on the contrary would deny, that anything useful for the cure or prevention of human or animal disease has ever been discovered in that way?—That is a question which it is very difficult to answer; it is all a matter of relative terms. What one considers useful another person might not consider useful. Personally, I believe that the abolition of vivisection will be accompanied by great changes and great development in the whole science of medicine; that new methods of healing will come in, and higher methods, as we know that the coarser medication and the coarser drugging are going out of fashion. From my own point of view, I am prepared to say that nothing which I should call useful has been obtained by experiments on the lower animals.

7628. Do you yourself attach great importance to your own opinion on the question of therapeutics?—I do not attach more importance to my own opinion than the average person does to his. When I am asked for my opinion I can but give it.

7629. I mean as compared with the average specialist on these subjects?—If you ask my opinion as a lay person, I give you my opinion as a lay person.

7630. That is just what I wanted to know; whether you offer it as a lay person?—Yes, I offer it as a lay person.

7631. I really did not know that, or I should not have asked you the question at all. But I will ask you to assume as a fact supported by an immense body of evidence, that discoveries which are valuable for the cure and prevention of human and animal diseases have been made by experimentation on the lower animals; and then I want to ask you this other question: Do you think it right to continue to take advantage of those useful discoveries?—First of all, I do not admit—I cannot admit—as I said, based on my previous answer, I cannot assume, that such inestimable advantages have been gained.

7632. You must not read more into the question than I said. I did not say inestimable advantages?—I beg your pardon: I did not mean to do so.

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7633. I did not say whether large or small; I asked you to assume, which does not mean that you are to assent to it. I assume from what you told me before that your knowledge is not wide enough to entitle you to form an opinion; but it is reasonable to ask you to assume that there is a great deal of evidence which many people accept as proving that useful discoveries have been made, and I wanted to know—it is an ethical question bearing on the justifiability of experimentation—whether you think it is right that we should continue to make use of these discoveries in practical medicine. I ask you to assume that for the sake of argument?—Assuming for the sake of argument that such advantages have been derived by vivisectional experiments, personally I should never make use of anything which I consciously knew had been derived by such means.

7634. I will ask you to assume as one of these useful discoveries, the discovery, for instance, of tuberculin, which many people think is calculated to play a very useful part in exterminating or reducing tuberculosis among the lower animals. Do you really think it is immoral on the part of farmers to employ tuberculin, or would you actually discountenance it; would you be opposed to the Government compelling the use of tuberculin, for instance, as a means of suppressing tuberculosis?—Tuberculin is one of the lasting monuments to the failure of drugs obtained through experiments on living animals. The terrible Koch fiasco a few years ago—

7635. You misunderstand me?—No; may I finish? When tuberculin was sent out into the world as a remedy for human beings and finally was discarded as a remedy, it was a great fiasco.

7636. But really I must protest again. I asked you to assume as a matter of argument that the tuberculin was really useful?—I cannot assume that.

7637. I did not wish you to give evidence to the Commission on the value of tuberculin as a diagnostic for tuberculosis?—You chose an instance which was too tempting to leave alone—that was, the fiasco of tuberculin.

7638. Will you forgive me for saying that I do not care in the least for your opinion about the value of tuberculin, but I wanted to know whether, assuming it for the sake of argument to be of great value, you think it would be right to take advantage of it, or whether you would actually deprive farmers of the advantage which they can derive from the systematic use of tuberculin?—Then I cannot assume that with regard to tuberculin without saying that I consider it a great fiasco. It was a great fiasco with human beings, and I do not believe that it is valuable as a test for tuberculosis in cattle.

7639. I am sorry that I am the first person who has put to you a question which has failed to get an answer. When you make use of the term "exploiting animals for human use," would you explain very shortly to the Commission what you understand by that expression. Is the keeping of animals in confinement—what we call the domestication of animals—covered by the expression exploitation of animals for human use?—No, it need not be.

7640. Is the killing of animals by methods involving avoidable pain in order that their flesh may be used for human food, exploiting them for human use?—I answer that killing animals for food by methods which involve pain is certainly exploitation of animals.

7641. Are you a vegetarian?—I am a vegetarian.

7642. On that account?—On that account.

7643. You say that you believe that all experiments on animals could be dispensed with, and that science would not suffer on that account?—Yes.

7644. Do you mean by that that up to the present time experimentation on animals has provided nothing of any value whatever—that it is absolutely valueless?—I think I answered that question before. I said that it is a matter of opinion as to what is valuable. To my mind vivisection has produced nothing which is valuable; but I am quite aware that there are other minds to which those results are exceedingly valuable. It is all a matter of comparative values, and I believe that the abolition of vivisection, when these methods are discarded, will be followed by new scientific methods and new scientific light, and that science need not suffer. Science has immense possibilities.

7645. I must put to you one more concrete case referring to a subject about which I know something myself. I believe it to be a fact that a valuable method of immunising animals against cattle plague was discovered within the last ten years—I mean the use of what is called the serum of animals which have recovered from cattle plague or have been hyper-immunised against it. The belief which I hold is shared by, I think, almost everybody who has examined the evidence, or had an opportunity of studying the thing practically. The method has been employed on an immense scale, and is believed to have saved millions of money. Will you tell me whether you think it is in the least likely that that discovery would have been made without experimentation on animals? It involved a great deal of experimentation on animals?—I should not dream of denying that that method has been discovered by experimentation on animals; in fact, that is something that we all know; but the point is whether some other method might not have been employed and could not have been employed with equal advantage. I believe on the whole in natural immunity versus artificial immunity, and I am not at all inclined to admit that the principle is a true or right one.

7646. But when you speak of natural immunity, are you aware that cattle plague was threatening to devastate the whole of the animals in South Africa; there was no time to provide a new breed of cattle with natural immunity. Will you give us some suggestion as to an alternative that might have been used?—I believe that probably there was some cause of that outbreak of cattle plague which could have been avoided. I believe in methods of prevention by sanitation above all.

7647. But it was too late to avoid it; it had been introduced into the country?—I do not know anything of the particular case which you are quoting, but I think that there is enough in the literature of the failure of sera and the whole principle of serum-therapy to warrant very strong opinions against the introduction of unnatural immunity.

7648. That is all the answer that you are prepared to give with regard to that question?—I believe that slaughter and isolation have been proved to be the best remedies in cases like that.

7649. Do you seriously suggest that it would have been better to refrain from attempting to immunise cattle in South Africa, and to have killed them all off?—I have no evidence that the cattle were immunised.

7650. I ask you to assume that, and I think you will not fall into error if you assume that immense numbers were immunised.

EIGHTEENTH DAY.

Wednesday, 8th May 1907.

PRESENT:

The Right Hon. the Viscount SELBY (*Chairman*).

Colonel the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
 Sir W. S. CHURCH, Bart., K.C.B., M.D.
 Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
 Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. W. H. GASKELL, M.D., F.R.S.
 Mr. J. TOMKINSON, M.P.
 Mr. G. WILSON, LL.D., M.D.
 Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. HENRY MORRIS, F.R.C.S., called in; and Examined.

7651. (*Chairman*.) You are the President of the Royal College of Surgeons of England?—Yes.

7652. And you have been desired by the Council of the College to give evidence before the Commission?—I have been.

7653. The Council, when a letter was read from the Commission, I believe, discussed it, and agreed to a resolution. They discussed it, and agreed to the resolution contained in my précis.

7654. Would you just read us that resolution?—"The Council of the Royal College of Surgeons of England express the opinion that the Act of 1876 is sufficient protection against any abuse of vivisection, and earnestly hope that the progress of surgical science will not be hindered by further restrictive legislation."

7655. You were not present, probably, at the Royal Commission on Vivisection in 1876?—No.

7656. And your evidence is relating to the advance in surgery and to its relation to experiments on living animals since 1876?—Yes. I may say that I have read the evidence of 1876—perhaps not entirely, but very nearly from cover to cover—so that I know the nature of the evidence of 1876. But this evidence of mine is based upon material facts and discoveries which have occurred since 1876.

7657. You propose in your evidence to refer to the advances in surgical knowledge which have been made, or in part made, since 1876 by experiments on animals—(1) As to the scientific and practical conditions essential for the proper healing of wounds; (2) as to the precise causes of many diseases the real nature of which was previously quite unknown to the profession; (3) as to the immunising or curative influence in certain infective diseases of the sera of immunised animals; and (4) as to the importance at the present time of experiments on animals for carrying on research as to the nature and origin of cancer in man and in animals?—Yes.

7658. Experiments on animals in relation to aseptic surgery and the healing of wounds have been of material advantage, in your opinion?—Most material. I quote Lord Lister. We could not have arrived at the knowledge we possess at the present day had it not have been for experiments on animals.

7659. You do not say that that knowledge has arisen from experiments on animals alone?—No.

7660. But there are other modes of investigation which have been going on in research?—Certainly.

7661. Chemical investigations, for instance?—Yes, each of the forms of investigation has been essential, but experiments on animals have been equally, if not more essential or important than the other forms of investigation.

7662. And it is not easy, in your opinion, to separate the advantages which have been derived from each particular source?—It is impossible.

7663. It is your decided opinion as an experienced surgeon that vivisection has been essential?—I have no hesitation in expressing that opinion.

7664. I use the word vivisection, you understand, because everybody uses it. Experiments on animals are not all what we should properly call vivisection, but I use it as meaning experiments on animals?—Yes. Mr. Lister (now Lord Lister), in giving evidence before the Royal Commission in 1876, stated that he could not have made his way in the subject of antiseptics without the assistance he had derived from experiments on the lower animals. It was by means of experiments on animals whilst studying the phenomena of inflammation that Lister ascertained many very important and fundamental facts. Thence he went on to the study of the blood, more especially the coagulation of the blood, thrombosis, etc. The facts ascertained in both these directions formed the basis of his subsequent work on the treatment of wounds. Lister, having come to the conclusion that the disasters which attended wounds were directly dependent on putrefaction of the discharges in the wounds, directed his attention to attempts to prevent this putrefaction, but for a long time without any marked success, because the causes of this putrefaction were unknown. It was after Pasteur had shown that putrefaction, like other fermentations, is caused by microbes growing in putrescible substances, that the problem how to protect wounds from bacteria presented itself to Lister. Lister himself demonstrated in his experiments on lactic fermentation that there must be different kinds of bacteria having different functions. So far, the early beginnings of aseptic surgery were only in part directly associated with experiments on animals.

7665. Pasteur's and Lister's experiments that you speak of were partly on animals?—Yes, both of them were very largely on animals. May I interpose a brief summary of Pasteur's work in three or four lines here?

7666. If you please?—Pasteur's works may be classed in three series—three great discoveries: (1) That each fermentation is produced by the development of a special microbe; (2) that each infectious disease is produced by the development within the organism of a special microbe; and (3) that the microbe of an infectious disease, cultivated under certain detrimental conditions, is attenuated in its pathogenic activity—from the virus it has become a vaccine, and as a vaccine is used for protection and cure.

7667. The important thing for our inquiry is, how far these discoveries were assisted by experiments on animals, and how far experiments were or were not essential?—With regard to (2) and (3) experiments were essential. Pasteur discovered that infectious diseases were produced by different microbes, by experiment.

7668. On animals?—Yes, and it is true that in the first instance it was by an accident he discovered that the microbe of an infectious disease, when cultivated under circumstances which are detrimental to that microbe, converts the microbe and its effect from a virus or poison of the disease into a curative or protective agency against the disease. Those discoveries were made by experiments on animals.

7669. We are not all of us, by any means, skilled

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in surgical or medical phraseology. What is the precise meaning of cultivating a bacillus?—Growing it on different media, like potato or broth, and things of that kind, and then producing the bacillus in multiple from the organism which he put upon that material, and also producing the toxins from it.

7670. You mean that the bacillus, as it were, multiplies itself in this new substance?—Yes.

7671. In what way is the bacillus so produced less effective as a poison than the one that is taken out of a living animal?—For instance, this cultivation, if you accept the term now, may be kept for a longer or shorter time, and be impaired in its poisonous effect by being kept; and then when that material is injected into an animal, it does not produce the full poisonous effect upon that animal, but it produces a modified condition, which prevents it from taking the disease.

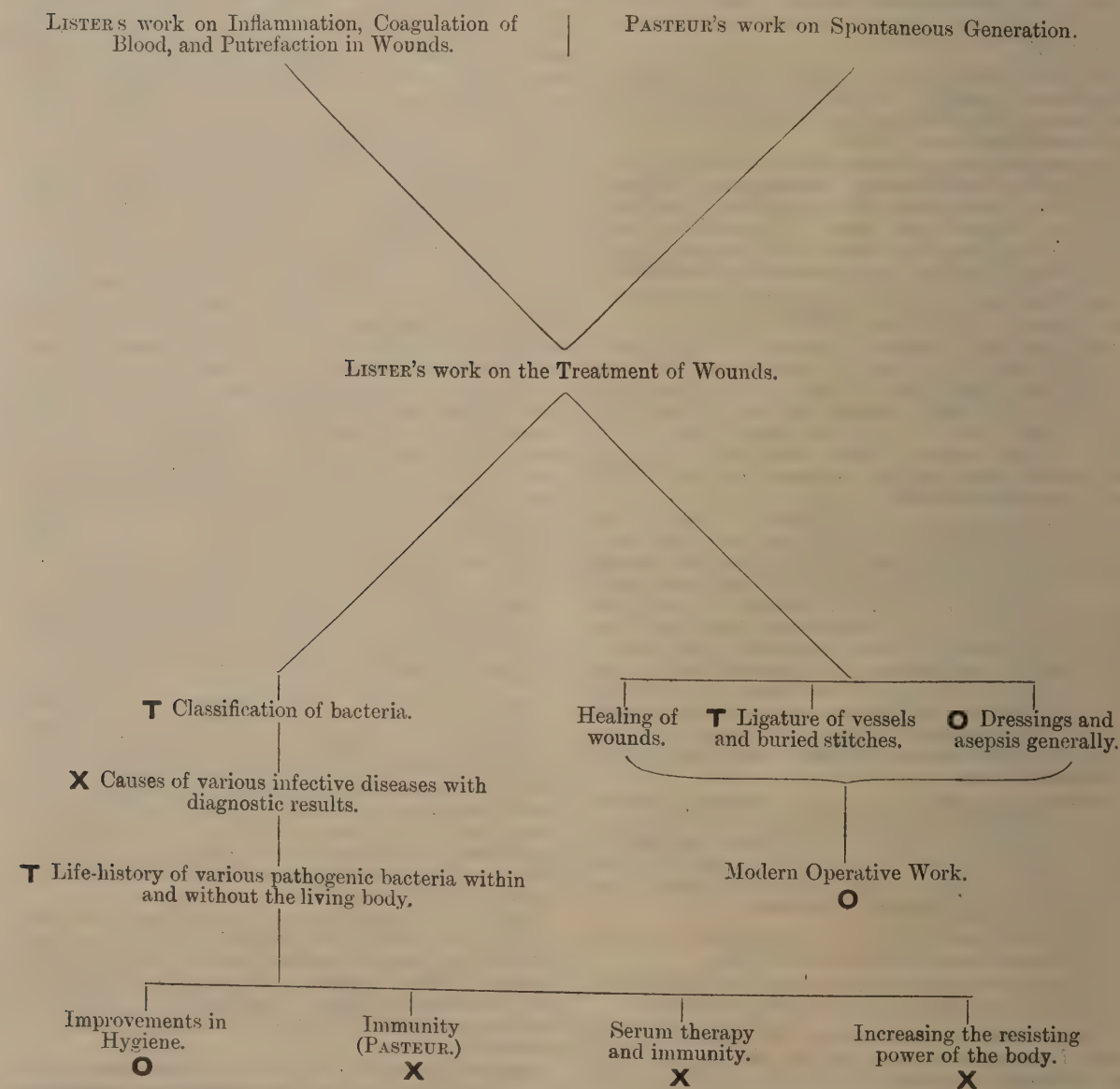
7672. That is to say, the virus is stronger where the bacillus is found in the blood of a living animal than it is in this cultivated substance or mixture?—When it is fresh. Perhaps I may take the case of the chicken cholera, by way of making what I want to convey

clearer. Pasteur found out that the poison of chicken cholera, after it had been kept some considerable time, did not produce the disease when injected into chicken, and that in those chickens which had that old material injected, when submitted to fresh and more active cultures did not develop the disease; whereas other chicken which had not been previously treated with this old culture did develop the disease, and died after being injected with the fresh culture. Does that convey to you what I mean by one of Pasteur's great discoveries? Then, besides that, take, for instance, the tetanus bacillus being taken from a dog which died of tetanus. If that is introduced into other dogs, it produces the disease; but under certain circumstances, which I have mentioned in my *précis*, the poison becomes so weak that it can be injected without producing the virulence of the disease. It is the same with anthrax. The anthrax bacillus has been submitted to various altered conditions by Professor Sclavo and others, and this, when introduced into animals, serves as a means of preventing the disease or of curing it, instead of developing it in its full force.

7673. Then you have a table, which will appear here in your evidence?—If you please.

LISTER'S WORK AND WHAT HAS FOLLOWED FROM IT.

(Drawn for me for the purposes of this Statement by Mr. W. Watson Cheyne, F.R.S., C.B.
Member of the Council of the Royal College of Surgeons.)



Explanation of Signs.

- O Profoundly influenced by knowledge obtained by experiments on animals, especially those leading to the diagnosis and life-history of bacteria.
 - X = Impossible without experiment on animals.
 - T = Dependent to a great extent on experiments on animals.
- The above "tree" indicates the development of Modern Bacteriology from Lister's work on the treatment of wounds and the relation of the various researches to experiments on animals.

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I would say that as the result of the most intense study and work, Lister constantly improved his methods of wound treatment, with the most remarkable results, in the prevention of septic diseases, as well as of certain diseases which at first he had not looked on as due to the entrance of bacteria, such as erysipelas and tetanus; the latter had not previously been thought to be associated with decomposition in wounds. But knowledge of these matters was only increasing slowly and imperfectly till the appearance of Koch's work on the infective diseases of wounds. Koch showed, entirely by experiments on animals, that there are two great classes of bacteria—viz., those that cannot grow in the living tissues—the non-pathogenic bacteria; and those that are able to grow in the living tissues, and growing there produce various forms of disease. Koch further showed that the troubles which arose in connection with wounds might, on the one hand, be due to the absorption of the chemical products of the growth of one or both of these kinds of bacteria in the discharges of the wound; or, on the other hand, to the entrance of pathogenic organisms into the tissues, and their growth in the body. In this way he demonstrated the occurrence of septicæmia in mice, due to small bacilli; septicæmia in rabbits, due to cocci; pyæmia in mice, due to micrococci, and many other things of the same kind.

7674. What exactly is the distinction between a bacillus and a coccus?—A coccus is a small round body, and a bacillus is a rod shape or a dumb-bell shape or a drum-stick shape. They are exceedingly microscopic, of course.

7675. Are these experiments on mice and rabbits applicable to septicæmia in man also?—Yes, septicæmia in man and the cause of septicæmia in man have also been demonstrated and proved accurately by the experiments on mice and rabbits.

7676. What is the connection between discovering septicæmia in man and in animals; how is it arrived at by these experiments on animals?—By cultivating the organisms which are found in the discharges or the blood of men suffering from septicæmia or pyæmia.

7677. Would you not find it in the blood of the men themselves, supposing you made an incision in them?—You may find it there, as has been the case in many diseases in animals. You may find organisms and appearances constantly in the blood or the tissues, but you have not the proof that those are the causes of the diseases until experiments have been made on animals with these materials or organisms, and the same disease has been developed in the animals so inoculated or treated. For instance, if you take the tubercle bacilli found in the tubercle nodules of the human being, and cultivate it, and inject the cultivated material into animals; or even, without cultivating the bacilli outside the body, if you inject the bacilli direct from the urine or from the blood of man, you can produce tubercle in the animal. But it had to be proved that the disease was due to the bacillus by experiments on animals.

7678. That is to say, you might find that particular bacillus in the blood of a man who had a particular disease, but you could not be sure that that was the cause of the disease without experimenting on animals?—That is so. I was going to say that in that particular disease of tuberculosis, many years before it was identified as being due to the presence of tubercle bacilli, it was known to be an infectious disease, and due to some material in the animal or the person suffering from it.

7679. You go on, in your *précis*, to Koch's work?—Koch established three great things; he laid down three great postulates which have a bearing on this whole subject. These were, that you must find the microbe of the disease, and then, having found it, you cultivate it in appropriate media outside the body and inject it into animals, and then reproduce the same disease over again in those animals so as absolutely to identify the disease with the particular organism by recovering it in the tissues or fluids of the injected animals.

7680. What are the three postulates? You say the third of the three postulates necessitates experiments on animals, but you have not told us what they are?—The first is that you must discover the micro-organism which is the cause of the disease, the coccus or bacteria as the case may be, in every case in which the disease exists. Then, by making a pure culture,

separating it from everything else, and making the pure culture of this particular organism, you introduce it into an animal, and it produces the disease there; and then you will find in the blood and tissues of that animal the same organism as you found originally in the individual who was attacked by the disease.

7681. That is the third postulate?—Yes.

7682. The third of these you say necessitates experiments on animals?—Certainly.

7683. I was going to ask, does not the second also? No. Not the cultivation of the organism outside the body.

7684. When you say that experimentation is essential, can you suggest any other means by which it could be done?—No.

7685. Based on these postulates, the discovery was made of the bacteriological origin of a great variety of diseases, both in man and in animals—*e.g.*, tubercle, tetanus, diphtheria, all sorts of wound infections, such as septicæmia, pyæmia, sapremia, erysipelas, and infective gangrene, as well as gonorrhœa, plague, cholera, pneumonia, and so on.

7686. (*Colonel Lockwood.*) What is sapremia?—It is a blood poison. It is the name given to that form of blood poison where the intensity of the disease and the exhibition of the poison depend upon the amount of the poison; so that, if you have a mild quantity of the poison you get a mild form of the disease, and by increasing it you get an increasing disease. It is known as "septic intoxication."

7687. (*Sir William Collins.*) Is it not a form of blood poisoning that is independent of any micro-organism?—No.

7688. You would not say that?—No, I would not.

7689. (*Chairman.*) The particular bacillus which is responsible for each of those diseases you say has been discovered by experiments on animals?—Yes, and identified and proved to be the cause of the disease.

7690. (*Colonel Lockwood.*) And classified?—Yes, more or less classified into aerobic and non-aerobic, if you understand those terms; and again classified into those organisms where the poison is contained within the body of the micro-organism or is outside the actual body; those where the animal itself causes the disease and those where what is called the toxin or an emanation from the micro-organism causes it.

7691. (*Chairman.*) Tetanus organism, for example, is found in the ground?—Yes, it is one of those that does not live in the air, and is called anaerobic therefore, and its poisonous effect is produced by its toxins rather than by the bacillus itself.

7692. When you say that the poisonous effect is caused rather by its toxins than by the bacillus itself, what is the distinction between the bacillus and its toxin?—The bacillus is the organism, but the toxin is some emanation from it.

7693. Is not the way that all bacilli work by their toxin?—No, some of them work by themselves.

7694. Without emitting any toxin?—Yes. As you have asked on this point, I think I could give an illustration, for instance, of the importance of classification of bacteria, and how experiments on animals prove the importance of it in identifying different bacilli in the blood of a diseased animal. Doubts were thrown upon the accuracy of Pasteur's work in reference to anthrax, and certain animals died, which were said to have died from anthrax, but not from septicæmia. Pasteur, knowing that the bacillus of anthrax is an aerobic organism, and that the bacillus of septicæmia (or using the word blood poisoning in that sense) is an anaerobic organism, cultivated from discharges or blood the anthrax bacillus, when exposed to the air, and not the septicæmia bacillus, and the septicæmia bacillus in an atmosphere of carbonic acid, and not the anthrax bacillus.

7695. What do you mean when you say, "And not the anthrax bacillus"?—From the same fluid he was able to cultivate one bacillus in a culture exposed to the air, namely, the anthrax bacillus, and the other bacillus, namely, the septic vibrio of septicæmia, only from a culture exposed to an atmosphere of carbonic acid; and then by taking those separate products of the cultivations he produced the anthrax disease by the one material and the septicæmia by the other material,

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in different animals. Thus, by the knowledge that the septicæmia organism was anærobic and anthrax bacillus was ærobic, he was able to by these different modes of cultivation to get each of these organisms separately from the same blood; and then with each of these organisms separately he produced the corresponding disease in a separate and isolated form.

7696. That is to say, each can only live and multiply in its own element?—Yes.

7697. The ærobic bacillus, you mean, is one that lives in the air?—Yes.

7698. And the anærobic, either in the body or in the ground?—Yes, tetanus, for instance, is an anærobic.

7699. But if it is in the body of a human being?—It will live in the body of a human being—the tetanus bacillus will do that; but outside the body it is existing and persisting in the earth, and in dust, and so on, where it is not exposed to oxygen.

7700. You gave us the particular bacillus that you said was an ærobic bacillus; what was that?—The anthrax bacillus. The septicæmia bacillus is an anærobic bacillus.

7701. What is a specimen of an ærobic bacillus?—The septic vibrio of septicæmia. I only bring that matter forward to show you the importance of these experiments on animals; that by the introduction of pure cultures of these separate micro-organisms you are able to absolutely identify the nature of the disease of the body from which it is taken, and to produce that same disease again in the animal which is inoculated.

7702. These are experiments in which the only operation consists in inoculating?—Yes, they are inoculating experiments.

7703. With a lancet or syringe?—A syringe.

7704. And any suffering in the animal is a suffering of the disease, really?—Yes.

7705. You go on to speak of the value of experiments as regards improving methods of treatment and prevention?—Yes. Of course, the whole of the aseptic and antiseptic methods of the treatment of wounds is the outcome of this knowledge of the micro-organisms in the putrefactive discharges being the cause of the disease. The discovery of the causes of these diseases has led to the early and precise diagnosis of them, and to a more intelligible and effective manner of treatment than was previously possible. In the prevention of the spread of diseases and in hygienic science, advances have been made, owing to experiments on animals, which could not have occurred had such experiments been prohibited. Further, the study of bacteria, and their effects on animals under various conditions, led Pasteur to open up the whole question of immunity. In the course of his experiments on chicken cholera, Pasteur wished to inoculate some fowls, and for this purpose took a cultivation of the bacteria of chicken cholera, which had stood in a flask in his laboratory for some weeks. He found that the animals so inoculated did not acquire the disease as they ought to have done, and on re-inoculation of the same with a fresh cultivation, he further found, to his surprise, that they did not develop the disease; while fresh fowls, which had not been previously subjected to the first injections, did. In other words, he had protected or immunised the fowls by the first inoculation, although they had not shown any symptoms of the disease. This accidental discovery led him to obtain and employ attenuated cultures, which could reproduce the disease in a form having attenuated virulence, and at length a veritable vaccine to completely protect against the virulence of the disease. ("The Life of Pasteur," by René Vallery Radot, vol. II., p. 100-1.) These discoveries have proved of great benefit both to animals and human beings, in proof of which it is only necessary to name anthrax, swine erysipelas, and rabies. "Rouget of Swine" (Pasteur's Life, p. 125, vol. II.). The experimental study of pathogenic organisms has also led to the discovery of protective and curative sera of proved value, and of great importance in treating a number of diseases, the most striking instance being diphtheria. The marvellous improvements which have been made since the Royal Commission of 1876 in the treatment of wounds, and in our knowledge (a) of proper disinfection; (b) of the sterilisation of dressings and instruments; (c) of the action of antiseptics on wounds;

and (d) of the behaviour of various substances used as ligatures, when buried in the tissues, are due to the precision and clearness with which the nature, the virulence, and the habitats of the various pyogenic organisms have been made out.

7706. When you say buried, do you mean temporarily or permanently buried?—Some of them have been permanently and some temporarily. The old plan of ligaturing was very frequently a disastrous one, because it entailed the application of a silk thread to a vessel, and it was left projecting from the wound. Suppuration was very common in those days, and in the process of separation of the ligature and the ligature coming away, as it was spoken of as doing, secondary hæmorrhage frequently occurred. Improvements were being sought, and various methods were employed, such as a cu-pressure, torsion by many long needles passed in, and twisting the vessels round, to do away with the danger of having these projecting ligatures; and then it went on to the employment, through Lister's experiments, of buried ligatures, using various material, such as ox-aorta gut, cat-gut, and silk.

7707. Which need not be taken out?—Which were tied, and the ends cut short, and left to be surrounded and grown into, or absorbed by the tissues.

7708. I suppose the important thing was that those should be in themselves aseptic?—Absolutely.

7709. And could be absorbed?—Yes, either absorbed or encysted, many of them being absorbed. That was a matter of the very first importance in connection with doing away with the liability of suppuration of wounds and of secondary hæmorrhage. By getting an immediate union, a union by first intention, as we call it, of the wound, these things round the vessels, cut short and left in the tissue, became buried and shut out from all liability of doing harm, which ligatures, hanging tail-like out of the wound, so often caused.

7710. Were these ligatures, which could be safely left in the wound, ascertained by experiments on animals?—Yes.

7711. It was partly the method and partly the materials that was of importance?—Yes.

7712. As regards the materials, would those be employed?—Yes, they were used upon animals in ligaturing the vessels of animals and so on; but also, you see, the connection with experiments on animals in this matter is in the healing of wounds by the discovery of the causes of the suppuration of wounds and the causes of septicæmia, pyæmia, etc. It all steps together. The experiments led to the knowledge of the preparation of these ligatures, as they led to the knowledge of how to get the wound to heal without suppuration.

7713. (Sir William Church.) The importance of the experimentation on animals was that it demonstrated what were the sources of dangers in these ligatures?—Yes.

7714. (Chairman.) All that arose on the question I asked you about the word "buried." Perhaps you will go on about the operations?—I have made it clear that they are simply buried in the wound.

7715. Yes?—Since the danger of septic troubles has been removed, enormous strides have been made in surgery in recent years. Many operations which used to be looked upon as too dangerous to be undertaken, except under stress of necessity or as offering a last chance of saving life, are now performed with absolute safety; many new and important ones, which were formerly never dreamed of, have now become matters of successful and almost daily performance. Finally, whereas the mortality, even after quite simple operations, was at one time so appalling that the patients were often deterred from submitting to, and the surgeon from performing them, operations of considerable magnitude are now undergone with the confidence which comes from knowing that the mortality is, in many instances, but a fraction of an unit per cent. To-day the abdominal cavity of man is opened. Every organ within is inspected and palpated for diagnostic as well as for operative purposes, the wound is closed, and the patient recovers from the effect of the anaesthesia, and takes a completely uneventful course towards convalescence. This course is very commonly without pain, without mental shock or distress, without discomfort of any kind beyond some anaesthesia sickness, some flatulence, and the weariness of

lying still. Within 10 or 14 days after such an operation the patient has generally quite recovered.

7716. In what way have experiments on animals assisted to that result?—Through the rapid healing and the healthy condition of the wound from the very start to the finish; whereas, before we knew through experiments on animals what the causes of putrefaction and suppuration were, and how to prevent them, the majority of these wounds suppurated.

7717. You mean Lister's and other's experiments with regard to aseptic treatment?—Quite so.

7718. (Mr. Tomkinson.) What does palpated mean?—Handling—taking hold of with your hands—examination by touch, I would rather put it; palpating is feeling with one's hand. You can examine an organ by sight as well as by handling. Or, an incision is made into the loin, the kidney is exposed and freed from its surroundings, is drawn out upon the loin, inspected, palpated, incised and examined by the finger inserted into the wound in the organ, and by a bougie passed down from it to the urinary bladder; the kidney is then replaced, and the patient is well and sitting up in 10 or 12 days afterwards.

7719. (Chairman.) We have heard a good deal about operations, not precisely the same, but somewhat of this nature, as regards the laying open of the body in animals. Of course, all those operations, whether upon man or upon animals, are, or should be, under anæsthetics?—Yes.

7720. You have told us what your experience is as to the amount of pain suffered by man. Would the pain suffered by animals under such an operation be, in your opinion, the same or greater or less than that suffered by man?—Allowing that an animal is equally susceptible to pain as man, there would be equally the same immunity from pain and suffering, provided that the aseptic condition after making the wound was kept up.

7721. And provided that in each case the anæsthesia was complete?—Of course, so far as the operation is concerned.

7722. I mean during the operation?—Yes, but not the subsequent course. I am laying emphasis here upon the immunity from subsequent pain—the pain during the recovery from operations, which has been such a remarkable and beneficial result of our knowledge of septic diseases derived from experiments on animals.

7723. Supposing an animal were subjected to an operation (that is, perhaps, hardly the right medical term), which involved cutting into the entrails under anæsthesia, would the animal be treated on recovery in the same way as you would treat a man?—Yes, I believe the Act compels that; I think, I am not quite sure, it compels that they should be treated antiseptically, and if they are treated antiseptically after the performance of the operation, the animal, equally with the man, will recover with practically no suffering.

7724. (Dr. Gaskell.) Is it not the fact, or perhaps you do not know, that the recovery would probably be quicker than in man?—I do not know that it is quicker, because really the rapidity with which some of the wounds in the human being heal is so very great that it is almost impossible to be quicker; but probably a great number of wounds upon animals do heal in a shorter time than in man.

7725. (Chairman.) I suppose the average constitution of a reasonably healthy animal is better than the average constitution of a human being who is kept alive by so much care very often?—Yes, no doubt.

7726. I think we will not trouble you to read the remainder of your *précis*, but it will be printed as part of your evidence?—If you please. The *précis* which deals with the subjects classified in the answer to Question 7657 continues:

A joint is the seat of a sudden and most painful inflammation which is mistaken for an attack of acute rheumatic fever; it is nowadays punctured, the fluid is withdrawn and examined, it is found to be septic, and the joint is then at once laid open and drained; immediately all pain vanishes, and the patient quickly recovers with a moveable and natural joint, instead of, as formerly, after a lingering illness if at all, and with a stiff joint, a deformed limb, and suppurating sinuses discharging on the surface of the body.

The disease technically known as osteomyelitis or diffuse abscess within bone is no longer looked upon as

due to exposure to cold, or as rheumatic; nor is empyema, or suppuration in the chest cavities, any longer attributed to access of air to the pleural surfaces; but both diseases are alike treated by the freest incisions and drainage, with the result that great suffering is relieved and thousands of lives which formerly would have succumbed to these diseases are saved.

The foregoing are some illustrations of the beneficent results of the combined labours of Lister, Pasteur, and Koch.

Further, I may refer to the recent work of Wright and others on opsonins, which holds out some hope of extending our knowledge as to how to increase the resistance of persons against or how to cure them of such diseases as tubercle, pyorrhœa, and the various diseases caused by staphylococci, such as boils, impetigo, gonorrhœa, etc. Wright has found, and Koch had previously found, that the introduction of the dead bodies of bacteria into animals increases the resistance of the animal to the disease produced by these bacteria, and he has devised a method of ascertaining the resistance of an individual to a particular pathogenic organism. The results show that by proceeding by his methods the resistance of an individual may be increased to such an extent that he is enabled to throw off the disease in a way that he otherwise was unable to do.

In various septic diseases sera have been introduced which are of considerable value. Of these may be mentioned anti-streptococcus serum. At first the results of the use of this serum were disappointing and contradictory. In some cases it seemed to exercise no benefit, while occasionally surprisingly good results were obtained. Further study of the streptococci has shown that there are many varieties of these organisms, and that the serum derived from one variety may be useless against another. This led to attempts to immunise an animal against a number of different varieties of streptococci, and these attempts have in turn led to improved results.

Quite recent results have been published by Fenwick and Parkinson of the beneficial use of rectal injections of an anti-streptococcus serum in gonorrhœal infections, and certain cases of hæmorrhagic purpura. The serum used by them was one made at the Wellcome Laboratories at Herne Hill, and is there known as the "Polyvalent" Anti-streptococcus Serum, in the manufacture of which 43 different strains of streptococci taken from such different diseases as septicæmia, puerperal fever, malignant sore throat, acute rheumatism, etc., are employed. (Med.-Chir. Trans. Vol. 89, p. 183, 1906.)

It is expected that this "Polyvalent" serum will remove the present uncertainty of the effects of treatment by other anti-streptococcus sera, and that it may prove an immunising agent against infection in certain operations where aseptic and antiseptic methods are not strictly applicable.

Experiments on Animals in relation to our Knowledge of Shock and how to prevent it.

Since the dread of septic troubles has been removed a very important subject to which surgeons are now directing their attention is the diminution or prevention of shock. Some of the most valuable operations are occasionally attended by the occurrence of severe and even fatal shock. As the result of much experimental work we are improving our methods in the way of preventing this calamity.

The investigations as to the relation of the vaso-motor nerves, the medulla, and spinal cord to shock, and as to the influence of injections of supra-renal capsule, of adrenalin and other vascular contractors, all depend on physiological experiments.

Many of the researches on blood-pressure, on the nervous regulation of the blood-vessels, etc., which seem of more scientific than practical importance, have a direct bearing on this question of shock, and should be encouraged, as they are tending in the direction of helping us to obviate this danger in surgery. All the vascular contractors have been discovered by experiments on animals, and there is much yet to be learnt about such drugs in inflammatory diseases and hæmorrhage (A. R. Cushny).

Other witnesses will not doubt bring testimony to bear on the importance of the experimental study of the action of drugs on the therapeutical treatment of diseases.

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II. and III.

Experiments on Animals in relation to the Bacterial Origin of Certain Diseases, and the Immunising and Curative Influence of Culture Sera.

In illustration of the necessity of continuing experiments on animals if the benefits which are obtainable from them by immunising, preventing, and curing certain diseases in man and animals are not to be checked or altogether to cease, I will mention the following diseases:—

A.—DIPHTHERIA.

The Klebs-Loeffler or diphtheria bacillus was first described by Klebs in 1875, and was obtained in pure culture by Loeffler in 1884. Its isolation from other bacteria which are also found in the mouth was a matter of great difficulty and the work of many years.

Behring and Kitasato in 1890 cleared the way for immunising healthy animals against, and curing infected animals of diphtheria, but it was not until 1897-1898 that Park isolated the particular strain of bacillus and obtained the particular toxin, which was of a power of sufficiently high virulence to produce an antitoxin of regular and standardised strength.

For the continuation of the antitoxin treatment to be possible two sets of vivisectional experiments must go on:—

- (1) The inoculation of guinea-pigs to standardise the strength of the toxin and the antitoxin; and
- (2) The inoculation of the horse with the toxin; and
- (3) The venesection of the horse to obtain the serum or antitoxin.

By No. 1 the minimal lethal dose of the toxin, or poison derived from the pure culture of the diphtheria bacillus, and the minimal quantity of serum required to neutralise the same are ascertained; 10,000 guinea-pigs a year for this purpose have been used in Ehrlich's laboratory, and about 2,000 a year were used at the laboratories on the Embankment for the same purpose.

Animal inoculations are required also for diagnostic purposes, and as a protection of the community against infection from persons convalescent from the disease. It is now the practice in the Metropolitan Asylums Board hospitals, as it has been for several years past in New York, to examine the secretion on the throats of patients who have recovered from diphtheria for the presence of bacilli. Any organisms resembling diphtheria bacillus are tested on animals not only to establish their virulence, but also to recognise that it is the diphtheria bacillus. One guinea-pig is injected simultaneously with a broth-culture of the suspected organism. Another guinea-pig receives the same kind of injection with the addition of a small quantity of diphtheria antitoxin. If the organism is virulent diphtheria bacillus the first guinea-pig will die, whereas the second will be quite unaffected.

At the Metropolitan Asylums Board laboratories the inoculation test on animals is also used for the diagnosis of cases occurring in the hospitals of tuberculosis, glanders, and cerebro-spinal meningitis. The milk supplied to the hospitals of the Board has also been tested for the presence of tubercle by the inoculation of guinea-pigs.

The diminution in the mortality from diphtheria since the introduction of the antitoxin treatment is considerable. There are numerous reports and statistics easy of access which prove the truth of this statement (Clinical Society's Transactions, and Stephen Paget's Book).

As in the case of the animals used for cancer research, so in the case of the horses used for obtaining diphtheria antitoxin, I can from personal inspection testify to the extreme care and attention and the great liberality as to fodder bestowed upon them, and to the absence of any painful or deleterious effect on them as the result of the injection and subsequent bleeding.

B.—TETANUS.

About 1884 Nicolaier showed the real microbic cause of tetanus to be a bacillus in earth and dust, which, when injected into the tissues of animals, produced tetanus in the animals so treated. Rosenbach, about

the same time, found the same bacillus in a patient suffering from traumatic tetanus. The identity of the bacillus of tetanus in man with Nicolaier's bacillus of earth was demonstrated in Koch's laboratory in April, 1887. In man the bacillus has been found in the wound-secretion, in the nerves leading from the seat of infection, and in the spinal cord of patients suffering from tetanus. The bacillus has been found also in surface soil, in street dust, and in the dust of old buildings, such as churches, etc.

Though it cannot be said that any one of the tetanus antitoxins has been found to be a curative agent in the disease, the preventive use of tetanus-antitoxin for the immunization of men and animals has given great results (see for the results obtained in the veterinary practice in France, the *Med. News*, 7th July, 1900; and for the results obtained in a large number of horses in the United States of America, see *Med. Annual*, 1901).

The preventive use of antitoxin in the human subject was shown in an outbreak of tetanus at Prague in 1899. (Paget's Book.) H. J. Scherck gives a strong proof of the power of antitetanic serum as an immunizing agent in man in relation to the injuries caused by toy pistols during the 4th July or Independence Day celebrations in America. (See "The Journal of the American Medical Association," August 18th, 1906, pp. 500, 505, and 512.)

The modern study of tetanus has done away with the useless operations of nerve-stretching and nerve-dividing, which were based on the fallacious theory that tetanus is an ascending inflammation of a nerve. It has led also to the more frequent, early, and complete excision of the wounded tissues, because it is now known that the bacilli, at the outset, are limited to the tissues in the immediate area of the wound. A certain number of cases of chronic tetanus have been reported as cured by the tetanus antitoxin. Injections into the spinal sheath are reported to have cured three patients out of four; and a fifth case, namely, that of a boy into whose nerves and spinal subarachnoid space the antitoxin was injected, is stated to have been cured. Still, other successful cases have been reported recently. ("The Medical Annual," 1905, p. 80; and 1906, p. 53.)

C.—HYDROPHOBIA.

Rabies is another disease the scientific study of which only commenced four years after the date of the 1876 Act. Though a prolonged search for the specific microbe has not hitherto been attended with complete certainty of success, yet, quite recently, Professor Negri, of Pavia, has found, in 50 out of 52 cases, a micro-organism in the nerve-tissues of animals dead from rabies ("Med. Annual," 1906, p. 418).

Renault and Lannelongue discovered some years ago that rabbits could be successfully inoculated with the saliva of rabid animals, such as the dog, fox, jackal; and later, Pasteur produced the disease in the same way. Though the specific microbe remained unidentified, it was ascertained and put beyond doubt that the essential cause of hydrophobia is a specific virus which can only be reproduced within the living body. Another convincing proof of its microbic origin was established by artificially producing the disease in healthy animals, by implanting in them fragments of brain or spinal cord tissue taken from animals dead of rabies. By transmitting the virus contained in nerve tissue through a series of rabbits, Pasteur was able to obtain a standardised virus of exact strength and having a known period of incubation (six days). He was also able to attenuate the virus in the nerve tissue by drying the spinal cord taken from animals dead of rabies—the greater number of days it was drying the weaker became the virus in the spinal cord. In this manner, he was able to have always on hand cords of different strengths of virus for use in different cases, just as a pharmaceutical chemist keeps on hand the same drug of different strengths. The essence of Pasteur's treatment is to render persons artificially immune before the disease caused by the bite has had time to act. Thus a man who has been bitten by a rabid animal can be immunized by inoculations whilst the disease is still dormant, with a succession of doses of graduated strength until he has within himself enough antitoxin to make him proof against any outbreak of the disease.

It is clear, therefore, that in order to maintain a constant supply of antitoxin, rabbits must be inocu-

lated with the virus to yield, immediately after being killed, spinal cords suitably affected by the disease.

Although up to the present prevention and not cure has been obtained by the antitoxin treatment, yet thousands of human lives have been saved by its immunizing power, and the mortality is absolutely undoubted cases has been reduced to less than 1 per cent. (Stephen Paget). I would like to add here that from a report published in "Annales de l'Institut Pasteur" (p. 411, vol. 1905) for the year 1904, it appears that of 755 persons who underwent the antirabic treatment at the institute only three died. This is a mortality of 0.39 per cent. This mortality will be still further improved if Negri's bodies are found to be the specific cause of the disease, because the suspected animal which has bitten a human being, or another animal, if at once killed and its nervous system examined, will show the presence therein of the micro-organism, and then the immunizing treatment would be commenced at once, instead of waiting to see if the offending animal develops rabies or not. Thus the proof that Negri's bodies are the real cause of rabies would be a discovery of the greatest diagnostic value, because most of the fatal cases which occur during or after the antitoxin treatment have been in persons upon whom the treatment was commenced too late to take effect. Though the detection of the organism of Negri in the nerve tissue of the animal which inflicted the bite would be a matter of microscopical or culture examination, still animal experiments will be required to test whether the organism complies with the three postulates of Koch before it can be admitted to be, or not to be, the cause of the disease. Again, if by an "intensive" modification of Pasteur's treatment a cure is ultimately to be discovered, it can only be by further experiments on animals.

It is a satisfaction to know that rabies in rabbits is of a painless paralytic kind, and that these animals do not suffer from muscular contractions as men, dogs and other animals do.

D.—ANTHRAX.

Anthrax (called also charbon, splenic fever, or splenic apoplexy, malignant pustule for the sore at the site of inoculation in man, and woolsorters' disease when the lungs are infected after inhalation of the spores of the bacillus anthracis) occurs in human beings, especially in those engaged in avocations having to do with hides and the wool or hair of animals, and in rag pickers. It affects sheep, cattle, horses, pigs, and goats, and is transmissible to guinea-pigs, rabbits, mice, and sparrows. It spreads amongst animals by means of the germinating spores of the bacillus which attach themselves to plants and grass, and which, when taken in with the food, become the cause of intestinal anthrax. Pasteur showed by injecting guinea-pigs with particles of earth, how flocks were exposed to infection, merely by grazing over certain fields. Anthrax was the first of the infectious diseases definitely proved to be of bacterial origin. As long back as 1849, little fine rod-like bodies were first observed in the blood of sheep dead of anthrax; in 1863 Davaine first proved that these rod-like bodies were living organisms (which he called bacteridia) and were present in the blood of every animal affected with anthrax; and later still, Koch succeeded in making cultures of them and reproducing the disease by inoculating other animals with these cultures. But even in 1876 and later it was denied that the bacillus anthracis was the cause of anthrax, until Pasteur's work finally settled the question in 1881. Inoculations against anthrax have now been employed since 25 years ago, and wherever employed the losses from the disease have sunk into insignificance. Sheep and cattle are inoculated with the attenuated virus known as the anti-charbon vaccine. This vaccine is obtained of different strengths by cultivating the anthrax bacillus in neutralised bouillon at different temperatures and for different lengths of time. It is found that a second inoculation with a vaccine of greater strength than that used for the first inoculation in the same animal, gives a greater degree of immunity and protects the animals against the most virulent form of anthrax. The inoculations are not altogether free of danger, and a certain number of sheep and cattle—though an excessively small proportion—die as the result of the inoculation. Millions of sheep and half a million of cattle were inoculated in France in the 12 years 1882-1893. The result of the

treatment of the mortality from charbon, including the mortality from the inoculations, was to reduce it from 10 per cent. to 0.94 per cent. in sheep and from 5 per cent. to 0.34 per cent. in cattle. M. Chamberland (Annales de l'Institut Pasteur, March, 1894, p. 163), from whose report these figures are taken, estimates that the money advantage of these inoculations to French agriculturists amounted at the least to 5,000,000 of francs in respect to sheep, and to 2,000,000 of francs in respect to cattle.

E.—GLANDERS.

Glanders or farcy, which originates in the horse and occurs in man by contagion, is a disease caused by a specific microbe, namely, the bacillus mallei. This disease is very rife among horses, and is stated to be on the increase. Tigers, lions, cats, sheep, goats, asses, and other animals, are susceptible to inoculation with the virus of glanders. In man, it occurs in the acute and chronic forms, and is exceedingly fatal in the acute. When man is the subject of glanders, diagnosis is rendered positive by detection of the bacillus in the nodules, the pus from the sore, the sputum from the lungs, and in the fluid of an affected joint. Numerous bacilli are also found in the blood of man more constantly than in the blood of glandered horses. Treatment is still most unsatisfactory.

Though glanders has been known for centuries, it was not until 1882 that the bacillus of glanders was discovered; later it was cultivated on sterilised potato and on solid, sterilised blood serum at a temperature of 104 deg. F. Inoculation experiments with the culture were made on horses, guinea-pigs, rats, and rabbits, to prove the identity of the cause of the disease. Test inoculations with mallein are largely used on horses before they are put into stables with other horses, to ascertain whether they are or are not in an incipient stage of infection. Up to the present, however, no preventive or curative antiglanders vaccine has been discovered, though future experiments may, it is to be hoped, furnish one. The knowledge of the cause of the disease has led to greater care being used with regard to sponges, cloths, etc., used by grooms and ostlers who are themselves in consequence less likely to be infected.

F.—GRAVES' DISEASE.

Graves' disease is nowadays treated with very marked improvement in all symptoms, and in some cases with complete cure as the result, with antithyroidin—i.e., serum removed from goats after removal of their thyroid gland. ("Medical Annual," p. 54, 1906.)

IV.

Experiments on Animals in Relation to Cancer Research.

In the matter of cancer, it is important to point out that before the establishment of research laboratories, and up to 1902, cancer had been studied almost exclusively from the clinical aspect, and by pathological, histological, and chemical examinations of the dead cancerous tumours. The result has been that our ignorance as to the cause and origin of cancer at the beginning of the twentieth century was almost as profound as ever before. Hence the importance of studying living cancer-tissue by transplanting it from animal to animal; and mice were selected as the subjects of the great majority of the experiments, because the disease occurs spontaneously and frequently in them.

To keep the tumour-tissue alive, and to provide a sufficient and continuous supply of it, large numbers of inoculations are essential:—

- (a) Because many of these inoculations fail entirely;
- (b) because in many instances the tumours which arise and for a short time grow in mice, subsequently atrophy and disappear;
- (c) because of the small amount of tumour-tissue obtainable from each mouse;

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- (d) because the step by step investigation of the changes which take place at the site of inoculation entails the sacrifice of a large number of animals at every stage; and
- (e) lastly, because the investigation of the secretions and body fluids of cancerously affected as compared with unaffected animals requires the use of a large number of mice.

If it be asked what, up to the present time, has been learnt or confirmed by all these experiments, the answer is:—

- (1) Positive knowledge has been gained that this mouse tumour, which is identical in nature with the human mammary tumour, grows by an inherent growth of its own, and is not due to a change in the tissues themselves of the animal into which it is engrafted.
- (2) That cancer can, and does, not so very infrequently, disappear spontaneously.
- (3) That the animals in which cancer has grown and then disappeared are immune to fresh inoculation.
- (4) That a mouse can be rendered immune by the injection into its body of some of the blood of a healthy or an immunised mouse.

It should be pointed out in reference to these inoculation experiments that they are practically painless; or, if pain can be said to be caused at all, it is but slight and momentary, and to be compared in the human subject with the pain caused by a simple subcutaneous puncture.

Neither is pain suffered by the animal in which the transplanted tissue grows even into a large tumour. It is only in the exceptional cases, in which the tumour ulcerates, that any pain occurs, and as this condition renders the tumour useless for further experiment, the animal is at once killed.

Evidence as to the painlessness of these growing tumours is afforded by the activity of movement, sleekness of coat, and general healthy appearance of the mice, their indifference to pinching or pressure of the tumour, which is not innervated; and further, by the fact that when they have been kept in separate compartments of the same cage they have eaten through the partition, mingled with one another, and a litter of young ones has been the result.

Every cutting operation in these investigations is performed under complete anaesthesia, and precautions are always taken to secure a septicity and immediate healing of the wound.

As regards other animals, inoculation experiments have been undertaken to a very limited extent on horses, dogs, rats, rabbits, frogs, and fish. Only in the case of the rat and dog have inoculations been followed by the development of the disease, and to these the same remarks apply as in the case of the mouse.

Any limitation placed upon the number of inoculations, whether in mice or in other animals, including anthropoid apes, to which resort may ultimately be necessary, will have a disastrous effect on the further development and the prosecution of cancer research in this country. It is at present impossible to determine how far the contrast between the success obtained in mice and the failure in other animals, is due to the discrepancy in the number of inoculations made.

As a member of the Executive Committee of the Imperial Cancer Research from its outset, I have had frequent opportunity of witnessing the complete absence of suffering in animals the subjects of these cancerous tumours, and of observing the extreme care and attention given to all the animals, both in London and at the farm.

Experiments on Animals under Certificates B. and C.

The foregoing observations have had reference almost entirely to inoculation experiments; but I should not complete the description of the importance of vivisection to the progress of surgery were I to omit altogether the mention of experiments of a larger kind.

I must refer in this connection to the localisation of the motor centres in relation to brain abscess, brain tumours, and brain injuries, and to the operation of trephining for these conditions.

I would not for a moment wish to imply that much had not been learnt as to the localisation of function from clinical observation apart from experi-

ments on animals; but I do not think it can seriously be contested that the extent and rate of acquisition of our knowledge of this subject, the precision of our knowledge, and the confirmation of what had been otherwise learnt, have been most important results of experiments on the brains of animals.

When it was pointed out that it was, possibly, not so much the original escape of bile, after rupture of the gall-bladder, etc., which brings about the fatal result, as the continual filtration of bile into the peritoneal sac, the question arose as to how far surgery could afford aid in preventing the further escape of bile in this kind of injury. To this end M. Herlin and M. L'Anglas made experiments on dogs and proved that these animals could live after their duct had been tied or the gall-bladder removed. Laparotomy in man followed by suturing the gall-bladder, tying its duct, or removal of the gall-bladder, has since saved a good many lives.

Dr. V. Czerny, to investigate the question whether the larynx could be extirpated without death resulting, performed a series of experiments on eight dogs, all of which underwent total extirpation of the larynx. The first four died from the consequences of the operation. The last four survived and remained well. It having been thus shown that the larynx is not indispensable to life, and that the operation is feasible, extirpation of the larynx became a recognised surgical operation. To what perfection it has been brought, may be seen from the results described in the latest contribution on the subject by Dr. Chevalier Jackson, of Pittsburgh ("British Medical Journal," November 24th, 1906). Of eight consecutive cases of total laryngectomy for laryngeal cancer not one patient died from the operation; one lived 7 years, one 3 years, one apparent cure was lost from observation, three recurred within one year, and one is too recent to record. One died from alcoholism eight months after operation. It deserves particularly to be mentioned that once the feasibility of removal of the larynx having been shown, Czerny's experiments have never, so far as is known, been repeated.

The repetition of operations of an experimental kind on animals has, however, often been the prelude to the adoption of most important operations on man.

Take, for instance, nephrectomy or extirpation of a kidney: Zambeccarius in 1670 and Roonhuyzen in 1672 and several others in the beginning of the 19th century, had shown by experiments upon animals that life could be well maintained after removing one of the kidneys. Still, when Gustav Simon of Heidelberg in 1869 wanted to relieve a woman of a distressing condition, and could only do so by removing the kidney, he was reluctant to operate without first making experiments for himself. He knew that pathology afforded him many instances of persons living after one kidney had been rendered gradually useless by slow disease, but he wanted to confirm, if he even knew of them, the experiments by others which showed that the sudden withdrawal of the functions of one organ could be physiologically tolerated. He wanted also to know what were the immediate risks of the operation as distinct from the risks of interfering with the urinary functions—the risks namely of hæmorrhage, of embolism of the renal vein, of pyæmia, etc. Hence his careful observation of fifteen hysterectomies on bitches contrasted with the same number of nephrectomies on dogs. (Hunterian Lectures, 1898, H. Morris.)

Again, when the numerous operations on the stomach and intestines for short circuiting, excision, etc., were rendered possible by the great safety with which the abdominal cavity could be opened and dealt with under aseptic precautions, surgeons were anxious to test for themselves the various methods and modifications of methods, for suturing the bowel, though these or some of these had been employed in experiments on animals, as well as in cases of injury in man, by surgeons of some centuries before.

It shows only a praiseworthy and natural solicitude and anxiety on the part of the surgeon that, before adopting a new operation on a human being, he should, as far as possible, test it beforehand on one of the lower animals under conditions and circumstances as nearly as possible similar to those of his patient.

7727. I understand that there are some matters which you would like to mention in addition to what you have stated in your *précis*?—In the first place I should like to add—it may have been already before the Commission, and, if so, I hope you will stop me, but it may be of some interest if it has not—the matter of anti-tetanic serum. Tetanus has been, as you may know, a

very common affection after wound inflicted during the celebrations of July 4th (Independence Day) in America.

7728. It was mentioned by some witness, I think, and I think you mentioned it in your own *précis*?—Yes, but I have got some figures here, if you care to hear them. For instance, the use of the prophylactic anti-toxin was introduced in 1904. In 1903 the number of these tetanic cases (this, of course, only goes to a certain extent as being valuable) was 415, and in 1906, after the use of anti-tetanic serum for all those who have been wounded or injured, the number of cases was only 89.

7729. Are you speaking there of New York?—Yes. Of course it depends, you will say, upon the number of people injured. I do not think I have quite got that out—I mean the proportion of injured to those who get tetanus; but there is the fact that the number of tetanus cases in 1903 was 415; and (after the use of the tetanus anti-toxin) in 1905 there were 105, and in 1906 there were 89.

7730. (Dr. Wilson.) Would they be wounds derived from fighting or from what is called gravel rash?—Mostly from pocket-pistol wounds, fire-works, and things of that sort—small wounds. They pick up in the streets bits of anything, bits of wood or paper, or anything of the sort from the dust and mess in the street, and put those into the pistols and fire these off as a sort of sport, like back-scratchers in our own streets; and those produce a certain number of wounds; and being produced though by small force, yet by an article which is impregnated with dirt and soil, they are very prone to produce tetanus. And then the introduction, for all those persons so wounded after these celebrations, of the anti-tetanic serum has had that effect in reducing the number of cases which occur.

7731. But would not a great deal depend upon the condition of the streets and the kind of weather—whether it was a fine day or a dirty day?—That does not touch the point, so far as I see. The question is, the liability of wounds to tetanus derived from a particular source, and here is the same cause one year as with the other, producing wounds of the same kind; and by the use of the anti-tetanus serum the proportion of cases of wounded persons who get tetanus as the result of those wounds is much diminished.

7732. (Chairman.) I think what Dr. Wilson meant was when there were 89 it may possibly have been a wet day, when all the dust and dirt was kept down, and it did not therefore cause the same number of cases; there may have been over 100 people wounded, and 90 per cent. of the 100 may have died; whereas in the other case it may have been a very dusty day, when the tetanus bacillus was very much about, and the 400 may have shown a less mortality per cent. than the 90. It is subject to that observation?—Of course, the 4th of July comes in the same time of the year, and generally under the same kind of conditions as to weather in America.

7733-4. (Mr. Tomkinson.) Were all these cases treated in the public hospitals? Do you know they are all treated with this serum?—I do not know that they are all treated. I know that they endeavour to treat all that are thus wounded. It is some time since I looked at any of these papers, because at the last moment I was rather suddenly summoned before this Commission; but I find, looking down a little lower in my notes, that I have the numbers wounded. The tetanus cases have diminished on the 4th July, but the number of wounded have risen from 4,449 cases in 1903 to 5,466 in 1906.

7735. (Chairman.) That quite answers Dr. Wilson?—And then, with 4,449 wounded in 1903, we have 415 cases of tetanus, and with the 5,466 in 1906 we have 89 cases of tetanus. The reference to these figures is "The Journal of the American Medical Association," August 18th, 1906, pp. 505-8.

7736. (Colonel Lockwood.) Is that in the United Kingdom?—No; in America.

7737. That is why you mentioned the 4th July?—Yes.

7738. (Dr. Wilson.) But do not you think it is possible that they had such a fright in the first instance that they would feel more careful about cleaning any wounds?—These statistics have only reference to a year or two, and it is difficult to arrive at any absolute conclusion from them perhaps. I do not want to press the point further.

7739. (Chairman.) I quite see the point. You admit that you cannot take it from the figures precisely—that the argument does not go up to the figures, as it were; but that a general presumption arises from the figures?—Yes. Without taking it as absolutely accurate it is a very striking effect.

7740. And allowing for other possible causes?—I refer on this same subject to a paper by Scherck. He gives a record of 291 injuries caused by these toy pistols in America during the 4th of July celebrations. The injured persons were immunized by the antitetanic serum without a single case of tetanus occurring, and Dr. Scherck compares these results with those obtained in a year when no serum was used, and in which nearly one-third of the injured succumbed to tetanus. That is merely in support of that statement. Then there is another point I thought it would be of some value to refer to, and that is, in the matter of the treatment of anthrax, or wool sorters' disease, or whatever name you know it by, by means of the anti-anthrax serum —

7741. Are you speaking of anthrax in mankind?—Yes. I have here from Dr. Legge, of the Factory Department, a letter, and also a copy of the Annual Report of the Chief Inspector of Factories and Workshops. Perhaps that is in the possession of the Commission. I only want to draw attention to what he says there. He says: "There is, I think, a consensus of medical opinion that in cases of cutaneous anthrax it" (that is this serum) "is a very valuable remedy. In internal anthrax administered subcutaneously it did not prevent a fatal issue in any case last year." A little lower down he says that in one case in which the serum was used the person recovered, and he gives here a table where there are cases of recovery after the use of the serum; and he gives me in a private letter which I have here some further information, which, perhaps, it will really be shortest if you will permit me to read.

7742. If you are only going to state what Dr. Legge says, I may tell you that he is coming himself as a witness?—Very well; but I can say it is distinctly favourable, and that at St. Bartholomew's Hospital now the only treatment in the last few cases of anthrax that they have had there has been the introduction of this serum of Selavo.

7743. (Colonel Lockwood.) Are they all successes?—They are all successes; and Dr. Legge will give you a table of cases which are more recent than those which he gave in his Milroy lectures before the College of Physicians in 1904 or 1905.

7744. (Sir Mackenzie Chalmers.) He has been working at it during the last three or four months?—Yes. Then another thing, perhaps, also has been before the Commission, and I pray you stop me if it has—that is with regard to the work in the Japanese Army.

7745. (Chairman.) I do not think we have had anything more than a casual reference to it?—Baron Takaki has said: "As one example of the results and terminations of treatment I give you a short account of the hospital established at Hiroshima. The hospital was established in April, 1904, and the report covers the time up to November, 1905. During this interval the hospital admitted 203,782 cases, whence 162,885 were transferred to other hospitals; but the average number of patients in the hospital was about 5,000, and the largest number the hospital had at a time was 10,000. Almost all serious cases were kept in the hospital, because they were unfit for transportation. Yet the result was so good that the ratio of death and invalided is a little above one per cent. Almost all the wounds of the soft tissues healed within ten days. Those with injuries to the bone have obtained favourable results, excepting those who were wounded at the siege of Port Arthur. The many men who were wounded in the head and chest have recovered from the wounds received; and there were also many cases of aneurysmal varix, wounds of nerves requiring operations. The number of operations performed at the hospitals amounted to more than 3,500." That is from Takaki's "Military Hygiene of the Japanese Army."

7746. I suppose something would depend upon the race there. You might have a race which had very few unhealthy members in it?—There, again, my Lord, other factors come in, of course, but still, from what we know of armies of all times and in all countries, it is a very remarkable result. These results ought to be contrasted with what obtained in the Franco-Prussian War. Nélétou, the famous French surgeon, in despair at the high death-rate attending almost all operations

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on the wounded, declared that he who would conquer purulent infections would deserve a golden statue. That golden statue is really almost deserved when you find under the circumstances of war that only one per cent. out of such a very large number of wounded and operated upon were fatal.

7747. He does not give the percentage in the Franco-Prussian War?—No, not here. Those figures are to be easily obtained, however, of course.

7748. (Dr. Wilson.) Have you any statistics with regard to the South African War?—No, but they are published, I believe.

7749. They are not so satisfactory as the Japanese?—I have not got them here.

7750. (Chairman.) That is the percentage of those who died of wounds—not of disease?—These are wounded, yes. Then I would like also to mention a statement which I have had from Sir Samuel Wilks, who was the pathologist at Guy's many years ago. He writes, and says: "The discovery of impurities getting into the system being the great cause of the mortality after injuries and operations, led to the improved modes of dressing introduced by Lister. So that the greatest operations may now be performed with safety. All this is well known to the surgeon, but it came home to myself in an almost startling manner from what I witnessed in the post-mortem room. Some time before, when writing a paper on pyemia, I had no difficulty in collecting a hundred cases which had occurred in a very short period previously, when suddenly this terrible malady disappeared, it was gone, never to return." He says he had the greatest difficulty in finding in more recent researches any records of pyemia that have occurred recently.

7751. Of course, the importance of all this to us is only with reference to how far this is owing to experiments on animals. You have already given us your view about that?—Yes, and these are further illustrations—that is all. There is one other matter that I think I have not mentioned in my paper, which I should like to say one word upon, and that is, the desirability of retaining the power on the part of lecturers on physiology of giving demonstrations to classes. I think it is a matter of importance that they should still have that power. These exhibitions are of very great importance in teaching classes.

7752. Is the importance that you attach to that with reference to enabling students to follow what the structure or the nature of the disease is from inspecting the animal, or to teach them how to operate?—I am not suggesting that there should be experiments on animals for the purpose of teaching how to operate; but I am suggesting that it is very important to retain the power that still exists on the part of physiologists to give a certain small number (and they always are but few) of demonstrations to classes who are learning the functions of the body; for instance, to know what the action of the heart is like and what the action of the intestines is like, and the effect of influencing the organs through irritation or section of different nerves. There are a few things of that sort which can only really be fully and completely impressed upon the student by having practical demonstrations of them; and they are performed in small classes (not in a great theatre), before a group of students, upon animals, which are under an anæsthetic and are killed as soon as the experiment is over, and before they are allowed to come round from the anæsthetic. This is merely one of the things that concerns the teaching of the sciences of the profession to which many of us attach a good deal of importance.

7753. Did those demonstrations and experiments take place in your student days?—Yes, I believe Guy's was one of the few places in London where they were shown, but I was a pupil of Dr. Pavy at Guy's Hospital, and Dr. Pavy used to show us a few of these things. There were not many experiments, perhaps only four or five in the course of a whole session; but I can say from personal experience how one was impressed by the sight for a few minutes of a thing of that sort, which no amount of reading or verbal descriptions or lectures would ever have given to one.

7754. Was that before or after 1876?—It is so many years ago that it goes back to before 1876.

7755. Before the Act of 1876?—It goes back to before the Act of 1876, because it must have been in 1867 or thereabouts. I do not remember quite the exact date. Among the questions that were put to some of the wit-

nesses in 1876—I think to Sir James Paget and Dr. Pavy himself—one had reference to a certain regulation which is printed in the College of Surgeons' Calendar in the regulations relating to the education and examination of candidates for the Fellowship. It is there stated that a course of practical physiology must be attended at a recognised school, and questions were asked as to what practical physiology meant. We have, in our Calendar since 1874, had the following note: "By this is meant that the learners themselves shall individually be engaged on the necessary experiments, manipulations, et cetera, but it is not hereby intended that the learners shall perform the vivisections"; and in case these regulations before you now I should like to explain that although we expect them to have seen the experiments, we do not expect them to perform them themselves.

7756. That I quite understand?—Perhaps 20 men will see an experiment which is shown by a lecturer.

7757. (Sir Mackenzie Chalmers.) Who is a licensed person?—Who is a licensed person, and who is, of course, an expert in the manipulations required for; but they are not to be performed by the students individually.

7758. (Colonel Lockwood.) Not necessarily?—No.

7759. They could not do so without a licence?—No.

7760. (Dr. Gaskell.) Is that regulation still necessary for the Fellowship?—Yes; it still appears in this set of regulations, but it has that note in italics.

7761. There is no such regulation for membership?—No.

7762. It is for the Fellowship?—Yes.

7763. (Sir Mackenzie Chalmers.) What is the actual number of Fellows of the Royal College?—There are either 1,300 or 1,350—I think 1,350. But the number of men who come up at each batch—and the examinations is held twice a year—perhaps varies from 50 to 70.

7764. (Dr. Wilson.) Would it necessitate a certificate from the teacher that the candidate for the Fellowship has attended such demonstrations?—Yes.

7765. (Chairman.) Is that all you wish to add?—If you please.

7766. (Colonel Lockwood.) How is it known that experiments on living animals are going to take place at these lectures? Are they called "A Course of Physiological Lectures," and it is understood that they will include experiments?—As I have said, only on very very few occasions are there any experiments on animals, perhaps only on four or five, or, at the utmost, half a dozen, in the course of a winter session.

7767. How will the men know?—Those men who are attending the lectures would be informed at the end of a class—this is the way I have known it done—that at the next lecture, or the next time of meeting, there would be this or the other shown upon a living animal.

7768. Now, supposing I myself, or we will say Sir William Collins, or any member of this Royal Commission, wished to see an operation on a live animal—you will understand my motive for asking the question—and that we did not wish the demonstrator to know that we were coming, how could it be worked? You see my point. My point is that if the man was ready for me other people might say, "He knew that you were coming"?—You mean a surprise visit.

7769. Yes?—Of course, the inspector can go in at any time, there is nothing to keep him out; and the object of keeping anybody else out would merely be in conformity with that section in the Act which provides that these things are not to be for public exhibition. But if you were to go to and say, "I am very much interested in seeing this experiment," there is not a lecturer in London or in the country who would not allow you to come in and witness it, I feel sure.

7770. But you see my point. Other people might say, "It is quite true that you went, and you saw the experiment; but the man, of course, knew you were coming, and so he conducted it more humanely than if you had not been present"?—I do not think anything of the kind. The Commission must take my opinion for what they think it is worth; but I feel satisfied that there is not a single lecturer in the country who would vary his experiment because there happened to be a member of the Commission or a member of the Anti-vivisection Society or of one of the Anti-Vivisection Societies present.

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7771. I am quite content to take it so.

7772. (*Chairman.*) It is really a question of the amount of anæsthesia what Colonel Lockwood is aiming at, whether the anæsthetics are always applied fully so as to ensure the absence of pain?—I believe, most certainly, that they are. I should think the truth is that there is probably a waste of anæsthetic—that there is more anæsthetic given rather than that there is too little. In the experiments that I have seen, sometimes when it has been known that I was going to be a spectator, and sometimes when it was not, it has seemed to me that there has been rather an over-administration of anæsthetic instead of an under-administration.

7773. (*Sir Mackenzie Chalmers.*) More than you would give to a human being?—Yes.

7774. And which would entail danger in the human being?—Yes. You see, if a dog dies, it does not matter, because it is going to die at the end of the experiment.

7775. (*Colonel Lockwood.*) You gave some evidence about the Japanese. Do the Japanese make experiments on animals?—I cannot answer that; I should think they do.

7776. But you do not know of the existence of any experiments on animals?—I do not.

7777. But you would, I suppose, allow that a nation like the Japanese, who are more, I was going to say vegetarian than carnivorous, are more likely to recover from wounds, as the Indians do, than a carnivorous European?—Yes, I should think there is much in that.

7778. If we grant that Lister's and Pasteur's discoveries are facts, do you see any necessity for continuing experiments bearing on those discoveries of Lister and Pasteur?—I think there is every reason, every necessity, if we are to go on progressing in medicine and surgery, that these experiments must be continued.

7779. On the lines of Lister and Pasteur?—Yes, certainly.

7780. To perfect and continue?—Yes, and to deal with and to combat other diseases.

7781. Do you claim that these injections are a cure for tetanus, and cholera, and anthrax?—Yes.

7782. You would go so far as that?—Yes.

7783. You made your name, I think, which is a very great one, by experiments on the kidney, was it not?—I never made an experiment in my life.

7784. I do not mean on an animal. I mean operations—I apologise?—Oh, no need for that. I thought perhaps you meant experiments on animals.

7785. No, I meant operations?—Yes.

7786. Were those operations which you have conducted so skilfully, and fortunately discovered, and were you made to do them, by experiments on living animals?—Personally, not at all. The operation that I did in the first place, and was the first to do, was done quite irrespective of any experiment upon animals; that is the cutting of a stone out of a kidney. But that was not the case with regard to cutting a kidney out of the body, because Simon of Heidelberg, who was the first to do that, made a deliberate set of experiments for the express purpose of knowing various things before he submitted a patient to the operation. He, of course, knew what the effect of the loss of a kidney by disease (a slow process) was; but he wanted to know what the loss of a kidney, by being taken suddenly away, would be, and until he knew that he did not feel at liberty to perform the operation of cutting the kidney out. But the kidney which he proposed to remove was healthy—I am speaking now historically of the first operation—and the operation was deliberately planned for the removal of a healthy kidney from the human body.

7787. Total excision?—Total excision; it was a healthy kidney, and the tube running from the kidney to the bladder, which had been cut in the course of previous operation accidentally, had its end sticking out of the flesh. The consequence was that the poor woman was constantly suffering from the annoyance of urine trickling down her side or down her front, and she implored for relief from this by having her kidney removed. That led Simon of Heidelberg to

make his experiments to find out what the amount of shock would be, and what would be the risk of producing that form of blood poisoning which is known as uremia, and what would be the effect upon the health by the sudden taking away of a kidney that was otherwise healthy, except for this leakage from this tube. Accordingly, he made these experiments.

7788. He made those experiments first on an animal?—He made those experiments first on dogs and other animals, and then he applied the results to this living female patient.

7789. As regards your particular operation, you did not do that from knowledge acquired by experiments on a living subject?—Not at all from experiments on animals; but from what I had learnt by experience from operations on the liver and other organs in the human subject.

7790. Have you witnessed any or many experiments under licence?—No, not many. I have seen a fair number. In quite recent years I have seen some, since last year, for instance.

7791. Have you ever witnessed any cruelty on the part of a demonstrator?—No, absolutely none.

7792. Or any levity on the part of those who witnessed them?—No.

7793. And always under an anæsthetic?—No, not always under an anæsthetic; it depends upon the class of experiment. When I have seen an experiment to show any effect of blood pressure, or things of that sort, the animal has been completely anæsthetised; but when I have seen experiments on guinea-pigs and rabbits, or what not, for the purpose, for example, of seeing the effect of the injection of serum (some I saw the other day) those were not done under chloroform.

7794. Have you seen any of those experiments on the eye?—I have not seen any experiments on the eye.

7795. But where severe pain would have been inflicted, anæsthetics, according to the licence, have always been administered?—Yes, absolutely.

7796. And the animal destroyed before recovery?—Yes.

7797. Do you look upon morphia as an anæsthetic sufficient to prevent, more than dull, pain in a living subject, used for the purpose of an anæsthetic?—No.

7798. Would you say that you could not give an animal enough morphia to make it totally insensible to pain, if that was the only anæsthetic used?—I do not think it ever is the only anæsthetic used. I bear in mind, of course, that at one time it was the great means of rendering the human subject anæsthetised before the introduction of chloroform and ether; but I think that now morphia is given perhaps as an adjunct to the administration of an anæsthetic. I would rather not express any very definite opinion about it, because, as I say, it does not come into my way; but just as I know in the case of the human subject, so I think in regard to animals; if an experiment were wanted to be done on an animal where a very small quantity of anæsthetic was given, where as little anæsthetic as could be should be given, then, I think, morphia would be a very valuable adjunct administered before the administration of the chloroform. But the complete anæsthesia would be the same with a less quantity of chloroform, if the morphia had been previously given.

7799. But you would not look upon it as a complete anæsthetic, even given in great quantity?—No.

7800. Do you think that there is enough resemblance between animals and human beings to make experiments on living animals useful?—Yes.

7801. I may take that generally from what you have told us?—Yes.

7802. How long can an animal be kept under anæsthetics, would you say?—I have seen an animal kept under chloroform, I suppose, for two hours.

7803. At a stretch?—Yes, at a stretch, but then I have seen a living human subject often kept for as long a time.

7804. (*Sir Mackenzie Chalmers.*) What is the longest time with a human subject that you have seen yourself?—I could not answer that question off-hand, but I should think the longest time I have ever seen a

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human subject under anæsthetics was between three and four hours for the treatment of complete compression of an abdominal (aortic) aneurism.*

7805. (*Colonel Lockwood.*) Up to the present time do you think there has been any hindrance to science and to your profession generally by the regulations issued by the Home Office for experiments on living animals?—No, I cannot say that I do think so. I think the present Act of 1876, under which vivisectional experiments are done, was amply protective against cruelty to animals and sufficiently free and liberal for the due prosecution of proper scientific and physiological inquiry.

7806. Would you object to any further regulations?—Yes. There is one respect in which I should like to see greater freedom. Under the Act, of course, the Home Secretary can grant a licence to anyone. I should certainly like to see the thoroughly trained first aids in the laboratory permitted to do such very slight and mechanical operations as the injection of a small quantity of serum. There are a very large number of animals treated by this serum injection, and if you are injecting a considerable number of them it requires, as things are now, a highly-skilled man to spend a great deal of time in the work of just injecting the serum which these men who are trained assistants in the laboratories could do quite as easily and quite as completely and safely as the director himself does. It is a great waste of time, and is a great hardship upon the director to have to do all that simple routine work.

7807. (*Sir Mackenzie Chalmers.*) What you mean is that you think the licence should cover Certificate A, so to speak, for the assistants?—Yes.

7808. It is only Certificate A you are referring to?—Yes.

7809. You think that when a man of established character holds a licence and Certificate A, the Certificate A should extend to any able assistant of his?—Yes, either on the recommendation of the director, or it may be by having a certificate filled up as now for the director himself; but I certainly think that power should be given to a man who has been for a long time, and is, highly trained in this kind of simple laboratory work, to do it, merely to save labour to the scientific man himself.

7810. (*Colonel Lockwood.*) I suppose I may take it that you naturally would object to the exclusion of dogs from experiments?—I should certainly.

7811. You think it would be a hindrance to science?—Yes, a very material hindrance.

7812. You have read, very likely, the evidence of previous witnesses before this Commission?—I have.

7813. Are there any remarks you would like to make as regards the evidence of the witnesses who are opposed to vivisection?—If I had the material before me I could write pages of objection.

7814. But nothing strikes you particularly as very false or very wrong?—That is rather too wide a question, I think, to put to me, because I have not it all quite at my finger-ends.

7815. Of course, you know the very strong feelings that exists in the minds of many people as to the propriety of any experiments on living animals?—Yes.

7816. What do you think is the foundation of that feeling? Do you think it is simple romance, or that cruelties have existed and do exist abroad, undoubtedly; or what do you think is the foundation for this very strong prevailing feeling?—There are many words one could apply.

7817. Ignorance, I suppose is one?—I should rather not use any words to give offence, but I think it is not unlikely that a good deal of the feeling against all forms of experiments on animals, is due largely to what you have alluded to, the, shall I say, recklessness and the indifference perhaps on the part of some, with which experiments have been reported as having been done, and I believe have been done, in some of the places abroad. I think that that having been spread abroad, and not having lost in disrepute by travelling, is very largely the cause which has led people who have not looked at the thing and have not considered the thing all round, and who do not really know the way in which things are conducted in this country, to take the objection that they have taken.

7818. You do not believe as a man of science, and I

have no doubt as humane a man as myself, that there is any real foundation for this strong feeling that exists?—I do not believe there is an iota of foundation.

7819. (*Sir William Church.*) You told the Commission, in answer to a question by Colonel Lockwood, that you do not consider morphia an anæsthetic?—Yes.

7820. But do you think that an animal, a dog or a cat, which receives a poisonous dose of morphia or opium is in a condition to feel pain up to the time that death occurs?—No, I do not think so. They suffer no pain.

7821. But so far as sensation goes, there is perhaps no anæsthetic that we know so much about as morphia or opium, because we know that when a man recovers from a nearly poisonous dose, and recovers with difficulty, he has been quite insensible to all that has happened to him during the time that he has been under the action of the drug?—Yes, we know that equally as we do with regard to chloroform and ether.

7822. Therefore, when you told the Commission that morphia was not an anæsthetic, you meant that if used as a perfect anæsthetic, the animal would not survive?—Yes. I was assuming that Colonel Lockwood meant to ask me whether I thought that in the case of an operation done upon an animal to which morphia had been administered, the intention being, of course, that it should have an affect for the time and the animal should live afterwards, it was as sufficient as chloroform, and I said that I did not think it was. But if you are going to give a poisonous dose to an animal, then of course the operation might be done and the animal would not feel pain.

7823. I wished to put the question because we have been told here by several witnesses that opium is not an anæsthetic, and therefore even when a poisonous dose is given, the animal is probably suffering tortures until death occurs?—That is not so.

7824. (*Sir William Collins.*) I think you said that you had never yourself held a vivisection licence?—No, I have never performed any experiment.

7825. Nor have you practised any vivisection?—No.

7826. Nor have you used vivisection, of course, for demonstration purposes?—No.

7827. Nor for obtaining manual skill?—Certainly not.

7828. I gathered from one answer which you made to the noble Chairman that you rather objected to the use of vivisection for the purpose of obtaining manual skill?—Those are my feelings. I do not think it is necessary.

7829. Should I be right in thinking that many practising surgeons practise vivisection?—I should think not for that purpose.

7830. Or for any purpose?—Well, there are surgeons who practise vivisection no doubt occasionally for the purpose of ascertaining the effect or the technique of a new operation, but not as a means of demonstration, if that is what you are asking me.

7831. Shall I be right in thinking that there are many practising surgeons who have never practised vivisection?—Surely yes.

7832. I only wanted to get it from you as President of the Royal College of Surgeons, whether that was the case or not?—That is so.

7833. I understand that the Council of our College which you represent is not anxious to see, with the single exception that you mentioned of the permission to assistants in laboratories to perform minor injection operations, any modification in the existing law or practice?—I think they are content with it as it is.

7834. Shall I be right in thinking that they would regret a modification of the existing law?—I am sure that you would, if by modification you mean curtailment.

7835. Of such protection as is accorded to animals by the existing law?—I thought you meant curtailment in the way of privilege to perform experiments. I think they would regret that, but I think that they would equally regret to see any curtailment of the proper and due protection of animals.

7836. They recognise, in fact, that vivisection should be practicable, but that it should be practised under restrictions?—Yes.

* The exact time was 4½ hours. For particulars see 56th Vol. of the *Medico-Chirurgical Transactions*, p. 390. 1873—H. M.

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7837. I gather from your *précis* that you are anxious to state that in your opinion the use of experiments on animals has been a part cause, not by any means the whole cause, of the great improvement to which you have borne witness in the course of your examination?—That is so.

7838. And that you find a difficulty in disentangling the causes that have been at work in affecting that progress?—Yes.

7839. I think you state that it is practically impossible to allocate with precision that which has been due to vivisection and that which has been due to other causes?—Yes, I think so.

7840. That is to say, that you would not wish to underrate the importance of clinical observation, pathological anatomy, and so forth, which have also contributed to that end?—Certainly not.

7841. In the return which you put in which has been prepared by Mr. Watson Cheyne, you place Pasteur's work on spontaneous generation as the fundamental basis upon which the modern aseptic and antiseptic treatment of wounds has been erected?—I do, together, of course, with Lister's work.

7842. As one of the fundamental supports of that superstructure?—Yes.

7843. Should I be right in thinking that Professor Bastian has recently again contested the alleged conclusions of Pasteur as to spontaneous generation?—It is quite certain that he has done so.

7844. That is to say, that it is still an arguable question, so far as recent literature is concerned?—Yes, in his own opinion.

7845. And as regards Lister's work I suppose one would be right in thinking that there have been considerable modifications in the Listerian doctrine between 1876, when as Mr. Lister he gave evidence before the Royal Commission, and the present time?—I do not quite follow that.

7846. For instance, in regard to the use of the spray and drainage, and so forth, there has been a very considerable alteration in technique, if not in the principles upon which Listerism was based?—The technique of the employment of facts which he has laid down, the technique of the employment of the treatment which is based upon the facts which he has discovered.

7847. Should I be wrong in thinking that Lister laid great stress upon the use of the carbolic spray and drainage?—Quite so. Those are all modified now. The spray we know is an abolished thing.

7848. I notice that in this same Table which you have handed in, which has been prepared by Mr. Watson Cheyne three matters are marked with a x, which implies "impossible without experiments on animals." They are "immunity," "serum therapy and immunity," and "increasing the resisting power of the body"?—Yes.

7849. From your own personal experience would you say that surgery has derived great advantage from those three matters?—I should, certainly.

7850. What would you instance as the most striking example?—On which point?

7851. On any one of the three. Take opsonins, for instance?—I should say that the case I have mentioned with regard to the use of the anti-tetanic serum is an instance of immunity, and as giving increased resistance to the body; and so with regard to anti-anthrax serum.

7852. I was asking from your own experience?—Yes.

7853. That was in America, as I understand?—Yes, but one knows of it in cases in this country with regard to the treatment of tetanus.

7854. Have you yourself used it in many cases of tetanus?—No, not in many cases, because I am happy to say that I have had very few cases of tetanus to deal with.

7855. But you have used it?—Yes.

7856. Could you say in how many cases?—I think only in three cases. I have had very few cases of tetanus; they are not common cases in this country.

7857. Do you use it with a view to prevention or cure?—Cure.

7858. Was it successful in those three cases?—I am sorry to say it was not; that is a long time ago.

7859. Not in any of them?—No, they were all advanced cases; they were cases too late for the successful employment of treatment.

7860. Should I be right in thinking that it is generally stated now that the treatment is likely to be valuable rather as a preventive than as a curative?—Yes, although there have been cures up to quit recently claimed for the employment of the serum.

7861. In the figures which you gave us with regard to the 4th July in America, I do not think you stated whence those figures were obtained—on what authority?—I referred to one paper which is here on the memorandum that I mentioned. I am not quite sure about the other figures. I could tell you if you wished to know, but I think they came from an Annual Report of Surgery.*

7862. Are they official figures?—I do not know about their being official figures, if you mean by that that they came from some Government office.

7863. Do they cover the whole of the United States?—I cannot answer that question either without further reference, because what I have is just an extract merely as to the figures.

7864. Perhaps you will be so good as to add that to your evidence?—Yes, I will.† But I have given the reference to Scherck's Returns.

7865. With regard to what is essential for the diagnosing of tetanus, is it a bacteriological test?—I fear that it is not when the disease is developed.

7866. I mean for the 415 cases of which you spoke in 1903; how would those cases be diagnosed?—I suppose on the clinical symptoms.

7867. Does the Report state that?—That I cannot say; you must allow me not to answer that question, because I could not answer it for certain.

7868. Then you refer to the case of anthrax and some cases at St. Bartholomew's Hospital. Did I rightly understand you to say that in any case in which Slavo's serum had been used in anthrax a fatal result had occurred at St. Bartholomew's?—No. With regard to St. Bartholomew's, it has been used in the last three cases, and I have got a letter here from Mr. Bowlby referring to one or two cases. But I have not taken up the position of stating that in every case in which serum has been used the patient has recovered from the anthrax.

7869. I thought I gathered that in reference to the St. Bartholomew's cases?—In the last three cases of anthrax at St. Bartholomew's Hospital I said that they had relied upon the serum treatment only with speedy recovery of all three patients. In several cases, of course, both in Italy, this country, and elsewhere, and especially in the earlier times of the employment of Slavo's serum, not having confidence enough in the serum, surgeons excised the pustule as well as giving serum, but as they gained confidence with the serum they have done away with the operative part of the treatment and have been relying upon the injection of the serum alone.

7870. There again can you speak from your own personal knowledge of the use of Slavo's serum?—No. I have seen anthrax in Guy's many years ago when I was house surgeon there; but the disease does not come my way now. It is not met with in my hospital.

7871. You referred to the experience of the Factory Department of the Home Office; I do not know whether you have noticed in a recent Report that they point out that cases of anthrax appear to occur without the bacillus being found, and they insist on the importance of having regard to clinical as well as bacteriological evidence of the presence of anthrax?—Yes.

7872. Is it the case that the bacillus anthracis is not always discoverable?—It is not always discovered, but I should certainly say, with the knowledge that I have, that it is nevertheless present.

7873. In the case of anthrax do you think it is safe to argue from the experience in animals, from one set of animals to another and from animals to man?—I think so.

* The reference is "The Journal of the American Medical Association," Aug. 18th, 1906, pp. 505-8.—H. M.

† The tetanus cases occurred in 25 different States of America.—H. M.

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7874. Has it been noticed that certain breeds of sheep are quite unamenable to inoculation with anthrax, such as Algerian sheep, whereas other ordinary sheep suffer severely on inoculation?—I cannot speak as to that. The variety of cases in which sheep are affected in large numbers very largely depends on the particular locality and the fact that anthrax has been buried perhaps a long time ago, but as to different breeds I have no knowledge.

7875. You mentioned the case of septicemia in mice?—Yes.

7876. Is that poison the same when it is inoculated in field mice and house mice?—I believe not.

7877. It is quite different, is it not?—Yes.

7878. (Chairman.) Do you mean that it requires more, or that it gives the disease in one case and not in the other?—It is a different organism that produces the disease in the different animals.

7879. (Sir William Collins.) That is to say that one species of the same animal will not suffer whereas the other will after inoculation with the same poison?—Yes.

7880. You spoke of three points which Pasteur's experiments had established—first the specificity of the microbe, then its variation of virulence under cultivation, and so forth. It is not a fact that experience has shown that organisms originally asserted to be specific have been found by cultivation on various soils to undergo considerable morphological variation?—I think so.

7881. Is it true that many organisms declared to be the causes of certain diseases have subsequently been found to be not so?—I do not follow you.

7882. Take the case of sheep-pox, of which the alleged organism was at one time published with a considerable degree of confidence; was not that subsequently withdrawn?—Yes, and I believe different authorities have taken different views with regard to whether the same thing has been the cause of the disease or not. I believe that is the case now between a member of this Commission, if I may refer to him, and Dr. J. Hamilton.

7883. In reference to what disease?—In reference to the diseases in sheep, especially with regard to braxy and louping-ill, and so on.

7884. And I suppose Koch, by experiments on animals, was led to conclude that the tuberculosis of animals could not be communicated to man?—That was the report that he made here.

7885. As the result of his experiment upon animals?—Yes, but then, of course, there is the opposite view held from further investigations by the most recent Royal Commission in this country.

7886. You rather demurred to my suggestion that sapremia was to be regarded as a form of blood poisoning independent of a micro-organism?—Yes.

7887. Will you tell me what the micro-organism of sapremia is?—I do not think I can at this moment fix on any special micro-organisms.*

7888. In reply to the noble Chairman you stated, I think, that there were bacilli which worked by themselves without emitting any toxin as well as those which operated by way of toxin?—I meant to say there were bacilli whose toxin was the source of the poison and that there were others which by themselves were the source of the poison.

7889. Would you give an example of the latter class?—Tetanus is unquestionably an illustration of the toxin influence.

7890. It was the other class that I wanted?—I imagine that tubercle is largely due to the organism itself as the cause of the disease.

7891. I suppose that you would at any rate contend that there have been great advances in surgical practice which have been made irrespective of experiments on animals?—Yes.

7892. That follows from what you have already stated. The use of anaesthetics would be one of them, would it not?—Yes.

7893. Then the ligature—would that be one of them or not?—Yes, I think the ligature certainly would be. The ligature is a very old means of controlling hæmorrhage long before they thought of antiseptics.

7894. It was used by Ambrose Paré?—Yes, but it goes back to very early history; it was employed, if I remember rightly, by Celsus.

7895. You have drawn a distinction between, or, at any rate, you have mentioned aseptic and antiseptic surgery?—Yes.

7896. Was the late Mr. Lawson Tait one of the champions of the aseptic treatment?—Yes.

7897. Do you remember his views in regard to vivisection?—Oh, yes, I know that he was opposed to it, but then I must say that that would not influence me much.

7898. I was only anxious to know whether you would wish us to take it that what you stated in evidence was a uniformity of view of eminent surgeons or whether there were those who were really eminent surgeons who have taken a different view?—Yes, that cannot be denied, but they are very few in number as far as I know.

7899. Should I be right in thinking that aseptic surgery has taken the place of antiseptic surgery?—I hope so.

7900. You referred to the evidence of Sir James Paget in 1876; did you notice that in reply to one question, No. 382, he states, "I can quite believe that the ardent physiologists put more trust in the experiments on living animals than I should; and certainly those studying therapeutics and diseases think more of them than I should. I think more of the advantage of clinical inquiry. But I am very anxious that there should not be a general condemnation of experiments on animals since it seems to me that there are a number of things actually essential for the life of man, that cannot be ascertainable by other means." Could you associate yourself with that opinion?—To an extent. Certainly I think we all ought to attach the utmost importance to clinical inquiry; but I imagine that if Sir James Paget were giving evidence here to-day, instead of in 1876, he would attach even more importance than he does there to experiments on animals, from the discoveries which have been made since, and from the prospect of further discoveries that will be made.

7901. At any rate, your view would not be that, I suppose, of the late Sir Charles Bell, when he said: "Experiments have never been the means of discovery, and a survey of what has been attempted of late years in physiology will prove that the opening of living animals has done more to perpetuate error than to confirm the just views taken from the study of anatomy and natural motions"?—Although Sir Charles Bell stated that as his opinion at that time, it was not his opinion at other times, and I do not think it was the best representation of what his view really was. Sir Charles Bell made many experiments, and we know what we owe to them, but Sir Charles Bell was a man who felt very deeply the pain that these experiments caused to animals, and I have had from Sir Charles Bell's widow, and from his brother-in-law, the late Mr. Alexander Shaw, a statement that he felt it so strongly that he gave up performing experiments on animals because he hated the sight of the suffering, which they necessarily entailed. But, still, it was a good thing he performed them, because, as you know as well as I do, they were the starting point of our knowledge of the different functions of the nerves.

7902. As bearing upon what you have said, I see that Sir Charles Bell in a letter wrote as follows: "You may think me silly, but I cannot perfectly convince myself that I am authorised in nature or religion to do these cruelties." In other words, it is a conflict between the moral sentiment and scientific experiment?—And the desire to obtain knowledge from scientific investigations for the benefit of humanity. I think all those things entered into Sir Charles Bell's thoughts and feelings.

7903. (Chairman.) Were anaesthetics used at that time?—No, and at any rate they were useless to be used for such experiments as Sir Charles Bell resorted to. He wanted to learn essentially the differentiation in the functions of the nerves as between those which convey sensation and those which convey the impulse to muscular action, and he could not have discovered what he was seeking to arrive at had he not made experiments upon animals without anaesthetics at the time of the experiments; and of course, as they necessitated the exposure of the spinal cord, they were necessarily of a most torturing and suffering character.

* They are the micro-organisms of putrefaction—the bacilli of putrefaction and different species of "Proteus."—H. M.

7904. About what date would that be?—I suppose about 1820. I forget the date of Bell's death.

(Sir William Collins.) He died in 1842.

(Sir William Church.) 1811 was the date of his celebrated pamphlet, in which I think I am right in saying he himself attributed his discovery to the experiments he was making in 1811.

7904A. (Sir Mackenzie Chalmers.) Chloroform was discovered about 1846?—There again I cannot say the exact date.

7905. (Sir William Collins.) The quotations that I put to you are dated 1823 and 1822?—I said I thought it would be about 1826. I had forgotten the date of Sir Charles Bell's death, but it was after he went to Edinburgh, and he went there in 1836.

7906. There is little doubt, is there, that Sir Charles Bell did make experiments, and that he attributed a great increase of knowledge to those experiments, and that he subsequently wrote rather disparagingly of experiments on living animals, because he felt that they were to some extent in conflict with this sense of morality?—Yes, but that he did attach importance to them and that he did find out epoch-making knowledge by experiments I suppose is beyond all question in your mind or mine.

7907. I am afraid at the present moment I am interrogating?—I put that merely as a figure of speech. I suppose it is beyond all question—everybody must admit the epoch-making importance of his discoveries.

7908. Do you think that they need to be repeated?—No. I have only quite recently been asking for further information before signing one of the certificates and schedules as to the necessity of making certain experiments on the spinal cord; because I should not sign the certificate if it was a mere matter of repetition of experiments and for the first-hand knowledge of the individual who wanted to make them. I consider that those experiments ought not to be repeated unless with some very special object in view.

7909. Might I put it to you that *à priori* physiology, which is the science of living things, and surgery, which has so largely to do with wounds, are both likely to be benefited by knowledge obtained by operations on living animals?—I think that both surgery and physiology will be advanced by operations on living animals.

7910. *A priori* it would be very surprising that, physiology and surgery being what they are, standing in the relation that they do to the science of life and the healing of wounds, operations on living animals would not be productive of some additional knowledge by their practice?—It would be very unlikely.

7911. Do you think that *à posteriori* you are able to adduce evidence that that *à priori* assumption has been justified?—Absolutely.

7912. But I understood your view, and that of the Royal College of Surgeons, to be that there should be a limitation by law placed on the practice of vivisection?—Yes.

7913. (Dr. Gaskell.) Might I ask one question with regard to Sir Charles Bell's work. Did not Bell publish later a certain amount of material; could you not say that he published some early papers and some later papers?—Yes.

7914. Is it not the early papers that we now consider valuable?—Yes.

7915. I mean that what is really lasting in Bell's work is that part which he himself attributed to experiments on living animals?—Certainly.

7916. (Sir William Collins.) Would you give us the date as to what you mean by later and earlier?—I would give the date of the earlier papers as those when he was working largely by experiments on animals; I forget the dates.

7917. Do you think that the papers which were published in 1823 were not valuable?—Certainly they were; but I think those were not some of his earliest papers; he went on working here in London till 1836, and then afterwards for four or five years in Edinburgh.

7918. In the earlier papers you would include the papers of 1823?—Yes, although in answering these questions I would not by any means cast, or seem to wish to cast, any want of appreciation upon the later papers, because he wrote upon surgery; for instance, he wrote several treatises upon surgery which were the result of his experience as a surgeon later in life. But my answer to your question was chiefly in reference to those more scientific investigations.

7919. (Dr. Gaskell.) What I meant simply was that those investigations of Sir Charles Bell, which we look upon so highly now, and which have lasted right away to this time, were all done or put forward by him at the time, and in consequence of the experimental work that he was doing?—Yes; and then the later ones that he wrote more especially had reference to his practice as a surgeon.

7920. (Sir William Collins.) You attach value to them all?—Yes, I attach value to those, too, of course.

7921. (Sir Mackenzie Chalmers.) You have brought before us a unanimous resolution of the Council of the Royal College of Surgeons?—Yes.

7922. The practical effect of that is that your Council unanimously support the existing law?—Yes.

7923. With a possible modification in a small detail?—Yes.

7924. Would you kindly tell us how the Council is composed; what is the number of the Council?—There are 24 members of the Council.

7925. Elected by whom or chosen by whom?—Elected by the Fellows of the College.

7926. (Sir William Collins.) Not by the members?—Not by the members.

7927. (Sir Mackenzie Chalmers.) The Fellowship of the Royal College of Surgeons is the highest surgical degree in England, I suppose?—We think it is the highest surgical degree anywhere.

7928. At any rate the Council represent what I suppose might describe as the best surgical opinion in England?—I think I may fairly say that it does, although I am a member of it and therefore do not speak personally.

7929. You are President?—Yes.

7930. I am speaking of the Council?—Yes.

7931. We may take it as expressing practically the great body of instructed surgical opinion in England?—Yes; and I may add that they are all, or nearly every one of them, men who have spent a large part of their lives in teaching surgery as well as in practising it.

7932. I may take it, then, that your opinion is that experiments on animals in the past have been indissolubly mixed up with advance in the knowledge of surgery?—Yes.

7933. And, although it may be difficult to assign the proportions, you think it is absolutely necessary that they should continue for the future?—Yes.

7934. How long have you been practising yourself as a surgeon; you are still practising, are you not?—Yes; my house surgeon appointment was 1867 or 1868.

7935. Your experience as a surgeon goes over 40 years?—Yes.

7936. And during the whole of that time you have been continuously operating and in the operating room?—Yes.

7937. I want to ask you, if I may, one or two questions with reference to suggestions made by previous witnesses. Have you any opinion on this point? When an anæsthetic is administered is there any order in which you can put the disappearance of movement, disappearance of sensation, and disappearance of consciousness?—Disappearance of consciousness long precedes disappearance of movement.

7938. That is what I wanted to know. By consciousness I meant intellectual consciousness, as opposed to mere sensation of pain?—I take them all. Of course, really intellectual consciousness would go first; then the consciousness of pain would go long before the capacity for movement went.

7939. The movements might be even what you call purposive?—Purposive in this way, that they would be made to claw off the thing over the mouth, or made to kick away a hand or any restraining force over the knee—purposive to that extent.

7940. What I meant by purposive was that a person knowing nothing about anæsthetics, seeing an operation for the first time would think the patient was conscious?—Yes, and would think he was suffering horribly. I daresay many people would. I have seen a person, who has not known anything about what was going on, and has not been able afterwards to tell anything about what had been going on, having to be held down by six or eight persons during the latter portion of the administration of the anæsthetic.

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7941. Before the operation began?—Before the operation began.

7942. But absolutely unconscious?—Absolutely; they have not known the least thing of it afterwards.

7943. Does it vary with different individuals?—Yes, because some individuals will not struggle at all—they go off very quietly.

7944. Does it depend upon the nature of the anæsthetic to some extent at all?—It depends upon many causes. One always used to think, especially at Guy's, that people who had taken alcohol and people who had syphilis—I know it was the opinion of one of my teachers there, Mr. Bader—that syphilitic persons were amongst the most troublesome under chloroform, that they resisted the most; I mean people who had had syphilis comparatively recently.

7945. Now, may I take you to the next stage. During the operative procedure, may you, without recovery of sensation of pain, have any movements?—Yes. When a patient is absolutely unconscious to everything and is as still as possible, when you put the knife on the surface of his skin and begin to make an incision, he may struggle or turn, so that you may even yourself turn round and ask the anæsthetist whether the patient is fully under, and he will say, "Oh, yes, quite," and you know that he is.

7946. Are there any special nerves or muscles which are apt to contract and cause movements when the anæsthesia is still complete?—One of the tests that one often sees employed—it is a rough test—by a teacher showing a witness who is learning the administration, is to touch the conjunctive of the eye with the finger. We surgeons know that a patient may be absolutely insensitive to the touch of the conjunctiva with the finger, and yet give a very considerable writhe directly the operation is about to commence or is commencing round the fundament, round the anus. That probably is the last part of the body, so far as one knows, to answer to the anæsthetic.

7947. I want to take you again to the operation. In what order does full consciousness come back (consciousness and sensation)? Is there any recognised order?—I could not answer that in a precise way, but the return of consciousness is sometimes very much delayed. Patients resist the touch of the hand long before they are conscious; they resist your moving a limb, or putting it into a certain position.

7948. Yet there is no sensation of pain?—No sensation of pain. The sense of pain will come back perhaps sooner than the sense of consciousness does; a patient will feel a prick of pain sooner than he will be able to hold an intelligent conversation.

7949. But would the power of motion come back before the sensation of pain?—Yes, long before.

7950. Is there any appreciable interval?—It would vary. May I give an illustration? How long a patient will be before he appreciates pain may be judged of by those rare and most unfortunate accidents which have occurred with regard to hot water bottles. A nurse perhaps has put a hot water bottle against the patient's side or against the patient's limb, and he lies against it perhaps for an hour or two, until, when the misfortune is discovered, there is a very considerable burn, which may end in a very considerable slough.

7951. That is after the patient has been put back to bed?—Yes.

7952. So that there is a very considerable interval before full sensation of pain and full power of feeling returns, after a strong and full dose of an anæsthetic?—There is a considerable interval elapsing before the patient will make an effort to move away from a hot water bottle, for instance, which is producing such damage that a very large slough will ensue.

7953. Although the patient is capable of making movements?—Yes.

7954. So far as your experience goes in the administration of an anæsthetic to animals, a full dose has been used?—Yes, I have seen that.

7955. For the reason which you gave, that the worst that can happen is that the animal dies prematurely?—Yes.

7956. Whereas in the case of one of your patients, it is important that he should recover?—Yes.

7957. Have you any opinion as to whether there is any difference in intensity between animal and human sensibility?—I have not.

7958. There are no data perhaps?—I have not any data except one's general experience of the indifference with which dogs and animals will go about wounded, as compared with a human being. A dog will run about, or a horse go walking about, with the whole of its entrails hanging out; and provided that it does not affect their limbs, they will go about and eat with the greatest indifference.

7959. A dog directly after a good fight shakes himself and shows very small signs of pain, as a rule?—Yes, if it is merely a flesh tear, or something of that sort, or even if a cavity is opened.

7960. We had some evidence here the other day of a medical anti-vivisection witness, who suggested that experiments on animals might be deceptive because unconsciously the experimenter hypnotised the animal. Have you anything to say with regard to that?—That hypnotism has taken the place of an anæsthetic, do you mean?

7961. No, she said, as I understood her, that the experimenter got the result from the experiment which he desired because he hypnotised the animal?—I fear that is a subject upon which I could not follow the lady.

7962. At any rate, you never attributed the success of any surgical operation to that force?—No. There have, of course, been things done on certain people by hypnotism.

7963. By Dr. Braid, of Manchester, for instance?—Yes.

7964. As regards amendments of the law, do you think it would be right, in granting certificates, that particular forms of anæsthetics should be prescribed; or do you think that they might be safely left to the operator?—I think it might be safely left as it is under the Act now.

7965. You do not think that any amendment is required there?—I do not think so at all. The Act specifically states that curare must not be used.

7966. As an anæsthetic?—An anæsthetic, alone.

7967. It is used not as an anæsthetic, but as an adjunct to the operation?—Yes. I do not think that any further restrictions on that head are required.

7968. I suppose it is not likely that local anæsthetics would be used in experiments on animals in any severe experiment?—It depends upon what you mean by local anæsthetics.

7969. I was thinking of cocaine or eucaine?—I think it is possible that there might be cases where an injection of cocaine or urethane into the theca of the spinal chords might take the place of a general anæsthetic.

7970. For the purpose of operations below that place?—I am not sure that it need be limited to below the theca.

7971. Have you yourself in your own practice used local anæsthetics?—No.

7972. You always use a general anæsthetic?—Yes; but there have been many cases of surgical operations of moment reported as having been quite successful under an injection of eucaine and cocaine.

7973. Successful from the point of anæsthetisation?—Yes; so that that is a thing that might easily arrive.

7974. I do not know whether you have seen a pamphlet by Surgeon-General Sir James Thornton?—What is it entitled?

7975. It is "The Principal Claims on Behalf of Vivisection: A Refutation"?—No, I have not seen it.

7976. He has taken in a very convenient form—in alphabetical order—various heads, and has put in a most convenient and concise way the objections to vivisection. Some of those heads are purely surgical. I do not know whether it would be fair to read a short extract and ask you your opinion of it?—I shall be happy to hear it.

7977. On page 1, may I take the opening sentence? "Abdominal surgery had been seriously retarded by experiments on living animals. The following statement was made by Sir Frederick Treves in his 'Address on Some Rudiments of Intestinal Surgery,' delivered before the Midland Medical Society at Birmingham, and reported in the British Medical Journal, November 5th, 1898:—'Many years ago I carried out on the Continent sundry operations upon the intestines of dogs, but such are the differences between the human and canine bowel that when I came to operate upon man I found I was much hampered by

my new experience, that I had everything to unlearn, and that my experiments had done little but unfit me to deal with the human intestines." Have you any comment to make on that?—Of course, I am familiar with that sort of quotation, but I do not hesitate to say that my opinion is absolutely opposed to it as a general statement. I hold that abdominal surgery has been enormously advanced by experiments on animals, and I would go so far as to say that there are many operations, such, for instance, as that which I referred to in regard to the extirpation of the kidney, which I do not think any prudent and cautious surgeon would have undertaken, if it had not been for the knowledge which has been gained by experiments on animals.

7978. The next alphabetical head, which deals with surgery, is on page 4 on "Acupressure." I do not know how far you would agree with what he says there. "Lawson Tait, F.R.C.S., who conducted the chief experiments on this subject, writes: 'The conclusions of the experiments were quoted far and wide, were translated into foreign languages, and everything looked as if "acupressure" was to reform the art of surgery. But it did not; it speedily died out, and, I think, has been almost forgotten. The explanation of this lay in the simple fact that the closure of a dog's artery is altogether a different process from that seen in the human vessel, and my experiments were not only needless, but they were absolutely misleading.'" Have you any comment to make upon that paragraph?—With regard to acupressure, I should say that I remember the time when it was introduced and when it was very considerably tried, and I should say that in many instances it was a better method of controlling hæmorrhage with safety than the ligature was. I remember when it was used for amputations of thighs, and so on; and it used to produce an exceedingly good effect. But we have now safer, more certain, and more convenient methods of controlling primary hæmorrhage after amputations, etc., than by means of these acupressure needles. Acupressure is never employed now for that reason.

7979. It has been superseded by a better method?—Yes.

7980. It was an improvement on the old method?—Yes.

7981. But it has now been superseded by newer and better methods?—Yes.

7982. Then he controverts exactly what you said in your *précis* about antiseptic surgery. I do not think I need trouble you with that, because you have given your evidence, and it is in opposition to Sir James Thornton's view. Would you kindly look at page 7—"Blood-pressure Experiments"—and would you say whether you agree with this passage quoted from Dr. Macaulay: "For practical application in medicine the numerous experiments made since the time of Hales are quite useless. The force of the heart, for example, varies in the animals inspected, and under different conditions; and the variations are infinite in different persons, in various conditions of age, strength, and state of health. The general estimates may be interesting as facts for philosophical statement, but are useless with any view of applying such experiments to use in maladies either of the sanguineous or nervous system. More useful information can be obtained by observing the force of the heart as indicated on the delicate dial of a balance-chair than from all the experiments of vivisectioners." Will you make any comment on that that you think fit?—I have not had time to read these extracts and think them over in all points, but as I read this through here, I should suppose that the writer of it had very little experience of the experiments and results of experiments which have been made with regard to many drugs.

7983. Do you know the writer by name?—No.

7984. The writer is apparently Dr. Macaulay, in his Prize Essay in 1881. Do you know anything about Dr. Macaulay?—No, I cannot say that I do.

7985. He is not a known authority?—I do not know him.

7986. Sir James Thornton adopts it, I presume?—I was thinking that anyone who wrote it, or adopted it, had probably not had very much experience of experiments and the results of experiments on animals, as I mentioned.

7987. He next deals with brain experiments. Sir Victor Horsley will tell us about them?—Yes.

7988. Now, may I ask you to look at page 17, because that touches your evidence. "Kidney Removal. It has been asserted that the operation for removal of a diseased kidney was devised through experiments on dogs, but this claim cannot be sustained, as the operation in question is very ancient, and was done in the time of Hippocrates. It has long been known to pathologists that a person may live with one kidney practically useless; no experiments, therefore, were required to prove it, especially as there have been rare instances of congenital absence of one kidney." You have already dealt with this?—Yes, I have referred to it in my *précis*.

7989. Would you like to make any comment on that passage?—It is quite true that experiments have been made many years ago. I referred to them in some little detail in some lectures* I gave some years ago. Simon probably was aware of them—it is possible that he was not; but I think that even if he had been aware of them, they were not of such a date and of such a character that he would have felt justified in basing upon them the operation which he did upon the first patient of his.

7990. And since his operation, it has become an operation carried out from time to time?—Yes, nearly every week, I should say.

7991. It is an operation you have done yourself?—Very many times.

7992. Then there is rather a long attack on Pasteur's anti-rabic inoculations. Sir James Thornton suggests an alternative treatment, such as the Buisson baths. Have you any opinion to express upon that?—I have a great opinion of the anti-rabic treatment, but I know nothing about the other.

7993. If a patient of yours was bitten by a dog that you believed to be mad, would you take the responsibility of advising him to undergo Pasteur's treatment?—Yes, certainly.

7994. Without hesitation?—Without hesitation. But that is assuming that it was actually a rabid dog.

7995. I say a dog reasonably believed to be mad?—Yes.

7996. (Dr. Gaskell.) I wanted just to ask you whether in conditions of light anaesthesia, such as a dentist uses, and so on, you may not often get movements, and even cries, and the patients afterwards say that they felt no pain?—Yes.

7997. Not only movements, but actual cries at the moment when the operation is done?—That is so.

7998. I have seen a case with Sir George Murray Humphrey at Addenbrookes Hospital, in Cambridge, in which he removed a large fatty tumour. While the operation was going on, the patient came round, and conversed with me, and said he was very comfortable and felt no pain, although he knew exactly what was going on; and after the operation he jumped off the table and walked off to the ward by himself. Have you ever seen anything of that sort during anaesthesia when the patient apparently was entirely conscious and yet felt no pain?—No, I do not recall a parallel case.

7999. But have you not ever heard that the patient has said afterwards, "I knew what you were doing"?—Yes, I have heard that very often.

8000. And that means a certain amount of consciousness?—I suppose the patient would even have been conscious. I might perhaps give you an instance. At the end of an operation, perhaps it occurs to me before putting a dressing on the wound that I had better put another suture, another stitch, into the wound. The patient is now coming round, and gives a little cry; and afterwards, when he has been asked whether he cried because he felt the pain, he has forgotten all about it, if he had felt it.

8001. So that really one may take it that pain is one of the first things to go?—Yes.

8002. Even although there is still a certain amount of consciousness left in many cases?—Yes.

8003. Another point that I want to ask you about is with respect to manual skill. The Act says that no one shall be licensed under it for the purpose of acquiring manual skill?—Yes.

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* "The Origin and Progress of Renal Surgery"—Hunterian Lectures, 1898.—H.M.

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8004. Then I understood you to say that in the case of surgeons it might be advisable sometimes for them to perform an operation on an animal before performing on a human being?—Yes; but I would not say that from the point of view of his acquiring general manual skill, but from the point of view of learning the technique of some particular operation.

8005. But that at present is illegal, is it not; the Act would forbid you to obtain that necessary skill?—Yes.

8006. Would you not, therefore, as representing the Royal College of Surgeons, like to see the Act altered in that respect?—I am not quite sure whether the Act does forbid it.

8007. To obtain the technique, I said?—Yes, not manual skill; that is forbidden. To give an illustration: Supposing you are going to cut a cancer out of the bowel, and are going to remove it altogether; you have to cut the bowel above and below the site of the cancer widely, so as to take it all away; then you have to join those two ends of the bowel up. How are you going to do it? Will you stitch up one end entirely and graft the other end into the bowel above, or will you join them end to end? That is the kind of thing I meant; and you can find it out by experiments on animals; and you might find that out, I think, under the Act, under licence and Certificate B. That is a thing of which it is very desirable that we should be able to learn the technique in that way, or to test it in that way. But that is not what I thought you meant. I think the Act means that experimentation is not to be employed for learning manual skill on the part of a young surgeon.

8008. It does not say anything of the kind; it says manual skill?—I always read it in that way.

8009. One final question with respect to demonstrations. Have you read "The Shambles of Science"?—Oh, yes.

8010. Do you consider that for advanced students who are likely to be teachers of physiology, and are likely to hold teaching positions in some way in connection with medicine, the kind of demonstrations spoken of there ought to be forbidden?—I cannot call to mind all those demonstrations.

8011. They were demonstrations showing comparatively new discoveries in physiology; not demonstrations that you would give to young students first coming in to study physiology, but deliberately given as showing the latest results of experimental work in physiology?—I think that those who are going to be practical teachers of physiology ought to have an opportunity of witnessing many experiments, and perhaps doing experiments, which are not necessary to be shown to ordinary students—if that is your point.

8012. Yes. It has been said that those experiments ought not to be allowed, and hardly are allowed under the Act?—I cannot answer that question, because I do not know what those experiments are.

8013. I mean those experiments which are described in the "Shambles of Science"?—I have not got them sufficiently in mind.

8014. But they are experiments for advanced students showing the newest things in physiology?—I think, as I have said already, that many experiments ought to be permitted to those who are going to teach and practice physiology which ought not to be permitted, or ought not to be shown, to ordinary general students.

8015. Which would be useless for ordinary students?—Yes.

8016. (Mr. Tomkinson.) You spoke of the valuable results of surgical demonstrations in regard to observing the action of the heart and entrails?—Those we call physiological experiments; they are not surgical.

They do not come into any part of either surgical teaching or practice.

8017. I wish to ask you, would the action of those organs be affected by the fact of the animal being under anæsthetics?—Not sufficiently to prevent the observer getting a true and accurate idea of what goes on.

8018. With regard to the completeness of anæsthesia and the use of curare, you deprecate the use of curare alone?—Yes, I do; we do not know absolutely for certain everything about it.

8019. Do you think that, administered with chloroform or ether and other real anæsthetics, there may not be a danger of the animal being powerless through the effect of the curare, and yet the anæsthesia would be imperfect at the time?—I do not think so at all. I am aware to what you refer, of course—that the animal being under the influence of curare cannot move even though it may suffer; but I think, from the way in which I have always seen and have always heard that chloroform or ether is administered, there is not the least fear of that.

8020. You are absolutely satisfied in your own mind that complete anæsthesia is always aimed at most carefully and in your opinion secured?—Yes, I do think so; and certainly I have always seen it.

8021. Little has been said about the paragraph in your *précis* on the question of shock. May I ask whether you have seen that pamphlet giving a description of 16 experiments by Dr. George Crile upon animals?—Yes, I have read it a long time ago.

8022. The pamphlet excited a good deal of attention and, I may say, horror among a great many people, from the tremendous severity of the operations to which the dogs were subjected?—Yes.

8023. In your opinion were the results, from any point of view, at all commensurate with the severity of such operations on the observation of shock or the effect of shock?—I hardly feel that I can form a just opinion about that. I did not like many of the experiments that were performed, but I would not like to express a definite opinion.

8024. You hardly feel prepared to justify them altogether?—No; and I would hardly be prepared to say that they were not desirable to have been made once, but I certainly think that such things ought not to be repeated for the sake of repeating them.

8025. Provided that complete anæsthesia can be secured, I suppose you would be opposed and object to any really painful experiment without it?—I would not go quite so far as that; because, if I may again refer to the experiments which were made by Sir Charles Bell many years ago, it is possible that some investigations involving more or less pain might be very desirable; but as a rule I should certainly say so.

8026. Could you express any opinion as to whether from the moral point of view it would be justifiable to inflict really painful experiments for possible results in search of alleviating human suffering or the prevention of it?—That is a matter upon which of course each individual will have his own opinion. As a great lover of dogs, and as a person very desirous of saving animal suffering, I should still say that I thought that from the moral aspect it was quite justifiable.

8027. (Dr. Wilson.) There is only one question that I should like to put to you. Are you of opinion that a very severe and prolonged operation can be performed on an animal under narcosis alone, under morphia for example, or paraldehyde or eucaïne, an operation involving the opening of the abdomen, the thorax, and so on, to continue, say, for an hour or an hour and a half or longer?—Under the conditions suggested by Sir William Church I certainly think that an operation could be performed under morphia alone without the animal feeling any pain.

8028. You think that the animal could live even through that period if it had a poisonous dose?—I think so.

NINETEENTH DAY.

Wednesday, 15th May 1907.

PRESENT :

Sir W. S. CHURCH, Bart., K.C.B., M.D. (*in the Chair*).

Colonel the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.*

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.

Sir J. McFADYEAN, M.B.

Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.

Mr. W. H. GASKELL, M.D., F.R.S.

Mr. J. TOMKINSON, M.P.

Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Major LEONARD ROGERS, M.D., F.R.C.P., I.M.S., called in; and Examined.

8029. (*Sir William Church.*) You are a Fellow of the Royal College of Physicians and Professor of Pathology at the Medical College, Calcutta?—Yes.

8030. And you are a Major in the Indian Medical Service?—Yes.

8031. You consider that our present knowledge of the physiological action, and of the antidotes of snake poisons, is founded entirely on the results of experiments on animals?—Yes.

8032. Will you tell us on what grounds you base that opinion?—The most important early experiments are those of Sir Joseph Fayrer and Sir Lauder Brunton on cobra venom, which showed that the cobra venom kills by producing paralysis of the respiratory centres, and has subsidiary actions, but that that is the principal action and the primary cause of death. As the result of those experiments, they carried on artificial respiration, and they found that they could keep animals alive for a large number of hours, even up to twenty-four hours after paralysis of respiration had taken place. And subsequently, a further Commission worked in India, and suggested the practical value of this method, and they succeeded in recovering a few animals after many hours of artificial respiration. And there have been a few cases on record, one in Ceylon in particular, where this method appears to have been successful in saving the lives of human beings who had been bitten by cobras; but, unfortunately, it only works if the dose of cobra venom is comparatively small, only just a lethal dose, but if the cobra injects several times a fatal dose it is not possible to recover the patient by that method.

8033. Have further experiments shown that it is probable that intravascular clotting also is part of the cause of death, besides the paralysis of respiration?—That is, in the case of Russell's viper, and viperine poisons. The viperine class of poisons have a totally different action from that of the cobra class, that is the poisonous colubrids. It was first shown by Professor C. J. Martin, now of the Lister Institute, that one of the snakes in Australia caused intravascular clotting, and he suggested that this was probably the physiological action of the viperine class; and it was afterwards shown by Lamb in Bombay, that Russell's viper does cause death in that way. Four years ago I investigated this subject at the physiological laboratory at the London University with Dr. Waller, and I tested vipers, both Russell's viper of India, the rattlesnake of America, and the African puff adder, and I found that, in addition to this intravascular clotting you also got paralysis of the vaso-motor centre, and that that is the

most essential cause of death in the case of viperine snake poisons. I also found that it could be counteracted to a considerable extent by the action of drugs, especially adrenalin, which contract the vessels and so counteract the fall of blood pressure. I published the experiments in a paper read before the Royal Society, and illustrated my point with tracings. So that our knowledge of the viperine poison's physiological action is also due entirely to experiments on animals.

8034. You wish also to refer to some experiments of Lille and Sir Thomas Fraser?—Yes. The next point in the action of snake poisons is the question of the action of antivenins, first discovered by Calmette of Lille, and independently by Sir Thomas Fraser. They found, as the result of repeated inoculation of cobra venom into animals such as horses over a period of many months, taking usually eighteen months, that they were able to produce a powerful serum or antidote in the blood of the infected horses, which has a definite specific action against the cobra venom; so that after injection of the cobra venom into an animal it is possible to save that animal's life by the injection into a different part of the body of a proper dose of the antivenin. In some experiments which I have done on this question, too, I found that the results are very much better if the antidote is injected intravenously. If the anti-cobra serum is injected intravenously it has exactly the same action upon the system of the animal that it has if mixed with cobra venom before injection; but if it is injected subcutaneously, it is absorbed more slowly, and does not act so efficiently. There is one difficulty with regard to the practical application of this treatment, which is, that the serum made from cobra venom does not act at all against the venoms of the viperine class, which have a different physiological action; and, further, the antivenin made from cobra venom only has a slight action against other colubrine poisons, such as the krait or the hamadryad.

8035. Therefore, although the antivenin has a decided protective influence, it is not practically of very great use?—No.

8036. Because it is not at hand?—It is not on hand, for one condition; you seldom have both the antivenin and the syringe on the spot; and even if you have it really requires much larger doses than are usually given.

8037. (*Sir Mackenzie Chalmers.*) And a very skilled operator?—Yes.

8038. No village man could do it?—No, I do not think so. There have been a number of cases reported where it has apparently had a good effect.

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8039. Where a doctor happens to have been near?—Yes. The best example was the case of Captain Lamb. He was investigating snake poison in Bombay and he was bitten by a cobra in the thumb, one fang entering. They were testing antivenin at the time, and the antivenin which they had they found was not strong enough to be of much use, so that they had to send for some more. It was an hour before they got an efficient serum, and by that time Captain Lamb was beginning to show symptoms of drowsiness and early symptoms of cobra poisoning. After an injection of the antivenin he recovered perfectly well with some local sloughing, which you always get after such poisoning. That is a definite case where antivenin appears to have saved life.

8040. (Sir William Church.) How long ago did you take up the subject?—I was working first at the sea snake poisons in Calcutta, in 1901, when I found that they were very poisonous, and that their venom had the same action as the cobra venom. Then when I was in London in 1903 I was working at the physiological action of the sea snakes and the viperine poisons which I have mentioned; and then Sir Lauder Brunton suggested to me the carrying out of experiments to test the value of the method he had previously suggested of a local application of permanganate of potash by incising the seat of the bite, or the injection of venom and rubbing in the crystals.

8041. Sir Lauder Brunton and Sir Joseph Fayrer, I think, found that mixing a solution of permanganate of potash with cobra venom destroyed its activity?—Yes.

8042. And then did he request you to continue the experiments with permanganate of potash?—He requested me to continue the experiments. He had previously published the suggestion of making the snake lancet, and it had actually been made; but as it had not been thoroughly tested and proved, it had not come into practice; and he asked me to take the matter up at that point. The first thing I did was to test the action of permanganate of potash on a number of different venoms, as it was previously only known that it destroyed cobra venom. I tested it against all the Indian colubrine poisons, the krait venom, the hamadryad and the banded krait, and the sea snakes, and also against Russell's viper of India, the rattlesnake of America, and the African puff adder; and I found that mixing a strong solution of permanganate of potash with any of these venoms would destroy the venoms in almost the same weight as the permanganate of potash; so that it has a very powerful action. It appears to act by oxydising these substances, which are all albumoses, which are an unstable form of albumen, into a harmless substance.

8043. Do you know at all why Sir Lauder Brunton and Sir Joseph Fayrer did not carry on their experiments with permanganate of potash?—I do not know the details of it. Sir Lauder Brunton told me that it was owing to the passing of the Anti-vivisection Act that his experiments were stopped—he was unable to go on. At that time he was trying injections of a solution of permanganate of potash as a form of treatment; but that apparently is not an efficient method of using it. What he told me was that his experiments were stopped by the passing of the Act. Then next I started carrying out experiments on animals as nearly as possible under conditions in which the permanganate of potash would be used in actual practice. I used rabbits and cats.

8044. You say that you carried out the experiments on animals as nearly as possible under the conditions in which the permanganate of potash would be used; but these animals, I gather, were under an anæsthetic?—They were chloroformed. There was a difference in that point. The animals were all chloroformed, then a measured dose of cobra venom was given in accordance with the weight of the animal, so many lethal doses, or, rather, so many times a fatal dose. The injections were made a little above the foot. Ordinarily snake bites are on the hand or foot, as a rule. After the measured time had elapsed, varying with the different experiments, a ligature was applied above the seat of the injection, in order to stop the circulation. An incision was then made at the seat of the injection with a small lancet—the actual lancet which is now being sold in India, and at the seat of the injection within 30 seconds I found that there was always a small blood-stained effusion, which is of practical importance, at any rate, as it shows the exact seat of the venom; a

few permanganate crystals were then poured into the wound and rubbed in until the wound was completely blackened. This took one or two minutes. The ligature was then immediately removed on the supposition that the poison had been destroyed by the application of the permanganate of potash. The cats were then allowed to come round—I had reason, of course, for doing that—and kept from five days to a week. They appeared to suffer no pain; in fact, the only difficulty I had with them was that they all wanted to be petted at the same time if I went to see them. As the result of the experiments, I found that after the injection of ten times a fatal dose of cobra venom I could save the animal by operating 30 seconds afterwards. That is about the time it takes to apply the ligature if a person is bitten in a limb. I lost one animal after the injection of five times a fatal dose (it was the first animal done), the application not having been efficiently carried out as in the latter case; but its life was very greatly prolonged as compared with the control animal injected with the same dose without the treatment. Then I used decreasing doses of venom at longer intervals, and I have now worked up to getting a recovery in the case of a cat not treated until half an hour after the injection of a double lethal dose.

8045. (Sir Mackenzie Chalmers.) By treating, do you mean the ligature?—The ligature and the permanganate treatment.

8046. They are contemporaneous practically?—Yes.

8047. When you talked of 30 seconds, what did that mean?—That means the application of the ligature.

8048. The other would be within half a minute?—Yes, as soon as possible. The time limit is the time when the ligature is applied which gives you time during which there has been a chance for the absorption of the venom.

8049. In the case of a human being, could you apply a ligature?—Yes, as a matter of fact, the natives almost invariably do it. Similarly I tested with Russell's viper as a type of the viperine class, and there I have got recoveries up to five times a fatal dose, which is more than any viper I know of can inject, judging from the amounts I have obtained from these snakes, in proportion to the weight of a man. That is the experimental basis of the method.

8050. (Sir William Church.) And this method has been successfully applied to human being?—Yes. Now with the printed instructions giving directions for the use of the lancet, supplied with each lancet, I ask to have reports sent to me; and I have received a number of reports during the last three years, which I found it necessary to classify in two different classes. In judging of the action of any remedy in the case of snake bite there are a great many fallacies to be avoided. In the first place, the patient may not have been bitten by a poisonous snake; and in the second place, he may not have received a fatal dose; consequently it is owing to this that so many remedies have been brought forward as cures for snake bite which have been found experimentally not to be efficient. In order to try and get absolutely accurate figures I have taken in my first class only the cases in which the snake has been killed and identified as a poisonous one, and in which there were definite fang marks where undoubtedly the patient had been bitten by a poisonous snake.

8051. May I ask in what way the snake has been identified as a poisonous one?—The snake has been identified as a poisonous one by the hospital assistant, in some cases by qualified medical men. Many years ago as the result of Fayrer's work the Government of India provided coloured plates of different poisonous snakes to all the dispensaries in India; so that these men are trained to recognise these snakes, and I think with the great majority of these cases certainly the identification has been undoubtedly accurate. I cannot vouch for it personally, but that is as near as I can get.

8052. (Sir Mackenzie Chalmers.) You mean that the snake is killed and brought to the dispensary?—The snake is killed and brought to the dispensary and identified by the assistant surgeon, who has been trained and has these coloured plates at his disposal.

8053. (Sir William Church.) The identification of a poisonous snake is not a difficult matter, is it?—No, not to these men who have the coloured plates, and have been trained to it.

8054. No non-poisonous snake has got the character

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istic fangs, has it?—No, the fangs are quite definite, and then, again, in nearly all the cases I have got a definite note of there were two definite fang marks, punctures which would not be produced by anything but a poisonous snake. Among these 14 cases where the snake was killed and identified, nine were cobras, three Russell's vipers, and two kraits, these being the commonest very poisonous snakes in India. The large number of cobra cases is important, because in measuring the amount of cobra venom that one can obtain from a freshly-caught cobra, the amount averages ten times a fatal dose for a man; so that it is exceedingly unlikely that many of these cobras can have injected less than one fatal dose, and it is very unlikely that more than at any rate one or two of these patients had not received a fatal dose of venom. Among these 14 cases there was one death, and this was a woman, who was brought to the hospital eleven hours after the infliction of the bite, when she was quite moribund. She had been bitten at 3 o'clock in the morning, and was brought to the hospital in the middle of the next day; and in that case, of course, no treatment could be expected to have any effect. On the other hand, there was one case of a cobra bite in which the patient was not treated until four hours afterwards, when definite symptoms had set in, and from my experience of experimental work I know that any animal which shows symptoms within four hours of injection of cobra venom will always die; so that I have no doubt that this man's life was saved by the treatment. It is also well known that an animal may recover after well-marked symptoms have set in; if you give just a fatal dose, or rather a dose which is just not fatal, the animal may actually remain paralysed for 24 or even 48 hours, and still recover. So that it does not follow that after symptoms have set in you may not save your patient; it entirely depends upon whether a fatal dose has already entered the circulation; if not, by destroying all venom locally which is not absorbed, you prevent a fatal dose being absorbed, and so may save your patient after symptoms have set in.

8055. (Mr. Ram.) In that case, of course, the symptoms showed, I understand, that beyond all doubt the cobra was the cause of the bite?—I think so, from the description.

8056. There is nothing to show, I suppose, whether the dose was a fatal dose or not?—Nothing more than the fact that the symptoms had set in within four hours; and I know that in animals if symptoms set in within four hours they never recover. If you give a non-fatal dose you often get no symptoms in less than 12 hours.

8057. (Sir Mackenzie Chalmers.) What is the ordinary period within which symptoms set in in the case of the cobra?—It depends entirely on the dose. If the cobra injects its full quantity of ten times a fatal dose, you get symptoms within half an hour, not, I think, much earlier; so that does give you ample time.

8058. You have half an hour's grace?—Yes.

8059. (Mr. Ram.) In the worst cases?—Yes.

8060. (Dr. Wilson.) Had the natives applied the ligature in these cases before they were brought to you?—Yes, in a number of them they had. In a good many of these cases the ligature had been applied.

8061. Do you know in what proportion?—I have notes of some; I have not been able to get full details of all the cases. I have corresponded with regard to them so far as I could. I have four cases here where the ligature was applied at once.

8062. Before they came to you?—Yes, before they were brought for treatment, and that, of course, makes it much more favourable.

8063. (Sir William Church.) In the 34 cases in which the snake was not killed and identified, was there evidence of any of those being bitten by a poisonous snake?—In a number of those cases it was reported that the snake was actually named as a cobra, but the snake was not killed; so that I thought it better to exclude them.

8064. (Sir Mackenzie Chalmers.) The natives know the cobra very well?—Yes, but I wanted to be on the safe side.

8065. It is not an easy snake to mistake for any other?—No. One difficulty is that even after a cobra has bitten a native they will not kill it very often;

so that a number of cases in that 34 were certainly bitten by cobras, but as the snake was not killed I put them in the other class.

8066. (Sir William Church.) Six of those cases were fatal?—Yes. One was not treated till nine hours afterwards, and three others from four to six hours after; but one observer reported three cases treated two hours after which died. That is the only record of any case which has failed that I know of; but the same observer reported 20 successful cases.

8067. You go on to say that it is unlikely that such a large number would have been saved—that is, 24 out of 30—without the use of the permanganate locally applied; and then you go on to say that in some of these cases it was successfully applied by persons without any medical training?—That ought also to have applied, of course, to the first series. It applies to both series.

8068. It applies to all?—It applies to all. There was one case of a Russell's viper treated by a European some thousand miles away in the north of Siam.

8069. You do not think that without your experiment on animals it would have occurred to anyone to use this treatment?—Sir Lauder Brunton had actually had the lancet made, and had described it in his book on "Therapeutics."

8070. He arrived at it in seeing the effect of mixing permanganate of potash with cobra venom before he injected it into the animal?—Yes. There have been thousands of remedies brought forward supposed to have been used in cases, but that evidence is so absolutely unreliable that without the experiments, I think, it would have been impossible to establish any form of treatment; and this treatment was undoubtedly established as the result of experimental work on animals.

8071. Could you inform the Commission at all what the mortality from snake bite in India is?—About 20,000 cases are reported every year in India, and 10,000 of these, just about half, occur in Bengal alone.

8072. (Mr. Tomkinson.) Fatal cases?—Yes, those are really fatal cases. Of course, you cannot look upon these as absolutely reliable statistics, but I think they give a very fair idea.

8073. (Sir Mackenzie Chalmers.) And year by year they are very much the same?—Year by year they are very much the same.

8074. (Sir William Church.) You do not think that a large number of deaths not due to snake poison are put down in the returns to snake poison?—I do not think so. I should think it is more likely the other way, because I know from experience that over 90 per cent. of deaths are returned as fever, and I have known any disease returned as fever. The ordinary tendency for the village man is to return everything as fever; in fact, I have myself met with cases of snake bite recorded as fever.

8075. So that a sudden and unexpected or suspicious death is not put down to snake bite, which has often been stated to be the case?—That is a conceivable thing; but I think, taking the whole cases, it is more likely that they are underestimated than overestimated.

8076. (Mr. Ram.) Who makes the return?—The return is made by the Headman of the village in different parts of India.

8077. Is he bound to make a return?—Yes.

8078. Of every death?—Yes. He is not a medical man.

8079. (Sir Mackenzie Chalmers.) He is a registrar?—Yes, in different parts of India he is sometimes called the Chokidor.

8080. (Sir William Church.) Can you give the Commission any idea of the amount of loss of life caused by snake bite?—I do not know the exact figures, but I know that there are many thousands a year returned as due to snake bite.

8081. Perhaps some members of the Commission would like to ask you some questions with regard to snake bite before we go on to the second part of your *précis*?—If you please.

8082. (Sir William Collins.) Do I rightly understand that the nature of the poison in these cases of snake bite is a chemical one?—What do you mean exactly by chemical?

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8083. Is it due to an organism?—No; it is chemical in that sense.

8084. And I think you said that it was due to albumoses?—Yes.

8085. Are they substances which are destroyed by oxidation?—Yes.

8086. And is the principle of the utilisation of the permanganate of potash to act as an oxydising agent upon the chemical poison?—The permanganate of potash was first used before the nature of the poisons was known. In Sir Lauder Brunton's time the composition of snake poison was not generally known. That is my suggestion, that it acts as an oxydiser.

8087. It is the essential principle of Condy's Fluid, is it not?—Yes.

8088. Are all cases of snake bite by poisonous snakes in the human subject fatal?—No, by no means.

8089. Can you give us any idea what proportion of cases are fatal?—It is very difficult to get evidence of that.

8090. Assuming a bite, by an admittedly poisonous snake, in a human subject, what proportion of cases would be fatal?—I should say a very large proportion, certainly 75 per cent.

8091. Do you think we might say that 25 per cent. would recover?—I do not think necessarily so. I say that at least 75 per cent. die, and probably a great many more. I am very anxious not to exaggerate.

8092. And even if symptoms have supervened may recovery occur?—Yes.

8093. Did I rightly understand you to say that a ligature was the regular mode of treatment?—The natives use a ligature almost invariably—very often indeed.

8094. Do you regard the ligature as a successful mode of treatment?—Not by itself; it would delay death, but would not save a man's life.

8095. Do you think that in any case it would preclude a fatal result?—I do not think so. I tried an experiment on it once by ligaturing the wound and opening it again after a time, and letting a little poison in at a time.

8096. Did you try a series of control experiments with the ligature without injection, and a series with the ligature with injection?—Yes; it delays death, but I never got any result of preventing death.

8097. Have you published those results?—No; they were done at Rawal Pindi the first year I was out. I did not get any practical results, so that I did not publish them.

8098. You are in the Indian Medical Service?—I am.

8099. Did you make reports annually to the Government?—Those were entirely private experiments of my own; I was in military employ, and I did them in my spare time.

8100. Have you in the course of any official report made a reference to these experiments?—No; all my papers have been published either by the Royal Society or in the medical journals. Most of my work was done in my spare time, mostly when I was on leave.

8101. How many cases of bite from a poisonous snake have you seen yourself in human beings?—One.

8102. Was the remedy applied in that case?—The remedy was applied by me in a few minutes, and the patients had no bad systems—that was in Calcutta.

8103. And in the cases which you have brought before us—this series of fourteen cases with one death—was the poison from the snake tested upon animals?—No, certainly not. How could it have been? There was no opportunity of doing so. These cases have all been reported to me by different medical men in different parts of the country, and there was no reason for doing it. It is all a question of how much the snake injected. If you make it bite another animal, it may have exhausted its venom, and you will prove nothing.

8104. You think it would be superfluous?—Absolutely. If a snake has bitten an animal and has exhausted its venom, if you made it bite a dog, and it did not kill it, that would not prove that it had not killed a man in the first instance.

8105. I should have thought that the experiments might have been considered of value; but I am quite prepared to take your opinion?—If I had been on the

spot to do it, I should have been very glad; but there is no doubt how very uniform the strength of cobra venom is.

8106. Do I correctly understand that in your second series of cases there were 34 cases and six deaths, as against 14 cases and one death in your first series?—Yes, so it happens.

8107. To what do you attribute the excess of mortality in the second series as against the first?—Because in the second series there were three cases which were not treated for four hours or more after the bite; and but one case occurred which was treated at a later time than that in the first series.

8108. But the distinction between the two classes is, that in the first the snake was identified, and in the second it was not?—That is one distinction; but there is the further distinction which I have mentioned, that in two fatal cases in the second series the patient did not come under treatment till much later than those which recovered in the first series, which makes an essential difference in the mortality.

8109. Has the Government of India adopted the method of using the permanganate of potash?—When the method was first brought out they declined to adopt it on Sir Joseph Fayer's recommendation; but since these results have been published they are now widely distributing the lancet in every village in several provinces, having been convinced of the utility of the method.

8110. At the public expense?—Yes.

8111. Is it being sold as well as supplied?—Yes, over 7,000 lancets are sold by one firm in Calcutta. Every European going shooting, and that sort of thing, practically invariably takes one.

8112. How long has that distribution been going on?—It is only three years ago since those results were published, and it is only last year that the Government have taken it up extensively. The previous distribution was entirely by sale through chemists.

8113. Is it too early yet to notice whether it has had any effect upon the mortality returns?—Certainly it is much too early.

8114. Do you institute any comparison between the method of Calmette and this method of permanganate?—The two should be combined. This is practically a method which applies to every kind of snake. Calmette's method does not, because the antivenin which is made for cobra is only useful for cobra. An antivenin has been made for Russell's viper recently, but it will only work for Russell's viper; and, further, the amount of sera required is very large, larger than is often practicable; but if the permanganate treatment is being used to destroy all the venom remaining locally at the seat of injection, then the use of antivenin becomes much more likely to save life, as it would have only to act against a much smaller amount of venom; so that if you happen to get a patient who has just absorbed a fatal dose and so that the permanganate treatment would be too late to save life, by destroying all the venom remaining locally by the permanganate method you could then save that patient's life by a comparatively small dose of the antivenin. So that the permanganate mode really makes the antivenin one much more practicable.

8115. Then by the combined method, the local use of permanganate crystals and subsequent injection of Calmette's serum, would you be able to say within a certain number of hours subsequent to the onset of symptoms, that you might deal with perfect success with a case of snake bite?—I think you could if you identified your snake, so that you knew what serum to use, and if you had got that particular serum available, which unfortunately is seldom the case.

8116. There is no polyvalent serum?—Not yet; they are making one for both cobra and Russell's viper at the Pasteur Institute at Kasauli.

8117. I suppose that the production of these sera is the result of experiments on animals?—Entirely.

8118. Seeing that snake poison is not due to an organism, by what mode is the antitoxin or vaccine or serum prepared?—It is prepared by injecting the measured doses of the venom which is obtained from the snakes and then dried, and in the dried condition it can be kept for 30 years or more without losing its strength; then measured quantities of this are injected, beginning with much less than a fatal dose and gradually increasing it until many times a fatal dose can

be injected into the animal; and this causes the production of the antidote in the horse's blood, just as after the injection of the diphtheria toxin in the case of the antidiphtheritic serum.

8119. Then is the antitoxin obtained from a horse by injection of an attenuated dose of the original poison?—Beginning with an attenuated dose, going on to very large doses.

8120. (*Sir Mackenzie Chalmers.*) The animal becomes tolerant as you go on?—Yes, and you may give a hundred times a fatal dose.

8121. (*Sir William Collins.*) It has been stated in the course of evidence before the Commission that the injection of a solution of permanganate of potash was not efficient?—It has not been found so.

8122. Whose experiments were those?—Sir James Fayer and Sir Lauder Brunton's experiments were the original ones, and some have been done by other people as well. There was a paper published by Brieger a short time ago, but he used a very weak solution.

8123. Do you mean that the animal died in spite of the use of the injection?—Yes. Another great disadvantage of the injection is that you must have a syringe, which is not available under ordinary conditions; whereas the snake lancet is an inch long and can be carried in the pocket.

8124. But apart from practicability, I understand you to state that the use of the solution by injection has been a failure, whereas the use of the crystal is found sufficient?—We have not got many successful cases from the use of solutions, but I think that is largely due to their not having been used strong enough. I think that possibly a stronger solution might be effective, but it is much less practicable.

8125. An injection of Condy's Fluid?—Yes; it is certainly much more difficult to destroy the venom in that way. By rubbing in the crystals you get much more chemical action; by injecting subcutaneously it is liable to be absorbed.

8126. I take it that it must be a solution of the crystal in the blood or the lymph?—Yes, but it is a much more strong solution.

8127. But you do not attribute virtue to the dry crystals?—No, it has to be dissolved in the serum. I think that a very strong solution would be effective; but it is nothing like as practicable. It must act in solution, of course. I think it is purely a matter of dose.

8128. In a little paper which Sir Lauder Brunton handed in, which goes with Ferris and Company's First Aid Snake-bite Outfit, it is stated "The best remedy that can be carried for this purpose is permanganate of potash, the efficacy of which as an antidote to the poison, not only of the cobra, but of other venomous snakes, when properly applied, has been conclusively demonstrated." Do you think that statement is justified?—What is the date of that?

8129. It was handed in the other day?—Yes. The only two large classes, namely the cobra class and the viperine class, have two absolutely distinct actions. There are a few which do have a combined action to a certain extent, but the permanganate has been tested now against such a large number and found uniformly active, that I think we may take it as practically certain that it will act against any kind of snake poison. It has not been tested, of course, against every variety in the world yet.

8130. If that be so, in view of the heavy mortality in India, a heavy responsibility would rest upon the public authorities if they did not supply this apparatus?—You have to take into account the conditions in India. If you take, say, Eastern Bengal, where so many snake bites occur and where almost every house is separate, with a large number of rice fields between, there is practically no such thing as a village. When you come to hundreds of thousands of square miles of country like that, you can see how difficult it is to have the remedy always on the spot. But the Government are doing now a great deal towards distributing it, and I expect that we shall get good results.

8131. You say that the fact has been known for 30 years that this permanganate of potash is an antidote?—If mixed with cobra venom outside the body, it was known 30 years ago that it would destroy the venom.

8132. And presuming that the application was made in time, many thousands of lives might have been saved if what you tell us is correct?—It was purely a matter of getting a practical method of using it, which we now have but which we had not at that time.

8133. (*Mr. Ram.*) First with regard to viperine poison, 15 May 1907 you say that, if treated, it is counteracted by the use of adrenalin?—It counteracts the fall of pressure to some extent.

8134. In that particular case of treating viperine poison, experiments on animals would be shown to be doubly useful, would they not?—Yes, I think so.

8135. In the first place, you ascertain, I understand, by experiments on animals, exactly the mischief caused by viperine poison, namely, intravascular clotting or paralysis of the vaso-motor centre?—Yes.

8136. In the second place, having discovered the exact injury inflicted by the poison, you treat it with adrenalin?—Yes.

8137. Adrenalin also was discovered, was it not, in consequence of experiments on animals?—Undoubtedly.

8138. With regard to the snake bites, had they been investigated clinically for a great many years?—Yes.

8139. As I understand, as long ago as 30 years, by means of clinical research, the use of permanganate of potash was discovered as being advantageous?—Not by clinical work.

8140. Was it ascertained even as long ago as 30 years that the use of permanganate of potash was advantageous by experiments on animals?—It was discovered by experiments in order to try and find some antidote. They were mixing different chemicals with snake poison in order to see if they could find some antidote, and it was found in that way.

8141. Do you know what it was at the passing of the Act which hindered Sir Lauder Brunton from carrying on further experiments?—I do not know the details of it.

8142. You do not know what bit of the Act, so to speak, pinched him?—No, I have only his statement that that was the fact.

8143. I understand that horses, for instance, have been made immune by the injection of snake poison?—Yes.

8144. That was ascertained by experiments on animals, I suppose?—Yes.

8145. Solely and entirely?—Entirely.

8146. With regard to the use of permanganate of potash, you told Sir William Collins that it does not have to operate against mischief produced by an organism?—No, it has an antiseptic effect as well, which is useful; because there is a great tendency for septic changes to follow on snake bites, especially viperine ones. But that is not its essential action.

8147. But the mischief which it is intended to obviate and does obviate is caused not by an organism, but chemically?—Yes.

8148. Objection has been taken, as you no doubt are aware, by a good many witnesses to what they term the injection of any matter from any animal into a human creature?—Yes.

8149. This treatment of permanganate of potash on snake bite would not be open to that objection?—No.

8150. With regard to immunising horses by serum, is this the only case in which an antitoxin can be shown to be valuable against an evil which is not caused by an organism?—There are other chemicals, such as ricin and abrin, of which it has been said, that by injecting these into animals you can produce a serum which will even prevent the action of these chemicals, although they are of vegetable origin; but not against such substances as morphia.

8151. (*Dr. Gaskell.*) Those substances are also albumoses?—I think they are very closely allied.

8152. They are vegetable albumoses?—Yes.

8153. (*Mr. Ram.*) Then, except with regard to vegetable albumoses, the evil which the antitoxin can prevent in all other cases is produced by organisms. Is that so, so far as you know?—I believe so, so far as I know. I do not know the details of all those things.

8154. (*Sir Mackenzie Chalmers.*) Have you made any experiments with duboyi poison?—That is Russell's viper.

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8155. What is that big snake that is so exceedingly poisonous?—The hamadryad.

8156. But there is another name for it?—The king cobra.

8157. Have you ever experimented on that?—Yes. I tested all these to see whether they had the same action as cobra venom, and whether Calmette's antivenin made from cobra venom would act against the other colubrine snakes, the hamadryad, the krait, and the banded krait. I found that they do have some action, but not enough to be of very practical use.

8158. The reason being that the physiological action of the two poisons is different?—No, the physiological action is the same. I tested them all first, and they all had the same physiological action in paralyzing the respiratory centres—that is to say, all the colubrine class and the sea snakes. The cobra antivenin does have some effect against all those, but it is very much less than the effect against the cobra itself.

8159. And it has no effect when you get it against viperine poison?—It has no practical effect really against any but the cobra.

8160. But the permanganate of potash if applied instantaneously operates, so far as you know, on all snake poisons that you have tried?—Yes.

8161. Did not Dr. Douglas Cunningham try some experiments for the Government of India?—Yes.

8162. They were negative, were they not, in their results?—He tested a large number of reputed antidotes, but the results were all negative.

8163. And also he tried Calmette's and Fraser's antivenin?—Yes; he showed that it had an effect, but the effect was not very strong; it required to be much stronger.

8164. Was that on account of the small doses?—Yes; as a matter of fact, if you take a full quantity of cobra venom and you inject an average quantity, you will want 400 c.c. of Calmette's serum of the usual strength to neutralise it; that is why it is not very practicable; but, of course, a snake may often inject a small quantity, and then it may be efficient, but it cannot be relied upon by itself in every case.

8165. As regards the ligature, I suppose that the object of the ligature is to prevent the poison spreading into the system?—Yes.

8166. But if you could not apply the permanganate of potash for some time, do I rightly understand that in spite of any ligature the poison does get into the system?—I think that the poison would diffuse to a considerable extent in the limb, and make it much more difficult to neutralise it locally. When some time has elapsed I advocate making a much longer excision and applying the permanganate of potash more thoroughly.

8167. Can you catch up the poison then?—Not altogether.

8168. To some extent the permanganate gets into the circulation and catches it up?—The permanganate does not get into the circulation. I have tried that, and I find that you cannot do it, for this reason: If you use the potassium salt permanganate, it is too poisonous to inject in sufficiently large doses. I tried to use the sodium permanganate, but I found that upon sterilising it by boiling first a lot of it gets precipitated and it is very difficult to use—it is more unstable. I have not been successful with it.

8169. If you cannot apply the permanganate of potash at once and to some extent the symptoms have set in, I suppose you apply other remedies as well in addition to it?—Yes, certainly; you apply any remedy that you think may be of value in addition.

8170. How long after a bite is it of any use whatever to apply permanganate?—The longest case I have got was four hours.

8171. Does some part of the poison remain locally as long as that?—Yes, you can show it in this way: If you take a dose which is just fatal to an animal, if you inject it intravenously it will kill the animal in 20 minutes, but if you inject the same dose subcutaneously it takes 24 and often 48 hours to kill the animal; showing that it is absorbed comparatively slowly. I think the reason is the point which I have mentioned, that when you make an incision at the seat of injection you always find a bloodstained effusion of serum in 30 seconds. I think that is a protective agency to lessen the rapidity of absorption, as the absorption of a small

dose is slow. So that if a very large dose is injected, a fatal dose may reach the circulation very rapidly before there has been much effusion to limit the absorption.

8172. Practically, then, the mortality of snake bite depends upon the amount of venom which happens to be injected?—Yes.

8173. And therefore all figures would be more or less fallacious, because it all depends upon the dose?—Undoubtedly.

8174. And to some extent, I suppose, it depends upon individual idiosyncrasy?—I doubt it. From experiments on animals I find no tendency of that kind. I should say that there is very little individual idiosyncrasy.

8175. It is almost impossible, I suppose, to get an intravenous injection by means of ordinary snake bite?—It might occur; in that case it would be fatal very rapidly.

8176. On the chance of its being intravenously injected, how soon would it be fatal—in a couple of hours?—If intravenously it would cause death quicker than that; it would not be likely to take more than 10 to 15 minutes, but even less.

8176A. In the ordinary case, when a fatal dose is not received, I suppose it is where clothing intervenes?—Yes, that is an important point. In this series I think that every one of the cases was inoculated not through clothes, but on the hand or foot.

8177. On the bare hand?—Yes, that is an important point in the series where the snake was identified.

8178. Because if a man was bitten through the trousers?—He might probably escape altogether.

8179. Have you tried the effect of permanganate of potash on scorpion poisoning?—I have not done so myself, but it has been tried in Egypt, I am told, and it has been found to be equally effective. It is used now in Egypt, I am told, for scorpion poisoning.

8180. Rubbed in the same way?—Yes, either rubbed in or injected.

8181. A great many natives in India are bitten by scorpions, are they not?—We do not hear much of it.

8182. Because the cases are not usually fatal?—Yes.

8183. Although they are very painful?—Yes, they are common enough. I have used the permanganate of potash in another way. I have found it very useful in preventing tetanus infection in wounds.

8184. Will you tell us anything that you know about that?—In Calcutta tetanus is an exceedingly common disease.

8185. Because the whole of the road dust is full of it?—Yes; so I carried out some experiments by putting a little dust from the Calcutta road under the skin of a rat, and I found that five out of six died of tetanus; but if I added a few crystals of permanganate of potash to the dust, then I only lost one animal, and that was not from tetanus. I then carried out a further series of experiments by scarifying the skin, rubbing in the dust, and using strong permanganate of potash solutions at different intervals, and I found by that measure also, even up to an hour afterwards, it was nearly always possible to prevent tetanus infection, although the control animals developed the disease. But permanganate of potash has also been used at my suggestion in cases of leopard bites. When persons are shooting in the wilds and there is no doctor near, permanganate crystals instilled into leopard bites, which are usually very fatal, in severe cases have prevented serious results from blood poisoning.

8186. The serious results arising simply from the dirtiness of the leopard's teeth?—Yes. I also suggested that it was worth trying in hydrophobia, and one of the medical officers in the South of India has used it in a number of cases of dogs which have been bitten by mad dogs, and a number of them have been saved apparently, and he is very enthusiastic about it.

8187. Using it according to your method of making an incision and rubbing in the actual crystal?—Yes, so that evidently of late it appears to be of very wide application.

8188. Is there a considerable death rate from tetanus at Calcutta at the present time?—Yes.

8189. And when a man scratches his hand on a road in Calcutta is this treatment applied?—They are

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using the tetanus serum in Calcutta as a prophylactic. It is of very little use after symptoms have developed, but it is now used at the Medical College Hospital in all wounds that are thought to be likely to be infected with tetanus; and it has very materially reduced the number of cases of tetanus at the Medical College Hospital in the last few years.

8190. What is the incubation period of tetanus?—It varies very much, from seven to fourteen days. In the early days incubation it is much more fatal.

8191. In fact, the larger the dose the quicker the disease comes on?—Yes.

8192. When you have used the permanganate of potash have you also used the antitetanus serum?—In these cases in the Calcutta Hospital they only use the serum.

8193. They have not used the permanganate?—No.

8194. In your opinion, would it be desirable, say, first to use the permanganate?—Personally, I should do so.

8195. You would use the double treatment?—Yes.

8196. You would first try the permanganate?—Yes, and then get the serum as well, as a prophylactic. I have not had cases of my own to treat.

8197. I wanted just to get clear whether there had been any cases in which you used the permanganate of potash alone and where the treatment has been successful, other than experiments on animals?—Only in experiments on animals, so far as I know. I have not had any actual cases yet. I have not had any opportunity of using it myself, and I do not know how far it has been used.

8198. It would not be safe to use only the permanganate on man without trying the antitetanic serum as well?—I should use both where they were available; but the permanganate is much more likely to be available.

8199. Is there any danger from the antitetanic serum itself as apart from the disease?—I have never seen any.

8200. What symptoms does it produce?—A slight rash or edema, just as in diphtheria, but I have never seen any serious symptoms. It has even been used for injecting into the spinal cord recently in cases of tetanus, without any harm at any rate, and one case has recovered; but tetanus is a disease from which a great many cases recover, and you want a large series of cases to test it.

8201. Tetanus is not an absolutely fatal disease; there may be recovery with ordinary treatment?—Yes, especially under the chloral treatment.

8202. (Dr. Gaskell.) Do you know anything about the treatment for snake bites by swallowing the poison glands?—That was very carefully tested by Professor Cunningham. There was an idea in India that snake charmers produced immunity by swallowing snake venom; but Professor Cunningham fed a goat for, I think, two or three years on large quantities of venom, he gave some very large quantities, and in addition he gave all the remaining portions of any that he had used in his experiments, and he subsequently tested that goat with just enough to kill an ordinary goat, and it died in the ordinary time; so that I think we may take it as quite certain that it does not produce any immunity in that way. And that is what one would expect, because one would expect an albumose of that nature to be destroyed in the digestion and not absorbed as such.

8203. With respect to what you answered just now about antialbumoses, you know that if the serum of an animal is injected you can get an antiserum?—Yes.

8204. So that it is not only a question of albumoses?—No.

8205. It applies to the various proteid bodies?—Yes, I was thinking at the time of the practical application.

8206. Do you think that snake charmers immunise themselves by small inoculations?—I think they do. I think that they often get little scratches, and get a little venom injected in that way. We had a man for many years at Calcutta who certainly had been scratched in that way repeatedly, and he was perfectly convinced himself that he had got immunity; and I know that Professor Cunningham thought so, too.

8207. (Dr. Wilson.) You said that in experimenting with this antivenin you found that it acted much more efficiently and rapidly if you injected it intravenously and not subcutaneously?—Yes, much more so.

8208. That, of course, would apply to the use of all sera?—I think so.

8209. In human beings as well as in animals?—Yes.

8210. So that to get the most efficient use of a serum, you would say from these experiments of yours, that instead of injecting it subcutaneously it ought always to be used intravenously?—Certainly in the case of snake poison where you want to get an immediate rapid effect, and, I think, also in many other cases possibly.

8211. But, of course, you admit that owing to the many difficulties which you have described, these sera, having to be manufactured for different snake poisons, until you get a polyvalent serum they are of little practical use?—That is my personal opinion.

8212. And I think you admitted also that in almost all cases which were brought to you, in the first series at all events, a ligature had been applied?—They were reported to me. I have not got full details of all, but I find there were four cases where a ligature was applied at once.

8213. And in the second series was a ligature also applied there; it would be, I suppose?—In regard to them I have not been able to get full details. I have corresponded with the people.

8214. I ask the question, because in China, according to a statement made by Mr. Cantlee, the well-known surgeon who has practised, as you know, out in China for a great many years, the Chinese immediately adopt the ligature; and he also says that it is a fact that very few Chinese die of snake bite. That is his positive statement?—I should like to know what the snakes are, and also how much venom they ordinarily emit, how many times what would be a fatal dose to man. You want to know all those factors. That is why I lay so much stress on our cobra cases, because I know that a cobra does eject on the average ten times a fatal dose.

8215. But, on the other hand, does it not appear that some considerable value might be attached to the use of the ligature?—I hoped so; but I may say that I did some experiments, but was not able to find it. It is a thing that wants further working out.

8216. Is it made part of the instructions which are issued with this lancet, that the ligature is to be applied immediately?—Yes. In my experiments with permanganate treatment ligatures were applied in nearly all the control animals which were injected with snake venom, but not treated with permanganate, but they all died in about the usual times, so that the ligatures alone completely failed to save any of them, even when only one fatal dose was injected.

8217. Now, with regard to permanganate of potash, are there other agencies, do you think, which would act equally well—for example, caustic potash?—Chloride of lime and gold chloride were found by Professor Cunningham to destroy snake venoms, but they are not nearly so practicable apparently as the permanganate; the gold chloride is exceedingly expensive while the chloride of lime is unstable.

8218. Have carbolic crystals been used?—I can not tell you absolutely. I know that Sir Joseph Fayrer and Sir Lauder Brunton tested a large number of things. I think that it was tested, but I cannot be quite certain.

8219. In early days, of course, permanganate of potash was a favourite antiseptic agent?—Yes.

8220. And in using it for tetanus, of course, in all the cases that go to you the wounds will be washed and cleaned, and so on?—Yes, but it is a curious thing with tetanus that ordinary antiseptics such as carbolic are certainly not as efficient as permanganate of potash. I remember a case of my own where the patient had been dragged along the road in London, and dirt had been ground into the bone. I scraped it out most thoroughly, thinking of tetanus, and I used strong carbolic; but the patient developed tetanus, and Mr. Collier, the surgeon, informed me the other day that he had had similar results; so that permanganate of potash does appear to be specially efficient as an antiseptic against tetanus organisms, possibly. I think this is owing to its strong oxydising action against the tetanus bacillus, which is an

anaerobic organism which cannot live in the presence of oxygen.

8221. But the serum now is only used as a prophylactic; it has failed as a cure?—It has not been successful as a cure so far as I know in my experience.

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8222. (Sir William Church.) Now you are going to be good enough to give us some information on the subject of rinderpest. It appears that rinderpest had attracted the attention of the Indian Government for many years?—Yes, as far back as 1881, I think.

8223. And they have a bacteriological laboratory under their own supervision?—Yes, entirely for studying diseases of animals.

8224. Especially for studying rinderpest and other diseases of animals?—Yes.

8225. And in 1899 you were in charge of the laboratory?—Yes; it was in November, 1898, that I started, and I was in charge all through 1899.

8226. Would you tell the Commission what the work of your laboratory then was?—At that time I was appointed to act for Dr. Lindgard, who had gone on sick leave. Professor Koch had been working in South Africa, and he had also visited India shortly before. The whole question of inoculation against rinderpest was being worked at in South Africa, and the Government of India wished the methods there used to be tested, to see how far they were applicable to the conditions in India. There were two chief methods. One was the bile method of Professor Koch, which had done great service in the early part of the outbreak in South Africa, but which was at that time being discarded in favour of the serum method. The serum method was that of Turner and Kolle. It consisted in preparing an antidotal serum by injecting large quantities of virulent blood obtained from an animal suffering from rinderpest, because the organism had not been discovered, but the blood is known to contain the infection; and by repeated injections in this way into an animal which had recovered from rinderpest and so become immune, it was possible to produce in the blood of cattle a serum which had a very strong antidotal action against rinderpest. This serum could be used in two different ways; it could be used as a prophylactic injection by injecting the serum alone into an animal which was exposed to infection as in an outbreak. It was found that injection of serum alone would protect the animal for some weeks or months, and in Indian conditions would tide the animal usually over the particular outbreak in face of which it was infected. But that method of injecting serum alone only produces a temporary immunity of comparatively short duration. The second method of using the serum, the serum simultaneous method of Kolle and Turner, was to get permanent immunity for life by producing a slight attack of rinderpest in the animal injected; and this is done by injecting a small quantity of virulent blood containing the infection in one side of the animal, and at the same time injecting in a separate place a measured quantity of the anti-rinderpest serum, the strength of which has been previously ascertained. By this method it is possible to produce a mild attack of the disease in the animal, which will protect it for the rest of its life. This method was very successful in South Africa, but I found that it did not work very successfully in India. The reason for the difference was this. In South Africa the disease was a new one, so that all animals were equally susceptible, and consequently it was possible to regulate the dose of serum so accurately as to produce a mild attack of the disease in 90 per cent. of the animals, and yet not to have more than a half per cent. of fatal cases. In India, on the other hand, I found that owing to rinderpest having been present for very many years, the susceptibility of animals varied very greatly, so that it was impossible to so regulate the dose of serum as to get the results which were obtained in South Africa. If a large dose of serum was used so as to be sure of not getting many fatalities, then only a small number of animals, from 30 to 50 per cent., actually developed an attack of the disease, and so only that number obtained permanent immunity. If, on the other hand, the dose of serum was reduced so as to get a larger number of attacks, then the mortality was much higher than they got in South Africa. I found, however, that I could get over this difficulty, after long experiments, by using a large dose of serum to start with at the same time that the blood was injected, so as not to lose any animals, and then to any animals

which did not react, which got no temperature or fever, or any signs of the disease at the end of one week, I found that I could then safely give a second dose of virulent blood, and then I nearly always got a reaction with permanent immunity; the reason being that those animals which did not react at the first inoculation were less susceptible than the others, and the serum was too strong to allow the virus to develop, but at the end of a week some of the strength of the serum had passed off, and then you could get the reaction safely. And this method I called the re-inoculation method. This method has been tested in the Philippine Islands, and is now being recommended in the Philippine Islands as preferable to all other methods of obtaining permanent immunity; but in India the method of simply injecting the serum alone is most generally used, because the more complicated re-inoculation method requires more skilful observers than are generally available. I may say that at the end of my year's work I made arrangements for turning out 100,000 doses of serum a year. I also inoculated a large number of animals myself in actual outbreaks. In two districts I inoculated over 500 animals that were actually in the same sheds with other infected animals, and at the same time I had notes of all the other animals similarly exposed to infection which were not inoculated because the villagers did not agree to it. These were all subsequently reinvestigated at the end of two months, and it was found that no one of the 500 animals which had been inoculated had developed rinderpest later than four days after the inoculation—that is to say, within the incubation period of the disease; so that those who developed the disease within four days must have been infected before the inoculation. We may say, therefore, that no animal which was not already infected subsequently developed the disease after inoculation in that outbreak. On the other hand, of about an equal number of animals similarly exposed to infection and not inoculated, one half developed the disease and half of those died, making a mortality of 25 per cent. of the uninoculated animals. These results were so conclusive that the Government of India arranged to turn out 100,000 doses of serum a year. Many thousands of inoculations have been done year by year since, and, I think it was last year, a second laboratory was sanctioned in the South of India because the one laboratory is not sufficient to make enough serum—the results have been so satisfactory.

8227. In what way do you deduce the variable susceptibility to the disease of cattle in India. What is the cause of the presence of rinderpest for so many years in the country?—Rinderpest is always present in India, and a very large number of districts are affected every year. You will find that one district will get rid of a lot of disease in one year, and after that there will be comparatively little for a few years and then you get another outbreak again; and when you come to an actual outbreak there will be a certain number of animals which perhaps have had the disease in a previous outbreak several years before, and those will be permanently immune.

8228. Then an epidemic of rinderpest in India shows itself in the same locality during the life of the same animals?—It will not affect animals which have had it before, but there certainly is a very variable susceptibility.

8229. The interval between epidemics in localities in India is a short one?—Yes; it varies very much in different parts.

8230. But frequently it is short?—Yes. I may say that the natives are so fully aware of the protective action of an attack of rinderpest, that in the hill parts where the rinderpest is comparatively rare, and when it occurs it carries off 90 per cent. of the animals, an animal which has had rinderpest is worth double the value of one that has not. That is a very definite fact, and you can produce equal immunity by inoculation by the simultaneous or re-inoculation methods.

8231. That is your explanation of the varying susceptibility of Indian cattle?—Yes, that is so far as I can understand it. It is very difficult to explain, but it is a fact at any rate.

8232. Has the value of re-inoculation of cattle in India been recognised by the natives?—The re-inoculation is used very little in India, because, as I say, it is more difficult to carry out.

8233. But the value of your serum treatment has been recognised by the natives?—Undoubtedly. To give my own experience, after doing inoculations in three districts, the results of which I have just given you, I had applications from the natives to go back and do more in each place, although at first many of them had refused to have it done.

8234. From what class of natives would that be?—All the poorer village class; the ordinary agricultural labourer.

8235. (Sir Mackenzie Chalmers.) In what districts?—In Alleghur and Bulundhahar.

8236. In the north-west?—Yes.

8237. (Sir William Church.) Would they be Hindoos?—Mostly Hindoos, but a good many Mahomedans as well. I found when I got to a village that if the disease was still very prevalent and they were still losing animals, I had no difficulty in getting a good many done. I simply explained that it was just like vaccination, that as by vaccinating their children they were prevented from getting small-pox, so by vaccinating these animals we could prevent the animals getting rinderpest, and they understood it at once.

8238. So that these uninstructed and perfectly impartial natives were rapidly convinced of the value of it?—Yes; it touched their pockets, too, I may say. I found that some men who were selling cattle, when we wanted to have them inoculated said that they would not have it done because they wanted to sell their cattle, and they did not mind if the cattle got it after they were purchased; but when they belonged to the villagers themselves they were very keen about it.

8239. Do the animals suffer very much from which you get the blood—those in which you artificially produce rinderpest?—They certainly must suffer. The disease is more nearly like typhoid than any other human disease; you get ulceration of the intestines, and you get a high temperature, 104° or so, and the animals soon became drowsy rather like a typhoid patient; and when they get to that stage I do not think they suffer much, but they do get violent diarrhoea and running from the nose. They must suffer to some extent.

8240. Do you keep these animals until they die, or are they killed?—As soon as they have fully developed the disease they are bled to death. In that way you get your blood for injecting, and the animal is put out of pain.

8241. Have you any figures that you can bring forward to show the loss of life among animals during the last few years?—No, I have not. The disease varies very much. You get some epidemic years when they lose 100,000 animals in one district. In other years the disease is much more scattered.

8242. But if there is any value in this treatment, the epidemic ought to get less and less severe so far as mortality goes?—It is only the last six years that it has been introduced, and I have not been able to get at the Veterinary Reports. The figures have been published in Government Reports—probably I can get them at the India Office—showing the results of inoculations in the last few years.

8243. You think that figures will be available from the India Office?—I think I can get you figures as to the actual effect of inoculations in outbreaks.

8244. (Sir Mackenzie Chalmers.) It is only a very minute portion of the whole cattle in India that are treated?—There are several million cattle in a single district, so that you could not expect at present a marked effect on the whole of the cattle in India; but there are figures showing a very material reduction of mortality in particular localities; I think I can get you some.

8245. (Sir William Collins.) Is cattle plague distributed equally all over India?—Yes, I think pretty well all over India.

8246. Are there very large tracts of India which are free from it?—Not that I know. The hill tracts only suffer occasionally. The hill tracts may be free for several years at a time, but then you get very bad outbreaks afterwards. But in the plains it is practically always present, so far as I know, scattered about.

8247. What is the recognised administrative method of dealing with cattle plague in India?—It is practically impossible to do anything.

8248. Is slaughter of infected cattle carried out?—No, the natives will not allow it.

8249. Is there no attempt at the so-called stamping-out system?—It is quite impossible. You have to remember that the veterinary staff in India is exceedingly small. There is one veterinary officer for a whole province. They are being increased, but require to be increased more.

8250. (Sir Mackenzie Chalmers.) A province having from 30 to 70 millions now?—Yes, you have 50 millions in Bengal.

8251. There used to be 70 millions; now, since the partition, there are 50 millions?—Yes.

8252. (Sir William Collins.) Is the combined method of which you have spoken used now with Government sanction?—It is used where you can get the favourable conditions. It has been used in Assam among planters and people where it can be thoroughly carried out and where they understand it, but it is not used much by villagers; they prefer the simple serum method, because it tides them over their difficulties, and also because it does not make the animals ill. In several villages where the disease was not very prevalent I found that they would not allow the inoculation to be done. I had to tell them that by the serum simultaneous method the animals might be ill for two or three weeks, and they would not allow it. By the serum alone you get over the difficulty.

8253. Has the Government discouraged the use of infected blood because of the danger of communication of the disease to the animals?—No, I think not. Wherever it can be done it is done, but it is quite exceptional to find a place where the conditions are favourable in India.

8254. Do you think that there is a danger of propagating the infection by the use of this combined method, inasmuch as you are using the actual infected blood?—You only use it in an actual outbreak, where there is an enormous amount of infected cattle.

8255. But where there is an enormous amount also of uninfected cattle, I suppose?—But by this method the symptoms are very slight—nothing but a little fever; you do not get running from the nose and discharges which carry the infection, so that I very much doubt whether there is any material infection from animals with these very slight attacks as the result of inoculation.

8256. There have been previous attempts at vaccination against cattle plague, have there not?—Before South Africa?

8257-8. Yes. Have you heard of McPherson's experiments in Bengal a good many years ago?—Yes; he did some.

8259. He tried communicating the infection of cattle plague to the human subject, did he not?—I do not know.

8260. Have you heard of Furnell's experiments in Assam?—What date was that?

8261. I think it must have been in the thirties?—I do not know about that.

8262. Vaccination was tried against cattle plague, was it not—the use of cow-pox in this country in 1866?—I know nothing about the outbreak in England at that time.

8263. Are you able to institute a comparison between the so-called stamping-out method, if that were practicable, and the method of preventive inoculation?—Preventive inoculation I should say would be a thousandfold preferable to stamping out, as saving such an enormous number of animals which you otherwise would have to sacrifice.

8264. You spoke of a particular outbreak in which the inoculated animals did not suffer, or at any rate did not suffer anything like so severely as the uninoculated. Where was that?—In the United Provinces, in Alleghur and Bulundhahar.

8265. Has that been the subject of official report?—Yes, that is all in my Report to the Government.

8266. What is the date of the Report?—It was published in April, 1900.

8267. In what volume would that be?—It is a Special Report on Inoculation against Rinderpest—Report of the Laboratory.

8268. To the Indian Government?—Yes.

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8269. Is it published in this country?—It is available in this country; it gives you the details and all my experimental results.

8270. (*Sir Mackenzie Chalmers.*) Of course as regards the stamping-out method by killing the cattle there is a religious objection in India?—Absolutely.

8271. It would be impossible in any Hindoo community?—Absolutely impossible; they would not consider it for a moment.

8272. So that, apart from the relative values, it would be an impossible thing for the Government to undertake?—Yes, absolutely impossible.

8273. There is one question that I omitted to ask you about snake poisons; I suppose it would be a very long time before the ordinary cultivator could ever be induced to carry about the permanganate of potash and the lancet?—Yes, the ordinary cultivator would not do it. The idea is to distribute it to the head men in the villages.

8274. In the first place, the ordinary cultivator has no clothes—no pocket, at any rate?—That is so.

8275. And no clothes in which he can carry it?—No.

8276. And it takes a long time to get an idea into his head about the treatment?—Yes, they tie up their pice in the corner of their dhoti; they might do it in time.

8277. (*Mr. Tomkinson.*) You are aware, of course, that there was a tremendous outbreak of rinderpest in this country some 40 years ago?—Yes.

8278. And no cure or preventive was known at that time?—No.

8279. They had to fall back upon the pole-axe altogether?—Yes.

8280. Is this discovery of yours protective only, not curative?—It is not curative. It has been used in that way, but it is not very efficient. I cannot tell you whether in the last few years they may have improved it.

8281. Then, so far, no cure has been discovered for an animal that is actually and virulently attacked?—I think not.

8282. Do I rightly understand that the instance about which you were asked by Sir William Church, as to the suffering of animals, was in the case of an animal being experimented on and inoculated with the virus of a dead animal?—No; I said that in preparing the serum, as there is no organism of the disease known, you have to take the blood of an animal that is suffering from the disease and inoculate it into the animal in which you produce the serum so that for that purpose you have to inoculate a certain number of animals with the disease.

8283. With the blood of the animal that has been attacked, naturally?—Yes; you have an animal which you inoculate with the disease in order to produce the disease, then you bleed that animal and use its blood to inoculate into the animal in which you produce the serum.

8284. From that second animal the serum is produced?—Yes, that animal is not killed.

8285. How much does that animal suffer?—That animal does not appear to suffer at all. You have to use an animal that has had an attack, or one which you have inoculated yourself; after that they get practically no reaction.

8286. Then they develop the serum which is required for a prophylactic?—Yes.

8287. Then in what way is that serum drawn from that animal's body?—It is drawn off from a vein in the neck, taking sufficient blood not to injure the animal, and then after a certain time you can take some more blood.

8288. Is it blood or a secretion?—It is the blood itself from which you separate the serum by a centrifugal machine, and then you add a little carbolic or lysol antiseptic and bottle it.

8289. Have you any idea how the disease originates otherwise than by infection or contagion?—It originates by contagion as a matter of fact. Unfortunately in India it is sometimes spread by the Chamars, the leather workers, who actually spread rinderpest in order to get the hides of dead cattle. It is a very difficult thing to stop.

8290. But any origin except that of infection or contagion has not been discovered?—No.

8291. (*Dr. Wilson.*) I suppose it is the horse that is used for rinderpest serum?—No; cattle are used. Horses are not susceptible to rinderpest.

8292. Would the serum, then, of an animal, which had an attack of the disease naturally, be looked upon as a prophylactic serum?—It would have some antidotal action, but not nearly strong enough to be of value; you would have to heighten it by further injections.

8293. Then the serum is used alone. You do not get the dual system?—No, we use chiefly, almost entirely, the serum alone in India.

8294. If a mild attack of the disease is induced by injecting the blood of a diseased animal it is merely a repetition of the old process of inoculation against small-pox; I mean inoculation from mild cases, as it were?—You cannot produce a mild case of rinderpest by inoculating from a mild case.

8295. It was believed in old days that you could produce small-pox in that way?—I do not think that is so with rinderpest. If you take a mild case it will produce a very violent case in an animal that is fully susceptible.

8296. By infusing the blood, you say, that a mild attack of the disease is induced, and, of course, that would produce immunity?—Permanent immunity is produced by injection of both virulent blood and the protective serum in a proper dose.

8297. But by the serum treatment it is temporary immunity?—It is temporary immunity which tides over that particular outbreak, but in a year's time the immunity will probably have vanished.

8298. So that you will have to go on?—If you get another outbreak you will have to inoculate other animals.

8299. (*Sir William Church.*) Could you tell the Commission anything about the working of the Kasauli Institute; do you know anything about it?—I know something about it. I have not worked there myself. Originally private subscriptions were raised in order to start a Pasteur Institute so that the treatment of hydrophobia could be carried out in India and the very serious delay of people going off to Paris could be avoided. When I went out to India first in 1893, subscriptions were being raised, and a year or so later one of the Anti-Vivisection Societies, I do not know which, sent out appeals to all the native Rajahs and people who had subscribed to this Pasteur Institute in India or had promised to subscribe to induce them to withdraw their subscriptions. However, Surgeon-General Harvey, at the Indian Medical Congress in 1894, gave an address on this question of anti-vivisection which neutralised that effort. Later on it was found that sufficient subscriptions could not be raised in that way, or, at least, before sufficient subscriptions were raised to do it in a private manner, the Government took it up, and I believe they gave a contribution equivalent to the amount which they had been spending on sending Europeans to Paris to be treated, and in that way the Kasauli Institute was started. The results are considered to have been so valuable that within the last year a second Pasteur Institute has been started in the south of India on account of the enormous area which has to be covered.

8300. (*Sir Mackenzie Chalmers.*) Do you know the results of the treatment at the Institute?—The figures are practically identical with those of Paris itself; the results have been very favourable.

8301. (*Sir William Church.*) Hydrophobia is not an uncommon disease in India, is it?—It is quite common.

8302. Has the population recognised the benefit of the Institution, or is there any difficulty in getting it recognised?—A large number of natives attend the Institute at the present time.

8303. Without pressure being placed upon them?—I think so.

8304. They recognise its value?—I believe so. I have not worked there myself.

8305. (*Sir Mackenzie Chalmers.*) Where can we get the exact figures?—I will look them up for you. I think I can find them for you.

8306. (*Sir William Collins.*) Is the Laboratory supported partly by Government aid and partly by volun-

tary contributions at the present time?—Yes, the original subscriptions were all funded. The Government pay most of the expenses, I think, now.

8307. (*Sir William Church.*) Is there anyone in this country at the present time who can give us information about it?—I do not know that there is anyone in the country who has worked there, but I shall be very pleased to look up the figures.

8308. I suppose the India Office would have them? Yes.

8309. (*Dr. Wilson.*) Is the Pasteur system of treatment carried out there?—Yes.

Rev. JOHN PAGE HOPPS, called in; and Examined.

8312. (*Sir William Church.*) You appear as a witness on behalf of the Social Purity Alliance?—I think so; that is what I understand.

8313. You are a minister of religion in London?—Yes; Little Portland Street Chapel.

8314. Of what denomination?—Unitarian.

8315. You wish to give your opinion as to the ethical bearing of vivisection upon public safety and public morals?—Entirely that. I should like to say first that I am very glad to hear the question asked me just now whether I am prepared to speak on the ethical side of the matter connected with vivisection, because when I was asked to give evidence, I thought it would scarcely be admissible, and I thought I should almost have to apologise for consenting to be presented as a witness, seeing that I am not in any way an authority on the subject of vivisection, but I have been informed by those who asked me to attend, that this Commission is open to receive personal impressions and opinions as to the ethical bearings of vivisection upon public safety and public morals, and, if I may venture to say so, upon those who practice vivisection. Moreover, on the broad ground that the law should be mindful of morals, an advocate for applying the claims of ethics to vivisection, may perhaps justly have a *locus standi* here. As to matters of fact, I have therefore very little to say; mine are only inferences based upon facts, and I must not be personal. One possible justification for the presence of such a witness as I am may be found in an inference which very seriously bears upon what I have called "public safety." The practice of vivisection, I suppose it is granted on all hands, undoubtedly produces a fascination for it which, in some cases, has confessedly risen to joy, and even to the perception of supposed religious sanctions.

8316. I would like to ask you at that point what evidence you can bring forward to support that statement?—I am proceeding to that. This may explain the dangerous suggestion made by one practitioner, that the hospitals ought to be supported, because "their inmates are made use of otherwise than for treatment"—a highly significant and sinister suggestion, which has very properly called forth from the other side the suggestion that the vivisector "is never safe to be trusted with a hospital patient on an operating table, and a tray of knives at his side." I do not say that is my opinion; I only say that I noticed both those statements.

8317. (*Sir John MacFadyean.*) Might we have references to the statements?—I should be very glad to supply them.

8318. Might we have them now. Who made the statements?—I have seen them stated. I will be very glad to supply the references later on. I am simply giving my own opinion in what I say. With regard to statements made by others I simply pledge my honour that I have seen them.

8319. (*Sir William Church.*) We do not for a moment doubt that you have seen them, but it is really quite necessary for us to know where you have seen them in order that we might see the nature of the context in which the statements have been made?—I think with regard to all other statements I do provide the authorities. An experimenter quite naturally desires subjects, and the temptation to seek them is great. There is a public peril here, because, unfortunately, in such a matter as this we must take the practitioners at their worst—in this case also the chain being no stronger anywhere than at its weakest part. Of course, it is well known that in Continental hospitals experimental vivisections of patients is common. The revelations of the "Deutsches Volksblatt" concerning the conducting of dangerous experiments purely for practice, and

8310. But is not this new treatment, the permanganate of potash treatment, likely to supplant the Pasteur system?—We should not like to rely on it entirely by itself. I think it is a most useful thing to start with to save any loss of time.

8311. Is there a tendency to combine both systems?—In the south of India the permanganate system has been used more particularly in dogs in hydrophobia cases. How far it has been used in the human subject I do not know. It is only last year that these results were published. I do not think the system is very generally known yet.

the inoculation with contagious diseases of young children and pregnant women, are not likely to be soon forgotten. One distinguished surgeon in Vienna frankly told the correspondent of the "Morning Leader" that in most free hospitals "patients are made use of in the cause of science," adding, "Tests must be made on somebody, and patients in charity hospitals are generally the only available subjects."

8320. I am afraid I must trouble you for the references. You have only given us the "Morning Leader." We wish to know who the Austrian surgeons are who have stated those views?—I am quoting from the "Star" of January 27th, 1899.

8321. We cannot accept the "Star" as an authority for what they state that people said in Austria?—Then I give it for what it is worth.

8322. (*Sir John MacFadyean.*) I submit another point. This evidence relates to Vienna—which is not germane to our inquiry?—I give it for what it is worth. I quote the "Star" of 27th January, 1899.

8323. (*Colonel Lockwood.*) It only refers to foreign hospitals?—We are all human beings, and what is human nature is human nature in Austria or in this country.

8324. (*Sir William Church.*) But you surely must realise that papers like the "Star" and many other papers are not very particular as to the truth of statements which they make. They will extract from a person's speech or writings a sentence which may bear a very different meaning from that which it bore in the original statement of the speaker or author?—I am very well aware of that.

8325. Therefore we must be referred, if you please, to the original published book or speech?—It was a statement made, according to the "Morning Leader" and the "Star" to an interviewer.

8326. It applies to foreign hospitals out of this country, where, of course, our law does not apply?—I only give it, as I think I said, as showing the natural effect upon a man who takes a very great interest in the subject—that is human nature; a man would like—I think I should myself, if I were a vivisector and thoroughly believed in it, and it was my profession—be very glad to get subjects in any direction, and if I was shut up in one direction I should try another and should pursue it to the very bitter end.

8327. (*Sir John MacFadyean.*) Might I ask whether there is any evidence that the medical man in Vienna who made these statements had ever practiced vivisection?—I know no more than I quote.

8328. Then it is not stated that he ever practiced vivisection. What bearing his inclinations might have upon the practice of vivisection does not appear?

8329. (*Sir William Church.*) We will not interrupt you, Sir, any more, but we will ask you perhaps after your statement has been made some further questions on it?—As to the ethical effects of vivisection upon the community, it is surely reasonable to suppose that the example set by such highly-placed men as adorn the medical profession will be followed by others, not so highly placed (I am speaking now of English practitioners) and perhaps not inspired by such beneficent intentions. The old jest, descriptive of the typical Englishman: "It is a fine day, let us go out and kill something," has its extremely serious side, and is connected with all the foul associations of what is called "sport." Of course it will be said that vivisection is justified because it has its uses in relation to the treatment of human beings. As to that I say nothing, except to remark that the medical profession is by no means unanimous as to it, and to add this doubt of my own—whether observations made during the special and artificially produced agony or distress of a dog

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on a dissecting table may not mislead when applied to a man in his bed. The expert ought to know best, but the common sense of it seems to be that every function and act of the organism would be affected, and, in some cases, violently affected, perhaps reversed, under torture. We know that secretions of every kind are so affected by fear, resentment, anger, shock or violent pain. What then would be the value of the observed action of drugs with a subject so affected? One can quite readily see that opinions arrived at in such circumstances might be dangerously wrong. Can any thoroughly reliable inferences be drawn concerning an acutely conscious man from the behaviour of a conscious, an unconscious, or a horribly tortured dog? Surely the torture is a *plus* which may mean a great deal. We know that anger can poison a mother's milk, and that a furious temper or filthy thoughts can produce vile tastes in the mouth, and play havoc with the heart, or, indeed, with every bodily function. Is it not highly probable that similar emotions *plus* agony may induce conditions and consequences which will go far to invalidate any conclusions that may be arrived at respecting the action of drugs? May it not be even dangerous to infer that what happens to an outraged—even if unconscious—dog will happen to a tenderly cared for child or man? But to return to my own province, the ethical consequences of vivisection, and may I venture to say, upon the practitioner himself. Here I really must apologise, I must take the preacher's privilege, for here nearly all is inference or moral probability. What, for instance, must be the ethical effect upon a man who revels in the disgusting work—who calls the ideal vivisector an "artist," and says, "The vivisector must approach a difficult vivisection with joyful excitement. He who shrinks from cutting into a living animal, or approaches an experiment as a disagreeable necessity, will never be an artist in vivisection!" The dreadful character of the vivisector's work will doubtless be described by others, and it may at all events be said of it that if a necessity, it is a painful necessity, and that the natural inference of it is the blunting of the fine moral sense. It may be said that this is the vivisector's business and that it is his own affair if he likes to take his chance. That is not so. The operator may be a physician, or a maker of physicians. He helps to create the atmosphere in which a powerful and beneficent, or possibly a formidable and maleficent, profession works. There is no need to be personal over this matter. There would be no end to that. It will suffice if we consider probabilities. What is likely to be the effect of practising vivisection upon men who so greatly need all the refinements of sympathy, if not its emotions? We seem to be living in a crisis of nervous maladies requiring not so much drugs as direction. The remedy for most of our maladies is not to be found in the carcase of a cat but in the soul of a man; and it is the more or less understood consciousness of this that has led to the great revival of "Confession" and the guidance of the priest in England. The question that trembles on the lips of thousands of sensitive spirits to-day is: "Can'st thou not minister to a mind diseased?" Whether the remedy sought will help or not in the end seems doubtful, but what is certain is that the Father Confessor, if he is to be of any use, must be skilled, not in the behaviour of muscles and glands and membranes, but in the subtle windings of passion, the treacheries of motive, and the dens or Elysian fields of love. The torture of a dog, the dabbling in blood, the abstraction of attention from agony, would spoil him "for the cure of souls"; and is not "the good physician's" vocation near enough to that of the true priest's to make some analogy, both of duty and fitness, useful here? Might it not even be said that the functions are often practically the same? If so, the paths of preparation for the one must be those of the other also. What tends to make the one gentle and keen, sensitive and pitiful, strong and wise, is needed by the other. But bloodshed blunts; and we are specially assured by zealous vivisectors that this is so, and ought to be so. How can it be otherwise? How could a man bear to carry through the tortures we have indicated, unless he acquired the necessary insensibility to, or forgetfulness of, the inflicted pain? And yet it is difficult to see how this could be acquired without deterioration. A man cannot win character and qualities all round, for all classes of work; and, though he may not be conscious of it, his creed is shaped by his actions, and not his actions by his creed. The great Dr. Abernethy's testimony has not been invalidated by time, "Vivisection

has the direct effect of deteriorating the moral sense, and, once the moral sense is destroyed, it is impossible to foresee the consequences." I am willing to believe—I am anxious to believe—that in the great majority of cases vivisectors have honestly persuaded themselves that they are doing man service, and are suffering no harm in doing it. But it does not follow that the harm is not done because he who suffers it is unconscious of it. The unconsciousness may be a part of the harm, in the development of a certain narrowing callousness which may be almost like the closing of the Holy of Holies in the inner self. Can a man ever inflict pain, except for the actual sufferer's good, without deterioration of affection and pity? But affection and pity belong to the sphere in which a living man can alone become "a living soul." Knowledge and insight in that divine sphere are worth infinitely more than any knowledge and insight that can be gained in the vivisector's laboratory; and knowledge and insight gained there may be paid for at a fearful rate. The operator may be so absorbed in his study as to be, in a way, oblivious of the disgusting details, but that makes no difference, except as a grave suggestion as to what is happening to the operator. It has been very properly decided in this country that cruelty to animals is an indictable offence. Why? Surely because it is an offence against public decency and public morals to inflict torture. So much has this been felt that even in regard to criminals, we have steadily and of set purpose drifted away from torture as ethically bad. Cruelty, then, in any form, has upon it the brand of social and legal wrong-doing, and the burden of proof is heavy upon him who wishes to have it licensed by law. Suggestions as to the ethically bad effects of vivisection upon the operator are only inferences, but, of recent years, there have been sufficient facts to warrant them. Only last year, a professor—Professor Metchnikoff, at King's College, May, 1906—reported in the *Journal of Preventive Medicine*, August, 1906, lecturing at King's College, London, asserted that "in questions of health, morality should not attempt to lead hygiene, but should rather follow her," again a very sinister suggestion, which goes a long way towards justifying the statement that these questionable pursuits lead to questionable doctrines, if not questionable ethical practices, and, in the case of this professor, one is hardly surprised at finding him highly interested in syphilitic experiments with chimpanzees and baboons, with his eyes upon "the beginners in prostitution," and a commendation of the application to them—that is, the beginners in prostitution—of the chimpanzee and baboon experiments, as "an advantage to them as well as for the men having intercourse with them." I need not follow this up, in order to point the immoral moral of this odious suggestion. But, apart from this directly ethical view, there is the confessed selfishness of the whole thing, and with that I practically conclude; that enters into my subject of the ethical side of it. The justification of vivisection is based upon sheer selfishness. When all is said on the score of results, and the "artist in vivisection" has done his best to convince us that he is the apostle of mercy, many grave considerations "give us pause." For instance, what law of God or Nature justifies this treatment of our poor relations? Who gave to this amazing enthusiast the right to say off-hand that he was at liberty to exploit the lower animals for experiments? What right has he to assume that these creatures are at his disposal for torture? If he proposed to kidnap or purchase cheap babies for laboratory purposes, his spectacles would not be safe in the streets; and if he put his proposal into effect, and got his babies on the dissecting table, the very costermongers would raid his laboratory, and even medical students might help them. Why? The cutting up of one baby might save the lives of thousands of other babies, and the torture of one for an hour might save thousands from suffering for years. But that would be no answer that society would accept. The killing of the cheapest baby would be murder, and its deliberate slaughter for any reason would be voted damnable. Why? Simply because of the egotism of the human animal, which has chosen to come to the conclusion that he is lord of all, with rights and sanctities inherent, which no other animal possesses. What ground is there for that? May not this conclusion of his be only the culmination of an insolent selfishness which, in action, may very easily make him the "inferior animal" after all? Not so far off, as some may imagine, this right to vivisect is related to the right of the odious vulgarian who desecrates the seashore and the beautiful cliffs for the purpose of shoot-

ing seagulls, whose bodies he is not over-anxious to possess; or that other vulgarian who thinks it sport to practice on pigeons, and other birds, preserved for the delectation of mere slaughterers. What right has this human animal to invade the right of his poor relations, and to rob them of their freedom and their life, for finery or for "sport"? Admitting that these are "inferior creatures," as well as "poor relations," what clause of any conceivable moral law hands them over to us for deliberate torture? Does not the whole thing turn, after all, upon the animal instincts which, at all the lower stages, suggest mere crass selfishness? But surely one of the notes of advance, one of the signs of civilisation, is the desire and the ability to win the higher and broader sympathies, and "let the ape and tiger die." And yet, after all, are we so absolutely sure that we know what birds and animals really are? Some of these not only astonish us with their faculties and affections, but entirely baffle and confound us before their mysterious performances. Is the carrier pigeon an inferior creature? Is the skylark inferior to the callous brute who would shoot it, or even eat it? Is the shepherd dog an inferior creature to the shepherd, who may get drunk every fortnight on bad whisky or raw rum? One need not be over fond of birds and "the lower animals" in order to be almost awed, at times, before their courage, their skill, their patience, their sensitiveness and good taste, their endurance and their love. There is a mystery here that we have not yet fathomed; and, before we talk so freely of "the lower animals," it might be well for us to consider whether we are in a position to judge as to what our relative places in Nature really are. Man's arrogance and masterfulness, always selfish and usually cruel, want repressing, not encouraging; and, in that repressing, we must be willing to pay the price, and to forego knowledge gained through the cowardly exploiting of weaker creatures. We must bid for the ideal man; and for this reason, that

"When the Ideal Man shall reign
In this world of tears and pain,
God and man in him shall meet—
Heaven his every sense will greet.

He will be a vital part
Of Great Nature's Mother-Heart.
All that live, and all that feel,
Will to his pure heart appeal.

His great sympathy shall flow
Through all forms of life below;
Flowers and birds will talk to him;
And the stars, like Seraphim,
Through their Heaven-revealing eyes,
Utter speech of Paradise.

He shall be great Nature's heir,
Lord of all things everywhere—
Like a benediction dwell,
Doing wisely all things well."

8330. Are you acquainted with the law of this country at the present time with regard to experimentation on animals?—I have read a very great deal about it on both sides.

8331. I beg your pardon, there cannot be two sides to the law?—I mean to say the law as interpreted by both sides, for there is a doubt, surely, about the interpretation of the law.

8332. I meant the rules and regulations laid down by the law with regard to experimentation on animals—the Act of Parliament—are you acquainted with that?—I think that, on the whole, I know it. I will not say that I am a legal expert, but I have naturally studied the subject.

8333. I only gathered from your statement that you were not acquainted with it, but I was mistaken, no doubt, in that. Would you tell the Commission can anyone perform these tortures upon animals?—No, I know that quite well. I am perfectly familiar with the fact that there are regulations, and that certain persons are authorised to perform them under certain conditions, but, surely, it is always an open question whether those conditions are complied with.

8334. Do you know what the conditions are that the law requires?—I cannot say that I could recite them; I know them in a general way.

8335. Would you tell me the general effect of them?—I should say in general, for instance, that, broadly

speaking, the law requires that the animal to be experimented upon shall be made unconscious. That is what the law, I suppose, requires. I doubt whether it is done.

8336. Does it require anything further?—Will you ask me on any specific point?

8337. With regard to the unconsciousness of the animal?—I have said that the law requires, I suppose—I am not sure in all cases—but I suppose in general the law requires that the animal shall be made unconscious.

8338. What else does the law require?—From my reading of cases I should extremely doubt whether that requirement is always complied with.

8339. From your reading of cases you doubt whether that is complied with. I dare say you will be asked some questions upon it; but what further does the law require besides the animal being rendered unconscious at the time when it is operated upon. Were you not aware that it requires that the animal shall be killed before it recovers consciousness?—I am not aware that in all cases the law requires the animal to be killed: if so, I think that would rather strengthen some parts of my statement. I think it strengthens it in two ways: it strengthens it, in the first place, by the suggestion that the animal is terribly injured beyond hope of recovery; and it strengthens it in this direction, that the creature is entirely deprived of its existence. I do not say that that is a deplorable thing, but I only say that it enters into the case if the animal is rendered worthless and has to be destroyed, through being put under most terrible conditions of distress.

8340. But it is unconscious the whole time?—I doubt it.

8341. What are your reasons for doubting it?—I have read thousands and thousands of pages with regard to that, and I have come in general to that conclusion. I am not here to enter into that side of it. I am here to strongly speak on the ethical side, but I should have been prepared to enter largely into that side, for I have written upon it, and I have extreme doubts whether in very many cases the creature is rendered unconscious.

8342. You also say that the example set by highly-placed and conscientious men may be followed by men not so trustworthy and not inspired by such beneficent intentions. Do you know the terms of the law for obtaining these licences?—I know that the law is often evaded in all kinds of ways. We are living in the midst of law-breakers.

8343. Do you know how a licence for performing an experiment on an animal is obtained?—I do not know the details offhand.

8344. Were you aware at the time you wrote that statement that there is very elaborate machinery for guarding against a licence falling into improper hands?—I suppose it would be so.

8345. I ask you the question: Were you aware that there is existing a very elaborate set of regulations for preventing a licence falling into the hands of an improper person?—I have also a strong impression, from my reading, that it is made very much easier than your question suggests.

8346. Would you be kind enough to furnish us with what your reading has been which has given you that impression?—I shall be extremely glad to be privileged to attend this Commission again to do that. I propose to-day to speak only on the ethical side of the question.

8347. I think if you will merely furnish us with the names of the works which you have read, that will be sufficient. I suppose I should not be wrong in imagining that you would give us the pamphlets of the various anti-vivisection societies as being your authority?—With all submission, I think that you are going beyond your right in saying that. But I only profess to come here to speak on the ethical side of the question.

8348. You see, if I may say so, that we wish only to know on what grounds you are justified in coming here to represent the ethical side. You have not taken up the position that all experimentation upon animals and that all deprivation of animal life by man for any purposes whatever is immoral?—No, I do not take that line.

8349. Therefore we wish to know on what grounds you have formed the opinion which you have expressed?—I profess my statement, and I am perfectly

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prepared to be examined upon the ethical argument which I have put forward.

8350. But in your *précis* you have made certain statements, and we are, I think, entitled to ask you on what grounds you have come to those conclusions?—I set forth all those grounds in my statement, the moral grounds and the ethical grounds.

8351. But you must have had some grounds; I want to know whether you were really acquainted with the law as it is settled, and you have answered that you do not know its details?—I only know in general that the law is as I state it—that it is understood that certain men have to prove so far as they can that they are right persons to perform such experiments.

8352. What is your ground for the statement that the medical profession is not united (of course, it is quite correct to say that it is not unanimous) as to the necessity for or the value of vivisection?—I think that has come within my own observation—that many medical men of standing do not believe it is of any use, but that it is misleading.

8353. That is another question?—That is the question.

8353A. Do you think that the bulk of the medical profession hold that view?—No; I should gather from my observation and reading that the majority of the medical profession believe in vivisection; but, as in the case of vaccination, opinion is divided.

8354. Then you laid great weight in the next statement that you made before us upon the point that observations made during artificially produced agony or distress may mislead?—Yes.

8355. Do you think that those persons who have studied physiology for many years do not know how to make allowances for those misleadings?—I can only say that I hope they do.

8356. They probably take that into consideration, do they not, in performing experiments?—To tell you the truth, I do not know what they take into consideration.

8357. But surely you would give them the credit for taking into consideration such very obvious things as that?—I do not know what the soldier in the heat of battle takes into consideration. He loves his profession, and he glories in the slaughter. Was it not Colonel Burnaby who was refused permission to go out with Kitchener to the Soudan, and in spite of the refusal he secretly went, and simply revelled in the fight?

8358. I do not think that comes into it?—I do not know. I think it is an answer.

8359. What do you mean by that; do you think that these experimenters revel in a number of experiments?—I should certainly revel in them myself if I were a vivisector. Every man loves his profession—loves the details of it.

8360. But there are not professional vivisectors, if I may say so. What do you mean by a professional vivisector?—I mean a man who regularly vivisects—a man who does it. I do not mean, and I do not believe, that it is a man's only business.

8361. What object do you think these people have in vivisection?—My dear Sir, I can imagine myself the delight there must be to the anatomist, for instance, in the dissection of the body. If I could do it, and loved it, I should delight in it for its own sake.

8362. I fail to follow you, but I will not ask you anything further on it?—It seems so obvious to me that a man who is a chemist must love his profession; it does not matter whether the man is making money or fame through it, the chemist must love his profession, and all the stinks of it. He must take delight in it. A great chemist in the north said to me, "People come here disgusted with the smell of this place; I love them all."

8363. Your own belief then is, that these vivisectors vivisect for their own amusement because they are so fond of doing it, and not for any purpose?—I would not say that. What I would say is that a man who believes in vivisection and believes that there are discoveries to be made which may be useful—I would admit all that honestly—thinks it is a useful thing, and may think he is doing God's service, even.

8364. Do you think people vivisect without a special object?—Yes, most assuredly; I should myself if I were a vivisector. I should experiment for the love of it.

8365. But you have no other ground except your own feeling for believing that?—I do not know; I think it is human nature. I think there are thousands of chemists to-day, and electricians, who are experimenting and worrying themselves, and working as hard as possible without any object except the chance of finding out something. I know it; I know many experimenters.

8366. And that is what you believe is the case with physiologists, too?—With many of them; I should think with most of them. I was going to say I hope with all of them. A man ought to love his profession.

8367. That is to say, that they put an animal under chloroform, and cut it up without knowing what they wish to observe?—No, I should not say that. The chemist knows what he wants to observe, the electrician knows what he wants to observe, but they do not know what is coming of their researches.

8368. Could progress ever take place in knowledge of any sort or kind if you knew beforehand what would come?—But then you do not know; the greatest discoverer does not know what is about to come. The man who discovered radium did not know what was coming. He was a seeker for years and years before he got the result, and the result came unexpectedly.

8369. You made a special reference, I think, to what you called Dr. Abernethy's verdict? What was that?—I quoted it.

8370. I am afraid I was unable to follow it; I was not acquainted with it myself?—Do you wish me to read it again?

8371. If you please?—"Vivisection has the direct effect of deteriorating the moral sense; and once the moral sense is destroyed it is impossible to foresee the consequences."

8372. Would you give me the reference to Mr. Abernethy—he was Mr. Abernethy. Was it in his lectures?—I will do my very best to find the reference. I have not got it here. I did not know that you required the book and the page.

8373. But you see you offer that as an important fact, and we want to know in what connection Mr. Abernethy said it. Do you know when Mr. Abernethy wrote that?—I was not with him when he wrote it.

8374. Do you know how long ago it was?—I shall be very glad to look it up and let you have the reference if you wish it.

8375. Did he write it before or after the time when anaesthetics were used in experimentation?—I cannot say.

8376. Then perhaps you would kindly let us know the reference?—I will do my best to find the reference.

8377. Do you consider that man has a right to use the lower animals for his convenience, and for his food?—For his convenience almost assuredly; for his food I have doubts.

8378. Are you a vegetarian?—No, I am not. I have very serious æsthetic doubts about it.

8379. But you are quite clear that man has no right to use the lower animals for the advancement of knowledge or the benefit of science, and indirectly, of medicine?—No, I have not said that. I think that man has no business to use the lower animals in the way of vivisection for that purpose.

8380. So that you would go for the total abolition of vivisection?—Certainly.

8381. Do you think that there is a great difference between seeing operations done on animals and seeing them done on man?—I do not know; I cannot say. I have never had any reason for observing. I have not seen them myself.

8382. But you are aware that in the medical profession, when men are learning their profession, they witness operations upon man?—I should think that in all these cases they are distinctly for doing good to the subject. That is not the case in vivisection.

8383. You stated also that you thought that there was a spirit arising which would lead to experimentation upon man in hospitals?—I should think that is very natural.

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8384. But is not almost every case in hospital an experiment?—I do not think so; not for the purpose, surely, of finding out knowledge independent of the patient. I should hope that in every case whatever was done was for the patient's good.

8385. But is not almost every case watched in order that something perhaps may be learned from it?—That is very dangerous.

8386. (*Colonel Lockwood.*) You are entirely ethical, and you know nothing of your own knowledge as to experiments on living animals?—That is so.

8387. Or as to how they are conducted, whether under anæsthetics or without anæsthetics?—As to my own personal knowledge I certainly do not know. I should imagine that very few people are acquainted with that. One only gathers from one's earnest, honest reading.

8388. All your knowledge of experiments is second-hand?—It must be.

8389. And we are to take your evidence for what it is worth on the purely ethical question?—That is so; that is what I said.

8390. Can you suggest to this Commission any better regulations for experimentation on living animals?—No, I cannot.

8391. You probably would not like to suggest any, because you are against the practice altogether?—That is so. I cannot suggest any improvement of the regulations.

8392. That is all I wanted to know; you have no suggestions to make?—None whatever with regard to regulations. Simply on the ethical ground I am against the whole thing.

8393. (*Sir William Collins.*) You state in your *précis* that the practice of vivisection is ethically bad, and I understand you to say that it ought to be prohibited by law. Is that so?—I should think that it ought to be prohibited by law. I suppose it must be if it is to be prevented. What I should wish would be that men would not want to do it.

8394. But we are a Commission charged to advise His Majesty as to what alterations, if any, should be made in the existing law, and I wanted to know what suggestion you make in regard to that?—My position is that of an abolitionist.

8395. Is that because vivisection is ethically bad?—Not only on that ground; because I did venture in my statement to include a doubt as to its efficiency, its utility, and the possibility of its being misleading.

8396. If it were found to be useful and yet ethically bad, should you advise its prohibition by law?—I think I should, on the ground that I do not quite think we have a right to exploit these mysterious creatures, very mysterious creatures in my judgment.

8397. Is it because you think that everything that is ethically bad should be prohibited by law?—Oh, well, that is a very broad question. I want as little law as possible. I want men to want to do right.

8398. I can understand your being opposed to vivisection on ethical grounds yourself and your desire that other people should think with you; but I wanted to know whether you think that because it is ethically bad and perhaps useless as well, therefore it ought to be prohibited by law?—I think that vivisection should be prohibited by law. I should have to consider a good deal as to all kinds of ethically bad things. For instance, lying is ethically bad; I do not know whether you should prohibit it by law; it is such a big question.

8399. May I ask why you select this one of many ethically bad things for prohibition by law, while others would remain permissible?—That is a question which I have answered in my statement. It is ethically bad on various public grounds. For instance, take the case of a doctor. I have a reverence for the medical profession; I am not against doctors, I reverence them; every year I preach a sermon in favour of doctors, but I should be very sorry to have attending me, if I were ill, a doctor who was bluntly addicted to vivisection.

8400. I want to follow out how it would be practically possible, even if it were desirable, to prohibit vivisection by law?—I suppose you could make law that no one should practise vivisection, and yet many people would do it on the sly. I doubt whether you could actually do away with it by law, but I would like it abolished by law. I should doubt the efficiency of such a law, just as I doubt the efficiency of the law as it stands.

8401. Do you think that a more stringent prohibitive law than that which at present exists would be liable to be evaded?—I think so. I believe that the provisions of the law are evaded now.

8402. Then, is there any practicable suggestion which you think we could make to His Majesty as to the alterations in the law, if we desired to restrict vivisection further than the restriction which exists at the present time?—I do not know of any.

8403. Then, is it rather by propagating your views and educating opinion that you would attain your end, than by penal legislation?—I am afraid that the other method would take too long.

8404. What is the other method?—The other method is what you suggest, appealing to the law.

8405. That, I understand, you thought would be an impracticable method?—Yes, I do. I have said already that I do not suggest it.

8406. Then, there is no practical suggestion that you are able to make?—Except abolition.

8407. I understood you to say that that was not practicable, because it would be evaded?—There is always the chance that you may catch your criminal.

8408. I thought you admitted a large liability to his escaping?—I think that now, while you do permit it, you do get a certain number of people who practise vivisection under legal conditions, and they are not criminals and therefore they go on with it; but there are criminals—all laws are evaded; and I do not think it follows at all that you must not legislate, or that you should fear to legislate, because the law may be evaded. The law now is justifying vivisection; I want the law not to justify vivisection.

8409. Do you think that it raises public respect for the law if you pass laws which it is well known will be evaded?—I would like the sinner to be a criminal. I put it roughly, but you are putting it roughly.

8410. I have not yet got from you a valuable suggestion as to what modification, if any, is desirable in the law?—I have said over and over again that I do not propose any modification of the law. I want the law abolished.

8411. But you limit yourself to ethical grounds, and you do not wish to give evidence upon the scientific, or so-called scientific grounds?—I have said so. I am not a scientist.

8412. (*Sir John McFadyean.*) You desire to give evidence on the ethical side of the question, and I notice that you rather resent being asked any questions bearing on the actual practice of vivisection?—I do not resent it.

8413. But I am going to put a question to you in this way: Are not your views as to the morality of vivisection based on certain conceptions which you now have as to what the word actually covers in practice? I mean that vivisection calls up to your mind certain practices, does it not?—Yes.

8414. And it is on that that your views as to the morality of it are based?—Not upon actually what is going on.

8415. But surely your views as to the morality of vivisection must depend partly on what vivisection actually is?—What it is in itself.

8416. Then it is permissible for me to put to you a few questions to ascertain whether your views as to what vivisection as practised in this country really means are wrong?—No, that is not it.

8417. Perhaps the sentence was too long?—No, it is not that; it is vivisection *per se*.

8418. But there is no such thing, if you will allow me to say so, as vivisection *per se*. When you speak about vivisection before this Commission you must mean what is practised under that name in this country, must you not?—I do not know that that is so.

8419. Do you, or do you not, mean that?—No. I do not confine myself to what is vivisection as practised to-day. If there was no vivisection going on I should be opposed to vivisection *per se*.

8420. But vivisection is not an abstract thing. Your views as to the morality of it must depend upon what the nature of vivisection is?—Oh, no; you might just as well ask me about murder. I disapprove of murder *per se*. I am not to be asked whether I disapprove of murder as carried on in this country.

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8421. Will you forgive me for saying that murder stands in no need of definition; but vivisection may. I will lead up to the point in this way. I notice that whenever you refer to the pain of vivisection you use expressions which indicate that in your opinion vivisectional operations in this country involve great pain. For instance, I noticed that you use the words "horribly tortured," "revelling in this disgusting work," "the artificially-produced agony in a dog," and other such expressions. I submit that it is perfectly fair for me to ask you what information within your own possession has led you to conclude that any experimenter in this country is in the habit of torturing a dog, or artificially producing agony in a dog?—If I had thought that I should have been asked for references to justify me in those statements, I should have brought my pile of books and publications, from which I could have quoted.

8422. I am afraid that you did not realise your responsibility. You have really made a very serious charge against licensed vivisectioners in this country, and I submit that you ought not to have lightly done that without being ready to produce evidence of what would be an offence against the law, namely, for any licensed vivisectioner to artificially put a dog in agony, or to carry out any experiment which could correctly be described as "horribly torturing" that dog. I want to know, do you hold to these expressions as being correct?—Certainly; from my reading and conversations I feel justified in making the statements.

8423. Then I must ask you, for the information of the Commission, to extract from this huge mass of material some half dozen cases in which you think that the author's account of the experiments justifies their being described as "horrible torture"?—I have not restricted myself to the practice of vivisection in this country. I have written and spoken about vivisection as practised anywhere.

8424. I will put it in this way: This Commission has only got to deal with vivisection as practised within the limits of the three kingdoms. Supposing the Commission, from the evidence put before them, arrive at the conclusion that your estimate as to what vivisection is is totally wrong, that is to say, that it does not involve the horrible torturing of animals, would you admit that the Commission ought to discount your evidence as to the morality of vivisection?—I do not think so.

8425. I am quite satisfied with the answer. Did I rightly understand you to mean that man is never justified in inflicting pain upon an animal, except for that individual animal's good?—I did not say that.

8426. Will you tell me what does justify man in inflicting pain upon an animal?—If I answered broadly Yes to that question, you might say that a man must not give his horse a taste of the whip. It is a big broad question. You may do all sorts of things. You may whip a dog that you love passionately?—I do not love any dog at all, but I have seen women who do give a dog a lash with the whip.

8427. Are these words not to be found in the evidence which you read to us a few minutes ago?—I do not think so.

8428. I submit that the words "Causing pain except for the animal's good" came in in some connection, and I wondered in what connection it was. You objected to vivisectional experiments because the pain was not inflicted for the animal's good, did you not?—Yes. But taking vivisection as it, there is a difference between vivisection and a lash of a whip.

8429. Taking vivisection as you conceive it to be, if you will forgive me, I think is a safer expression?—As it is in the world at large.

8430. You do admit that man is justified in certain circumstances in inflicting pain on the lower animals?—It is a broad question to which I could not say yes or no.

8431. I will also take that for an answer?—I would like to get at the bottom of it, and I think in five minutes we might if we talked it out. It is such a broad question. It is a question which seems to seek for an admission which will be turned to my disadvantage.

8432. There is no question of disadvantage really. We are seeking here to arrive at the truth?—It is a criticism of what I said in my statement, and you are putting a question as broad as that—"inflicting

pain on animals." There are grades of pain from the lash of a tiny whip to cutting up an animal alive.

8433. I will help you out with it. Is man justified in inflicting on the lower animals considerable pain for any other purpose than the individual animal's own benefit?—Any vivisectional pain?

8434. Let us put aside the way in which it is inflicted. Is man in any circumstances other than the individual animal's benefit justified in causing considerable pain?—There must be degrees when you speak of inflicting pain. I will give you a case. I do not love my dog; my wife does. I have seen her give it three strokes with a whip—gentle strokes, fairly gentle—and I have thought: Well, on the whole, it is rather a sharp thing to do. But if she were to kick that dog mercilessly, I should say it was base. It is a question of degree.

8435. I will put the question to you in this way: Do you think that man is justified, for the mere purpose of improving the quality of his animal food, in subjecting male animals to the severe vivisectional operation which is known as castration. You know what the operation of castration is as applied to an animal?—Yes, but is that for the animal's good?

8436. Although you have never seen it done, with the imagination that you possess you must be able to figure to yourself how exceedingly painful an operation it is when done without anaesthetics. Do you think it is justifiable?—Did you say for the animal's good?

8437. I did not say that?—I thought you did.

8438. I said that it was done for man's good—to improve the quality of his food?—I should not like the man who did it.

8439. It is not a question of the man who did it. Will you answer the question?—But there might be hundreds of questions of that kind, and I should give that sort of answer to them all.

8440. But I do not know what the answer is. Do you think it is justifiable?—No.

8441. I want to see whether you are perfectly consistent. Do you invariably ask for bull beef when you send to the butchers' for beef?—Never. I did not know that there was any difference.

8442. You will find the difference if you ask for it once and get it. Do you not think that it is very inconsistent in you, merely for good digestion or taste?—But I do not.

8443. But you spoke elsewhere of foregoing advantages derived from vivisection; why should you not set your face against this cruelty?—I should, decidedly. If a butcher told me, Here is one bit of meat got through this operation, and here is another, I should say, I will have neither of them.

8444. That is hardly going far enough. Ought you not to ask for bull beef, for instance?—I did not know there was such a distinction. I am not such a connoisseur, such a *gourmand*, in the matter.

8445. That is because you have always eaten the good sort, which is got by a painful method?—No, because I shrink from eating meat at all. I take no interest in the matter. I am almost persuaded to be a Christian in regard to that.

8446. But you are not quite consistent?—I admit it; I am not.

8447. (Mr. Ram.) Do you deny that any useful knowledge has been, or can be, obtained by vivisection, or that in any case would the acquisition of such knowledge be lawful?—I do not deny that knowledge can be obtained.

8448. Do you, then, deny that in any case it is lawful to attempt to get such useful knowledge?—Through vivisection it is not lawful.

8449. Do you base your objection to vivisection wholly upon the pain inflicted upon the animal?—I have never seen vivisection done, but I should imagine that there are various sorts of distress, not merely pain; but distress would be pain, too. I do not know what happens to an animal under vivisection, except that there are all the signs of misery, apparently.

8450. All the signs of misery, apparently?—Yes.

8451. How do you know that?—In all the experiments I have heard of—not on one side, as was suggested just now, from anti-vivisection societies, but I have read books and pamphlets by believers in vivisection, which indicate distress.

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8452. Accounts which indicate distress?—Distress and agony.

8453. Then may I take it that it is because of the pain in the cases of which you have read that you base your objection to vivisection?—Not entirely; that is one reason.

8454. You have told me that useful knowledge may be obtained?—No.

8455. I asked you whether you deny that knowledge may be obtained, and you said no?—If there were. I do not say that there is.

8456. Take it hypothetically. If useful knowledge can be obtained by vivisection of an animal which is subjected to no sort of pain or distress should you object to that vivisection?—I do not know that it could be.

8457. But you see you have come here to talk ethically to us, and I am trying to get really at what is your meaning?—Is it possible that you are in earnest?

8458. We have heard of many cases of it; anyhow, will you kindly imagine an animal subjected to no sort of pain or distress at all, an animal put under anaesthetics without pain, which is to die before it recovers from the anaesthetic. Is it wrong in such a case to attempt to get knowledge?—I do not know what happens under anaesthetics; I cannot say. There are degrees of the distress.

8459. I am putting to you the case of an animal that is absolutely anaesthetised, and without pain or anaesthetic before it can possibly recover any consciousness. Is it wrong to attempt to obtain useful knowledge by examination of that animal?—I do not know that it is quite fair to put a hypothetical case like that.

8460. I will take that answer, if you please. May I take it that you decline to answer that hypothetical case—is that your answer? (*After a pause.*) Is that the answer?—It would be a hypothetical answer to a hypothetical question.

8461. Let us have the hypothetical answer if you cannot give anything else. What is the hypothetical answer?—That is the answer.

8462. I will leave it there if you like. Do you think it is wrong for a surgeon to gain knowledge of anatomy through examining a dead animal?—No.

8463. Do you think it is wrong for a surgeon to kill an animal painlessly in order that he may examine it after it is dead?—I think it is.

8464. Is it wrong under any circumstances to take the life of an animal?—I do not think it is. That is my present stage. I have a doubtful opinion about it. I have said that already. With regard to killing animals for food, I am in a transition stage, as many men are.

8465. Since luncheon?—No.

8466. Was it permissible to kill the animal of which you last ate a portion?—I am in doubt about it.

8467. Take the case of the Dogs' Home. There is a miserable, lost, suffering, diseased dog at the Dogs' Home; no one claims it; it is put into the lethal chamber, and perfectly painlessly it dies. Is that right or wrong?—That is all right.

8468. Are you sure of that?—Yes.

8469. Thirty thousand dogs a year?—If you give me the case of an individual dog I will give you an answer. Your question a little while ago was so broadly hypothetical that it covered my wife's dog.

8470. There was nothing broadly hypothetical about the case of the animal that was killed to give you the last meal that you had of animal food. Was that right or wrong?—That is too broad.

8471. I come to the individual animal, the last animal that was killed in order that you might have animal food. Was that right or wrong?—I say I am in doubt about it; I am in a transition stage, as a vast number of men and women are to-day about that.

8472. You have not made up your mind what you will have for dinner to-night, then, I take it?—Very likely. Over and over again I say, "No, thank you, no meat."

8473. But still you are weak enough to take some?—I am in a doubtful frame of mind about it.

8474. Would it be the same thing with regard to fish and soup?—Yes.

8475. You are a vegetarian pure and simple?—No.

8476. Is it wrong to kill animals painlessly in order that their hides may be turned into leather?—That, again, is a doubtful question.

8477. I think I see a pair of very well soled shoes?—I know; I know we are all inconsistent.

8478. I am glad to say I am not in the least inconsistent about that?—We are all inconsistent—we are bound to be. You see some things clearly and other things you see doubtfully.

8479. You put the case of vivisectioning a baby, and you said that everybody would rise with horror against such an idea. Do you put babies and animals exactly on the same plane with regard to vivisection?—That is precisely what I did not do. I drew a broad contrast.

8480. What is the contrast?—That is what I want to know. That is the question I put in my statement. That is a question for the vivisectioners to answer.

8481. I thought it was rather a question for an ethical professor to answer?—No. It is a question that I have put to them.

8482. There is no objection to eating an animal that has been killed for food, at all events, if you have no doubts about it, is there?—But are there doubts?

8483. There is nothing wrong in eating animals as food?—That is the same question. I am in a doubtful state of mind about it.

8484. Is it doubtful whether you ought to eat a baby killed for food?—No, it is not.

8485. What makes the difference between a baby and a sheep?—That is what I want to know. I have been asking that. My statement asks that.

8486. We think that we do know?—No, I want an answer from the vivisectioner. The vivisectioner is bound to answer me that.

8487. Do you think it is wrong to drive a horse in a cab?—No.

8488. Do you think it is wrong to drive a man in a cab?—If the man liked it—.

8489. But if he did not like it, with the assistance of the whip, is that right or wrong?—I should think it very wrong.

8490. Why is it right to drive a horse and not right to drive a man?—Well, you can consult the man; you cannot consult the horse.

8491. But supposing the man says "No," and yet you drive him, is that right or wrong. Wrong you would say?—No, I would not.

8492. Not wrong to drive a man if he said "No"?—I should ask whether you are in earnest.

8493. Whether I am?—Yes.

8494. I am absolutely in earnest. I want to get at the distinction which you have quite gravely put to us, and I am quite gravely wanting to ascertain, if I can, the meaning of such phrases as you have used here. Why is it wrong to drive a man against his will and not wrong to drive a horse?—So far as I can get into the mind of a horse the horse has no opinion upon it; the man has.

8495. Is that the reason why you think it is right to drive a horse and wrong to drive a man—because the horse does not care whether it is driven or not?—No, it is not that. I do not know what the horse thinks about it; I have no means of knowing the horse's mind. I know the man's mind. But it may not be wrong to drive a man in a cab if he is foolish enough or slavish enough to permit it.

8496. Is that the only answer which you desire to give?—That is the answer.

8497. That is in your mind the distinction between so treating a man and so treating a horse?—Yes, that is the distinction.

8498. Can you give any other distinction?—That will do; that is enough.

8499. And you cannot give me any other?—I do not think that any other is needed.

8500. Have you any other that you could give me. (*After a pause.*) May I take it that you have not?—You may take it that I think that is the necessary answer; that answer is all that I care to give.

8501. I have only a little more to trouble you with.

Rev. J. P. Hoggins. Will you tell me this: have you any suggestion to make as to the recommendation which this Commission ought to make to His Majesty the King?—I have already said that I should be delighted if the Commission would recommend that vivisection should be abolished by law, that is to say, if vivisection were not recognised by law.

8502. Then that means that you desire the repeal of the present Act?—Certainly.

8503. And would you desire the enactment of any Act, and, if so, what, in its place?—It is immaterial whether you abolish the present Act or pass an Act forbidding vivisection. I should prefer that an Act should be passed forbidding it.

8504. Abolish the present Act and pass an Act forbidding it?—Yes.

8505. Do you think that would stop vivisection?—I am afraid not.

8506. Which do you think is the worse, so far as vivisection is concerned, the state of affairs in England or the state of affairs in either France or America?—That I cannot say.

8507. You have not studied it?—I have generally read about it, but I do not know truly what is going on in England.

8508. Do you know what is going on in either France or America?—I have read of it.

8509. Have you read of the horrors which take place there?—I have read very much about France.

8510. Take France, then. Have you read about the horrors that are taking place there?—Yes.

8511. Are you aware that there is no law regulating vivisection in France?—I have already said that I should like a law passed forbidding it.

8512. But are you aware that to-day there is no law in France which regulates vivisection; do you know that?—I took that for granted from what I have read.

8513. You know that there is a law in England which regulates vivisection?—Yes.

8514. Which do you think is the better condition with regard to vivisection, the state of things in France or the state of things in England?—That is not a fair question, because I have already said that I want a law passed to forbid vivisection—to make vivisection criminal.

8515. If you do not like to answer the question I will leave it. You want to make vivisection criminal?—Yes.

8516. Would that be wholly regardless of the object with which any vivisection might be performed—that you would make it criminal in every case?—Yes.

8517. Would you also make criminal the performance of any operation on any animal except for that animal's good?—Speaking generally I should.

8518. Are you aware of the farmyard operations which take place by the thousands every day on colts and young stock of all sorts?—For the animal's good?

8519. Not in the least for the animal's good: in order to make their meat taste better for man. Would you allow them or disallow them?—I should disallow them.

8520. Would you make them criminal?—Is there vivisection?

8521. Undoubtedly there is vivisection of a most exceedingly painful kind without any anaesthetic?—I think I should.

8522. You would make it criminal to perform any such experiments?—I think I should.

8523. You said that the practice of vivisection was ethically bad and hurtful to public morals, because it is based entirely upon human selfishness and the assumed right to capture, torment and destroy animals for repeated experiments that may prove nothing, or mislead, and for experiments which turn upon distress, and begin and end there. Is it because those experiments, as you express it, in your opinion, are for torment, and turn upon distress, and begin and end there, that you object to them?—That is the general reason—that is the ethical reason.

8524. And if experiments did not torment, and did not turn upon any distress, or inflict any distress at all, should you still object to them?—I do not believe it is possible.

8525. Will you imagine the case?—I cannot imagine it. I do not trust my fellow creatures enough.

8526. To do what—to imagine a case?—To keep within the limits of what you suggest. You see what I mean.

8527. No, I do not in the least.—There is the old hypothetical question again. The answer must be hypothetical. If human beings could be trusted to keep within those limits, I might say that I did not object. I think it is too dangerous.

8528. Is it because you do not trust any of your human fellow creatures?—I do not say "any."

8529. Because you do not trust some of your human fellow creatures to abstain from unnecessary pain, that you would object to vivisection altogether: is that the reason?—It is very near it.

8530. Can you put me any nearer, or do you adopt those words?—That is quite enough.

8531 (*Mr. Tomkinson.*) By the repeal of the law you mean, I presume, that the immunity which is given to the system now should be removed, and that all experimenters should be subjected to prosecution for cruelty to animals?—Yes.

8532. That would be the effect of a mere repeal of the present regulations?—I think that is so.

8533. Would you go further and have a special Act passed to make vivisection criminal?—If the mere repeal would be sufficient I would not want a law to specially make it criminal.

8534. You have been asked as to whether you think the results of a repeal of the law might be ill. Of course your object is to diminish the sum of animal suffering?—That is the main object, but it is not only that. My statement indicated other reasons—to diminish the danger to the vivisectioners, for instance.

8535. It has been put to you whether the result of abolition might not be to drive more of the system into other countries, where infinitely less mercy is shown to these animals?—That is likely.

8536. Or whether, secondly, it might drive the practice into secrecy?—That is very likely.

8537. Instead of its being practised openly in laboratories?—Is it?

8538. Not openly to the public—I mean that there is inspection—that it is under regulation; that it might have a tendency to drive it under the surface and into secrecy in private houses?—Yes.

8539. That occurs to you as a possible alternative?—Yes; but that is true of a great many things, you know; that is true of prostitution. Those questions all require a good deal of discrimination in both asking and answering. You may drive a thing under the surface and make it worse—one admits that possibility; but none the less should the State, the nation, clear itself.

8540. Of complicity?—Yes, that is what I should say.

8541. In regard to the question of exploiting animals for food, I gather that your mind is in a state of suspension?—In a great state of suspense.

8542. You said something in your statement about sport, but you only instanced such a thing as pigeon-shooting?—Yes.

8543. Does the same objection apply to all sport?—It applies to rabbit coursing and all those things. I would not apply it to horse racing, for instance.

8544. But to shooting?—Yes, I should think so. I never read, for instance, about the King's kind of shooting without a feeling of disgust.

8545. If you object to shooting, would that lead you logically to refuse to eat a pheasant or a partridge?—It makes me feel on the whole that I rather want to be a vegetarian.

8546. I see that you are turning that way; but hitherto you have not refused to eat a pheasant or a partridge?—I have eaten them.

8547. But you must be aware, as we all are, that an immense amount of cruelty is committed in even slaughter-houses?—Yes.

8548. And that pain is inflicted upon animals in those various operations which have been spoken of?—Yes.

8549. Do you draw any distinction with regard to

animals. You admit, I suppose, that it is necessary to destroy rats and animals of that sort?—Yes.

8550. Otherwise it would be a case of being eaten up by rats?—There again it is a question of degree—how you destroy your rats. There is a certain way of destroying a rat which is permissible. There is another way of destroying a rat which is vulgar and brutal.

8551. In regard to the common way of destroying or diminishing the number of rabbits, have you thought what trapping means? Have you ever seen a steel trap, which is the common mode of catching a rabbit?—No, I have not. I can quite imagine that very many ways of destroying wild animals are very brutal.

8552. They are extremely cruel. Do not you think that, logically, one ought to set one's face against that, as well as against vivisection?—I should be very sorry to catch these creatures in that cruel way.

8553. Then, as to that distinction of stray dogs, this instance has been put to the Commission: given a dog that is under sentence of euthanasia, to be put into the lethal chamber?—I rather like that.

8554. Would you protest against that same dog, which is under sentence of death, being utilised for some experiment?—I should not feel enough faith in my fellow-creatures to believe that it would be used without what I disapprove of. The lethal chamber, I suppose, is a genuine article, and there is no distress there, and there is no risk of margin for prolonged agony. In fact, I would rather like to be put in the lethal chamber myself if I were under sentence.

8555. With regard to that question which you found it so difficult to answer, I imagine that it really amounts to this: that you are sceptical as to perfect painlessness being insured, or anything approaching to perfect painlessness?—That is a very essential part of my feeling.

8556. But, even if you could be satisfied that no pain was inflicted, you would still object to the principle?—Not only to the principle; but, in my short statement I give two or three reasons why I think, apart from the misery of the animal, it should not be allowed—namely, the doubt as to the results being of value or the reverse, and the doubt as to the influence on the vivisector's ethical condition. That is a very important matter.

8557. You are aware, probably, that an enormous preponderance of operations on animals are very slight operations, of inoculation?—Yes.

8558. Operations which, in themselves, practically cause no pain?—Yes.

8559. To those you have less objection probably?—Certainly.

8560. (*Sir Mackenzie Chalmers.*) You have given evidence on the ethical side of vivisection?—Almost entirely.

8561. You know that other people take a different view from that which you take?—Certainly.

8562. And that, although, ethically, to you it is wrong, it is justifiable to other people?—That is all we can do.

8563. I want to ask you what is your test of what is ethically right or wrong—your own particular conscience?—My own conscience, my education, and my general spiritual condition.

8564. That is to say, it is merely a question of individual judgment?—Certainly.

8565. You have no outside standard to apply?—No. In regard to matters of this kind one must judge from one's own spiritual and moral condition as to what is right for others, although they may not feel it so. You must go by your own judgment and moral sense in any case.

8566. And where there is a conflict of what I may call ethics, or ethical opinion, is there not great difficulty in legislating?—You do not find it so in ordinary life. Lying is lying.

8567. But you would hardly legislate against lying?—I would not legislate against lying.

8568. You would not?—When it came to practical operation, as in lying about a bargain, or fraud in relation to a contract, you must legislate; but against lying as lying you cannot legislate.

8569. Are we talking always of the same thing. What do you understand by vivisection—any experi-

ment on animals, or only experiments producing pain?—I have freely said that there are grades of objection to vivisection; it extends from kicking a dog, which I mentioned just now, up to most cruel experiments. There are various grades of objection.

8570. By vivisection, you mean anything that would cause pain to an animal?—That is rather too broad a definition. If you added "for the animal's good" the objection does not quite go so far.

8571. Are we talking about the same thing when you object to vivisection, and other people are in favour of it, or are we covering different spheres?—I do not know at all. I should have to get into your mind.

8572. That is why I asked you how you would define vivisection?—I cannot define it except by taking the dictionary meaning of the word: "Cutting up an animal alive." Then I say that there are endless grades of objection from what is almost unimportant up to devilry. And then when you ask: "Are we talking about the same thing?" I should say that we are talking in the main about the same thing; but perhaps we may not at the same moment be talking about the same degree.

8573. Do you object, for instance, to feeding experiments. A great many experiments which are licensed under the Act are feeding experiments?—I do not know much about them. I know nothing about them, in fact.

8574. They come in under the present law, as you know?—Not under vivisection.

8575. They have to be licensed?—That is another thing altogether; but I know nothing about them at all.

8576. But still they are included in the returns of experiments?—They are very improperly called vivisection.

8577. As you are aware, there is no such term as vivisection known to the law. The existing law of England deals with experiments upon animals?—Yes.

8578. Have you ever read the Act?—I must have read it.

8579. You will find that the word vivisection is not mentioned; it is a law regulating experiments on animals. What I want to know is how far you object to all experiments on animals?—I know nothing whatever about the feeding experiments regulated by law. I did not know that they existed. I knew that there were very many experiments as to foods, that is to say, with human beings.

8580. Still if by any proposed amendment of the law you took the existing Act and made all the experiments under it unlawful, you would make feeding experiments unlawful. You would not wish to go so far as that?—I should like to know what those feeding experiments were; they might be abominably cruel. You might stuff an animal till it was suffering greatly.

8581. I will give you an example. A licensed experimenter applied the other day for leave to feed kittens with cow's milk instead of cat's milk, to test the effect of changing the mother's milk. Would you prohibit that?—Well, putting it as you do, I should say that I would like the Act entirely abolished.

8582. That is to say that everything done to an animal for the purpose of advancing knowledge you would forbid?—No, I think I would abolish the present Act. That would leave the experimenter still with his experiment on cat's or cow's milk.

8583. You would let it depend upon the general law of cruelty?—Yes, it would take its place in that way.

8584. Would that help you?—I do not know. I should hope that it would.

8585. I want to test you a little further to see how far we agree. We have had some evidence this morning to the effect that we lose in India 21,000 human lives a year from snake bite. How far in your opinion is it justifiable to experiment on animals with a new suggested cure?—With the virus?

8586. No, that the animal is to be inoculated with the virus, and then the suggested cure is to be applied to see whether it is efficacious?—I would rather go for the snakes.

8587. That is not quite an answer to my question. How far is it ethically justifiable?—Indeed that is the answer. It is very similar to the case of vaccination, and what I said about that was that I would rather go

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15 May 1907. for the insanitary conditions. I would turn people's attention away from the cure by inoculation, and turn their attention entirely in the direction in which they can prevent disease altogether. I think that is the safer and more profitable course.

8588. I think you may take it for granted that we cannot exterminate snakes in India, and that you would lose many many thousands of lives before you reduced the number of snakes. Is it ethically justifiable to try a new cure upon an animal for the purpose of saving human life?—I take that case of snakes in India, and I should say I doubt whether what you suggest is the right way.

8589. Take the case of plague, from which we lose many millions; is it justifiable to try any experiments on rats with regard to new cures or new modes of prophylaxis?—Could you confine it to rats. The trouble with me all the way through is that I do not trust the operators. I should go a great deal further in relation to being practically careless about vivisection if I could trust my fellow creatures more. I cannot; I do not believe it is possible.

8590. When you say that you cannot trust them, is it on purely general principles, or because you know anything about vivisection?—Because of what I know about human nature.

8591. Have you made inquiries in laboratories, or made inquiries from people who carry on experiments?—I do not know them. My knowledge is derived entirely through reading and conversation.

8592. Conversation with whom?—Not with anti-vivisectioners only.

8593. Conversation with people who have studied the subject?—Yes. Last Sunday I was talking to a London doctor, himself in favour of vivisection. I get information from these people as much as from others. I do not want to begin inoculation; I dread it—I suspect it.

8594. But you have never made any practical inquiry into the conduct of individual people as to whether they use full anaesthesia or not?—I have had no opportunity of doing so. I do not always know whether any of the friends that I talk with do vivisection or not. I do not know whether my doctor friend that I talked with last Sunday does. I did not ask him.

8595. You do not know whether he has held a licence, or whether he has ever been in a laboratory?—I do not know.

8596. Your distrust, then, is founded upon your general knowledge of human nature, and not on individual inquiry which you have made into the practice in England?—I could not say that. I have been for eight or ten years a rather industrious reader on the subject, reading all sorts of books, and talking with people, medical men and the like, on both sides.

8597. I understand you to say that you equally condemn killing an animal for pure sport as you would for the advancement of knowledge?—No.

8598. Which do you consider the more wrong?—There again you get on to degrees.

8599. Take the case of a good day's rabbit shooting?—I would not like to go rabbiting—that I confess.

8600. Would you prohibit it by law?—Certainly not.

8601. Much greater pain may be caused in that way?—I have no strong opinion about rabbit shooting at all. I have a very strong opinion about what goes on in London and the neighbourhood of London on Sunday afternoons—letting rabbits out of bags for dogs to capture.

8602. And whippet racing?—That is simply vulgar brutality—that is another grade.

8603. In ordinary rabbit shooting you may take it that 5 per cent. of the rabbits are not killed, only wounded?—I am very sorry.

8604. Still, you would not prohibit that by law?—No, I do not think I would.

8605. So that ethically, if I may put it so, it is less wrong to kill or maim for sport than it is for the advancement of knowledge?—No, that is not so, because you leave out there the many other considerations which make habitual vivisection dangerous and cruel.

8606. Which do not make sport dangerous and cruel. I am not talking of racing or anything of that kind;

I am talking of sport, in which the object is to kill the animal?—It depends upon what you want to kill it for.

8607. Take foxhunting?—I hate it. The only ethical reason for that is that you must destroy foxes, which would destroy more useful things, but foxhunting, as carried on, is to me most hateful.

8608. It is not the quickest way of killing a fox?—It is most hateful. I almost begin there to want to prohibit it. That is another stage. I resent it very much.

8609. I suppose you know that every horse in England that is not kept for breeding purposes is castrated?—I did not know that. I am very sorry to hear it.

8610. If you look in the streets you will not find a cab horse that is not a gelding in England?—I am sorry for it. I would rather take my chance of the horse.

8611. You know that the operation of gelding a horse is most hideously painful?—I hate it; I loathe it, now that you tell me of it.

8612. Would you prohibit it?—When you ask me whether I would prohibit these various things, what I said was, I would leave all these matters to the common law of cruelty to animals, and then judge every case on its merits.

8613. I daresay you remember that under the Act against cruelty it must be wilful cruelty. The words, you know, are "cruelly tortured"; it does not refer to the amount of pain, but to the intention of causing pain, or negligently causing pain without any useful purpose. Would that help you?—It makes it worse.

8614. Then your only practical suggestion to us is to repeal the present Act and leave the existing Law of Cruelty to Animals to take its place?—Yes, and to amend that as may be found to be necessary.

8615. (*Dr. Wilson.*) You say that you cannot trust men in general who experiment on animals?—That is so.

8616. Supposing that all the witnesses who have been experimenting on animals who have given evidence before this Commission had assured the Commission that their experiments, which it is taken for granted by the law shall be painless, have all been carried out painlessly, could you doubt the statement?—If I knew the men to be honest men and perfectly competent and sympathetic observers, I should believe what they say, and I should be very glad of it.

8617. Do you say that with assurance that you would believe them?—If I knew the men; I would believe any man whom I believed to be honest, competent, and sympathetic.

8618. (*Sir William Church.*) Might I ask you one question more arising out of what Dr. Wilson has just asked you. Why do you feel so doubtful of the honesty of experimenters upon animals?—I do not feel any more doubtful about their honesty than I do about that of other classes of human beings.

8619. Is it not the case that the experimenters who are carrying on experiments on living animals in this country are a highly educated class of men?—Yes.

8620. Men who hold honourable positions?—Yes.

8621. And whose conduct is open to observation by numbers of people daily who work in their laboratories?—Is that so? Do they not do a great deal of vivisection very much left to themselves.

8622. It must be done in a licensed laboratory?—That is all right.

8623. Which is open to their students?—Yes, but the students sympathise, as a rule, with that work.

8624. I am not asking about the students; I am only asking about the facts?—I do not know that it makes any difference that they belong to a high order of professional men and that they occupy honourable positions. We find lapses from righteousness in all grades of society.

8625. But apparently you have spoken this afternoon as if you could not trust any of these gentlemen?—Not "any." I never said any. I do not trust, in the present stage of social development, any order of men with doubtful powers. That is so with soldiering, and it is so with all kinds of occupations.

8626. Do you mean that these gentlemen deliberately break the law?—No, I do not. There are very many people who, in the pursuit of money, for instance, get themselves into entanglements.

8627. In the pursuit of money are they working in a licensed place, and probably in the presence of a number of students, or, at all events, of their assistants, who are aware of all that they do?—Yes, and

more, because they work in the City of London under the inspection and observation continually of hundreds of clients and fellow-financiers.

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8628. Have you not given evidence to-day on what you think is the case on many points without having made any inquiries as to whether it is so really?—I cannot say that; I do not admit that at all.

TWENTIETH DAY.

Wednesday, 29th May 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.

Sir W. S. CHURCH, Bart., K.C.B., M.D.

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.

Sir J. McFADYEAN, M.B.

Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.

Mr. W. H. GASKELL, M.D., F.R.S.

Mr. J. TOMKINSON, M.P.

Mr. G. WILSON, LL.D., M.B.

Captain C. BIGHAM, C.M.G. (*Secretary*).

The Rev. LIONEL SMITHETT LEWIS, M.A., called in; and Examined.

8629. (*Chairman*.) You and Miss Woodward, I believe, have been deputed by the Church Anti-Vivisection Society to present their views to this Commission?—That is so.

8630. And I see that you have sent us in a *précis* of your evidence. You speak entirely on what I may call the ethical question?—Entirely, except in so far, of course, as I give some evidence of the statements I make. I make a statement in the last paragraph, paragraph 13, that the logical outcome of experiments on animals is experiments on man, and that the right to perform such has been claimed and carried out. In making a statement like that, I should put in some facts.

8631. You deny that right?—I deny that right.

8632. That is all; it is only stating the converse view?—My evidence is on the ethical side entirely.

8633. We have had a good deal of evidence on the ethical question, and we all of us feel the importance of hearing it; but the Commission have been considering that matter, and I think it is right that I should say that they think that they have heard it at a length from some witnesses which makes it unnecessary for them to hear at the same length from others; they have given a very free hand to some witnesses as to the length of their evidence, but the points are really short and capable of being shortly put, and if I may say so, you seem to have put all the points (almost all of which I am bound to say have already been put before us very tersely and clearly in your *précis*: and what I propose to do is to take that statement of yours as being a statement of the views of the Church Anti-Vivisection Society. I do not think we ought to call upon you or invite other witnesses to enlarge upon this question, because it really comes to our hearing a certain number of speeches in favour of certain opinions. We want the opinions, we want to know how far those opinions are backed, but we are all of us, I hope, people of some common sense and experience in the world, and when we have heard these propositions stated fully two or three times, it is almost as much as our minds will absorb on the same point, and we do not gather much by repetition. That certain considerable classes of the community agree in those views, and that they should put their views quite shortly before us in support of the general principle, we are quite ready to admit, but many witnesses have given us a *précis* and have followed it by long written statements which I do not think, having regard to the time which should be taken in this Inquiry, we can continue to hear. Therefore, if

you will allow me, I will ask you, first of all, what is the Church Anti-Vivisection Society, what is the nature of the body, how many members does it contain?—The total clerical members in the last report were 375, the total lay members 496, making 871 in all.

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8634. Do they consist of all classes, or mostly of what we may call the more educated classes?—They consist of all classes, but mostly of the educated classes.

8635. Then, so far as I am personally concerned, but without prejudice to any other members of the Commission who may wish to ask you questions, I would ask you: Do these views which you have stated in the 13 paragraphs of your *précis* express fairly the views of the Church Anti-Vivisection Society?—I believe so. I sent my evidence in to them and they forwarded it; therefore I suppose that they approve of it.

8636. And I need not say that they are views which you yourself hold also?—Most strongly. My statement on the morality of vivisection is as follows: (1) Before we argue as to whether an act pays, we must first consider whether it is right. (2) Power of enjoyment shows the intention of the Creator that the possessor should be happy. (3) Power of enjoyment is the most dominant feature in man and beast. (4) Pain is intended to give warning that continuation of an act will prevent well-being and enjoyment; that is, pain is intended to preserve the *individual*, to promote his ultimate enjoyment. (5) An action involving temporary pain inflicted on another may secure to the sufferer greater physical good than would otherwise accrue. (6) An action involving temporary pain inflicted on another may secure to the sufferer greater moral good than would otherwise accrue. (7) It is immoral (because contrary to the primary intention of pain) to inflict pain unless it is honestly believed that the pain will procure, at least in part, the moral or physical good of the individual sufferer. (8) The right to take life does not imply the right to make living unendurable. (9) To control one's aversion from causing suffering, and to inflict temporary pain on another for the sake of future gain for the sufferer ennobles; to inflict pain on another unless this motive is present is cruel, and hardens and degrades the one inflicting it; to profit knowingly by pain inflicted on another where this motive is absent makes the person benefited a coward. (10) Because we see suffering all around us, to think that we may blindly inflict it is both foolish and impious. It is as if a great surgeon performed an operation for the benefit of the sufferer, and a

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mischievous ape who had witnessed it attempted to imitate his action without his knowledge. (11) The dog or cat, etc., gives up its wild nature, because it has learned to trust man; and this trust is betrayed, and by an act of treachery, when the victim is placed upon the vivisectional table. (12) I have tried to prove that vivisection by its nature is a perversion of the intention of pain (and therefore immoral), cruel, and cowardly, and that, furthermore, it is treacherous. That being so, while I might painlessly sacrifice the lives of many animals to save one valuable human one, I would not have one mouse painfully vivisected to save the greatest of human beings nor the life dearest to me. (13) The logical outcome of experiments on animals is experiments on man. The right to perform such has been claimed and carried out. A race of medical men hardened by vivisection is a public menace. The rights of animals rest on exactly the same foundation as the rights of man—justice and mercy. The temptation to a man who has hardened himself to make experiments on animals which were not for the good of the sufferer, to make experiments on human patients which are not for the good of the sufferer, must be very great. He has blunted his moral sense by inflicting pain in a way which was exactly opposite to the intention of pain. He has broken down the one safeguard which separates humanity from cruelty. Without the knowledge of God he is impiously claiming the power of God to inflict pain where he sees fit.

8637. (Sir William Church.) With respect to paragraph 12, have you any special object in choosing the mouse as an animal?—No object whatever; simply to take some very small creature which is not valued very much by most people (most people try to get rid of mice), and it occurred to my mind as an illustration.

8638. (Chairman.) To put your case stronger?—That is all.

8639. (Sir William Church.) You were not aware that there has been a great deal of experimentation of late upon mice with regard to cancer?—That did not enter into my mind when I wrote those words.

8640. You were not aware, either, that mice are very much used for diagnostic purposes?—No, I was not. That would not make any difference.

8641. As discriminating between different diseases?—No, but that would make no difference.

8642. (Colonel Lockwood.) You simply chose the mouse as the humblest representative of life?—I will not say the humblest, but as one that people do not value much.

8643. (Sir William Church.) Do you think we are authorised in killing rats and mice at all?—Certainly, when it is necessary; but, in my view, the death should be instantaneous. I think, for instance, the tooth trap, and I think that ordinary trap, the cage trap, are simply barbarous. I think the only trap that should be set is the one that breaks the back at once. As you ask me that question, I may say that I have never had a trap set in my own house, and if people would only cover up their food, which is very much more healthy for them and much more cleanly, they would not need to set mouse traps.

8644. Then, to protect your food, you think you may take proper steps to clear your premises of mice and rats?—As I have said, I think, in my *précis*, I admit the right of killing; but the right of killing does not imply the right of making living miserable. I think I can illustrate it in this way. Rightly or wrongly, at present the laws of this country think that it is permissible to hang a man for murder, and when people consider there has been a gross murder, the papers do not make a great stir about it; but if we read to-morrow morning that a prisoner who committed a murder and was condemned to death had been tortured in his cell, I do not believe there is a single paper that would not take it up; and from that I argue that the power to kill and the power to make living miserable are two very distinct things.

8645. Do you approve of the destruction of rats with the object to prevent the spread of plague?—I hate killing unless it is an absolute necessity. I see no sin, when people are absolutely convinced that plague is spread by rats, in their being killed. I do not say I believe that the evidence is sufficient; as one of the public, who form the jury in those things, I am entitled, of course, to my opinion, whether we have not got a craze for killing everything. I remem-

ber quite recently somebody wrote and suggested that all the pigeons in the City should be killed, because they deposited germs. One begins to wonder what you can do for fear of germs.

8646. But might I ask you, do you consider that we have the right to kill rats or other animals to prevent the spread, or, we think, with the object of preventing the spread, of disease in man?—I think that to kill painlessly for that object is no more sinful than to kill for food.

8647. Is it wrong to kill for food?—I think not.

8648. But you think that, although it is right to try and protect ourselves in this way, it is not right to take the life of an animal for the sake of investigating our views as to the production of disease?—I have never objected to an animal being killed for that purpose. I object to its being kept alive in suffering, which, to my mind, is a quite distinct thing.

8649. But might I ask you just to explain what you mean by being "kept alive in suffering"?—Of course, I am not here to give scientific evidence, but I felt that if I was going to give ethical evidence I must be able to give some reasonable cause for holding the opinion that vivisection inflicts pain; and, consequently, I can refer to evidence already given.

8650. I am speaking with regard to these mice. The mice are not used usually for vivisection in the way of demonstrating organs or anything of that sort; they are chiefly used, as I say, now largely in the investigation of cancer, and they are largely used as a means of differentiating disease by means of inoculation?—We have the evidence given before this Commission already, that very great pain sometimes—I can give you chapter and verse, I have it down here—results from inoculations. For instance, Mr. Thane, the inspector, at Question No. 457, says that under Certificate A, inoculations will cause, his words are, "great pain and suffering sometimes, and he gave as an instance the injection of tetanus toxin, infection with plague, and also the insertion of certain drugs; therefore, as I object to animals being kept in suffering, I most strongly object to a mouse being treated in that manner."

8651. (Sir William Collins.) In your paragraph 12 you state that while you might painlessly sacrifice the lives of many animals to save one valuable human one, you would not have one mouse painfully vivisected to save the greatest of human beings nor the life dearest to you. You appear there to draw a distinction between painful and painless vivisection?—I want to be very exact in what I say; that is why I put in that word "painfully." If you want to elicit my opinion upon that point, I think that under the present safeguards it is impossible to know what are painful vivisections and what are not; and when we consider what happens in slaughter-houses, which are so much more open than laboratories (and in every case when a man has animals within his power), what things can go on there, certainly in a closed laboratory, I think the risk is enormous of having vivisection carried on at all; but if it were possible to be sure that an animal was completely under anaesthesia, and was destroyed for certain before it recovered, then I should have no more objection than I should to killing an animal for food. But personally I may say this. I would not hand myself over to vivisectors under those circumstances, and, therefore, I would not have any animal handed over to them. I would not trust any body of men of any calling with full power behind closed doors, especially in the case of those who cannot speak for themselves and tell us afterwards what they have suffered. That is my view.

8652. My question merely was: Am I right in thinking that you draw a distinction between painful and painless vivisection?—If it was possible to get painless vivisection I should not disapprove of it, because it is the infliction of suffering to which I object. I want the total abolition of suffering.

8653. I thought your paragraph 12 implied that you considered there was such a thing as painless vivisection?—I have never said that I am satisfied that there is such a thing. I wanted to be very exact, and I wanted just to make quite clear my point, that it is the living in suffering that I object to—that we should inflict it upon them.

8654. Do you doubt that there is painless surgical operation?—Personally I have never known a person who was operated on (I believe the right word is aseptically) but what they have felt pain afterwards.

8655. But do you doubt that under anæsthetics a surgical operation may be performed of considerable magnitude, and the patient subsequently state that he has felt no pain?—Really I am here to answer ethical questions, and that is a scientific question, which I do not think it is quite fair to put to me; I did not come to give scientific evidence.

8656. (Chairman.) It is hardly a scientific question to ask you, do you think that if a boy or a grown-up person has a tooth taken out under gas or ether that is a painless operation?—If the questioner in asking me do I believe that there can be complete anæsthesia of human beings, of course I do, the same as any other reasonable person twelve years old would probably admit.

8657. (Sir William Collins.) I will not trouble you further on that. I will proceed to your paragraph 13, which deals more particularly with the moral aspect. I see you state that the logical outcome of experiments on animals is experiments on men, and the right to perform such has been claimed and carried out; that a race of medical men hardened by vivisection is a public menace; and that the rights of animals rest on exactly the same foundation as the rights of man. Do you put the rights of animals as regards either destruction or, as the term is frequently used, exploiting them, on the same level as the rights of man?—I did not say that the rights of animals are of the same magnitude as the rights of man. I said that they rested on the same foundation. Two trees of very different size may grow upon the same soil.

8658. Am I right in thinking that by that you mean that there is a difference in degree, although they may be of the same kind?—Certainly. I do not think I have the right to kill my brother man for food, but I think I have a right to kill animals for food.

8659. Then I take it that your opinion is that the rights of animals differ in degree as regards their use by man from the rights of men?—I do think so; at the same time I would just like to read this—they are the words of Jeremy Bentham on the rights of animals: "The day may come when the rest of the animal creation may acquire those rights which never would have been withholden from them but by the hand of tyranny." I am not quite sure of the next word, which was copied for me, but I could not read the person's writing. "The French"—I think it is—"have already discovered that the blackness of the skin is no reason why a human being should be abandoned without redress to the caprice of a tormentor. It may come one day to be recognised that the number of the legs" (again I am not quite sure that is the word; I think it is) "the callosity of the skin, or the termination of the os sacrum are reasons equally insufficient for abandoning a sensitive being to the same fate. . . . The question is not, can they reason, nor can they talk? but, can they suffer?" That is taken from the works of Jeremy Bentham, published under the superintendence of his executor, John Bowring, volume 1, pages 142-3, in a note. It is on the subject of An Introduction to the Principles of Morals and Legislation.

8660. That quotation would appear to contemplate a stage in the evolution of morals which we have not yet attained to, would it not?—We have not yet attained to that, but we have attained to it very much more than when that was written, I hope and think. For instance, it was practically in the Victorian era that people realised that an animal is entitled to wages for its work, is entitled to food and shelter, and if a man neglects those, the law of the land will deal with that man.

8661. On what moral principle would you differentiate between, as I understand you to say, the legitimate sacrifice of animals for food and possibly also for clothing and for convenience, and yet forbid it for the purpose of advancing knowledge or for an endeavour to improve medical treatment?—The great distinction that I draw is this. We have the right to take life when necessary, but to torture an animal, to use a strong word, or, rather, I prefer to say, to make living miserable, is a perversion of the intention of pain. It is perfectly clear to my mind that pain was given for the preservation of the individual, and I think we can see that in the case of fire. Supposing we had not felt pain from fire, we should have very often run in such danger that we should have been burnt up or seriously maimed before we knew of the danger; but the feeling of pain is a warning to the youngest child that a thing is to be avoided; and those of us who are older and have learnt that lesson, if we suddenly get near a

dangerous flame, and get warning from the heat of pain we feel, cease to do that which might have injured us. That seems to me to be the intention of pain. In my point of view every wrong is an excess or perversion of the use of a right power. I think we can see that practically every vice is an excess or perversion of a virtue. The infliction of suffering in a manner contrary to the intention of pain (the preservation of the individual) is a perversion of a power, and therefore a wrong, and nothing can make it right.

8662. I notice that you used with some hesitation the word "torture." Making another quotation from Jeremy Bentham, perhaps it may be regarded as a question-begging epithet. It implies, does it not, needless cruelty or intentional infliction of pain?—In that case, if that is your definition, I should most certainly call vivisection, in every case which inflicted the slightest pain, torture, because it is the perversion, in my opinion, of the intention of pain, which was to preserve the individual; instead of which the experiment is used to inflict pain on the individual, not even in part for its own sake, but absolutely for others. I hold that to be immoral, because I consider it to be a perversion of the intention of pain.

8663. (Sir John McFadyen.) In paragraph 7 I understand you to hold that the infliction of all pain on animals is immoral unless it is for the benefit of the individual animal?—In my copy of the *précis* I have inserted before the words "the moral" the words "at least in part." The passage runs:—"It is immoral (because contrary to the primary intention of pain) to inflict pain unless it is honestly believed that the pain will procure, at least in part, the moral or physical good of the individual sufferer."

8664. You are aware, of course, that for the purpose of improving the quality of beef and mutton and pork farmers and others who own animals habitually submit them to a very painful operation, to unsex them. Do you think that is immoral?—I have thought of that question for some time. To begin with, I have not the slightest doubt that in many cases that operation is performed when it need not be, and, knowing what human nature is, there is not the slightest doubt that it is very often cruelly performed; but, of course, a thousand wrongs do not make a right, and I think one of the great values of attacking one wrong is that the people you attack always draw strong attention to another, so that the field of attack opens out enormously. You ask me whether I think it wrong. I have to apply, of course, my principle. My principle is that you have no right to inflict suffering unless it is at least in part for the good of the being upon whom it is inflicted. The first thing I should have to decide is this: is it not in part for the good of the animal that this pain should be inflicted? So far as I know it seems to me that many animals lead a much happier life because that operation has been performed than they would have otherwise. If you can convince me that that is not the case, I should simply oppose it absolutely, because I cannot alter principles to fit facts; facts must be altered to fit principles.

8665. I suppose you do not seriously contend that the intention to benefit the horse or the ox or the pig that is unsexed by that operation has ever entered into the farmer's mind?—It may not have entered into the farmer's mind, but there is someone above the farmer, and that is the public; and there are many things which are permitted, whatever the original inflictor's intention may be, because the public can see that it is not without good to the being upon whom it is inflicted. That would be my answer.

8666. Do you discriminate between the lower animals and man in this connection?—In what way do you mean?

8667. I mean in connection with the operation of unsexing?—It should not be done, of course, except for the person's good.

8668. Do you suggest that it is for the good of either a human being or an animal to be unsexed?—In the case of animals, I am really not an authority upon a point like that. If it violates that principle which I laid down, I should strongly oppose it; but I am of opinion that a great many horses lead happy lives doing useful work, being loved and well treated, which probably would not lead such happy lives if that operation was not performed.

8669. Why should that be?—Because people, I understand, say it is impossible to use them unless that operation is performed.

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8670. I am not speaking about the using: I am coming to that presently; I am speaking about those animals which are kept for food purposes and killed for food purposes?—I do not know how far it is a question whether they could be kept at all in the country fields.

8671. Of course they could?—In that case, certainly, I think it wrong. If your facts are right, then I think the operation wrong.

8672. But supposing it is a mere matter of convenience to the owner; that is to say, that it would be more difficult to keep them in fields and so on, but not impossible, at greater expense, in your view that would not justify these operations?—Not if it causes great suffering.

8673. It is a matter beyond any dispute that many millions of animals are subjected to these painful operations in every twelve months, so that the amount of pain inflicted in this way on the brute creation must far exceed the whole amount of pain that is caused through what is called vivisection. Would it not be better to attack the greater evil first?—Everybody who supports an evil thinks somebody else's evil greater.

8674. But how do you measure the magnitude of an evil which consists in inflicting pain on animals?—You measured it; I did not. You said the greater; I did not.

8675. I did. I am asking how you measure it. You objected to my measurement; how do you measure it?—I attack all cruelty wherever I find it. I am not only an anti-vivisectionist; I attack all cruelty.

8676. Have you ever attacked this particular form of cruelty before?—It has not been brought to my mind. I have thought of it for some time—for about twelve months or so—and I have not got very much knowledge. I may tell you that I spoke to a doctor about it not long ago. I asked him my point. I said, "Is it not sometimes performed for the good of the animal itself?" "Oh, undoubtedly," he said. Like every other form of cruelty, like cruelty in a slaughterhouse and everywhere else, I abominate it. I attack every possible source of cruelty. You speak about attacking it. It is quite right; it would have to be done; we must have no squeamishness; but it is a very difficult subject to tackle in public.

8677. But that need not restrain you?—No, quite so; and every influence I can possibly bring to bear upon everybody, I would on that subject.

8678. So I understand that if your efforts in connection with the question of vivisection were successful you would probably in the next place try to bring about legislation to suppress these practices which farmers carry on?—You greatly mistake my attitude. I never wait until one evil is gone before I attack another. I do everything in my power as I see the ability to do so, to attack all evils at the same time. I prefer not to wait till one evil has gone to attack another.

8679. Of course, if you are going to do that immediately?—But I am not yet convinced that it is an evil so great as you say. I am in the state of a person who begins very gravely to suspect and to fear.

8680. But you must have misunderstood me. I did not intend to express an opinion such as you ascribe to me. I was not giving an opinion as to whether it is an evil. I do not myself believe it to be an evil. I merely wanted to find out whether you were consistent or not?—I thought you called it the greater evil.

8681. Oh, no?—I thought you asked me why I did not attack the greater evil rather than vivisection.

8682. From your point of view it was an evil; I prefer to describe it as so much pain. I said that these operations involve much more pain than all the operations conducted by vivisection?—That is a matter of opinion.

8683. You are not of that opinion?—I say that is a matter of opinion. I cannot give an opinion on that.

8684. But if I tell you that there are 10,000,000 animals on which the operation of castration or ovariectomy is performed in full consciousness every year, would you doubt it?—I say that of course I should want to know at what age it was performed; whether the animal suffers really at that age. I should be afraid it is so, and if the animal suffers, certainly it ought to be compulsorily under anæ-

thetics; and if anyone says that there are not enough anæsthetics to do so, I have not the slightest doubt that when the demand comes the supply will come also.

8685. That was not the question. It was a question of the comparison of the amount of pain that was inflicted by men. I think, in paragraph 9 of your *précis*, you indicate that in your opinion it is cowardly to share in any advantages which may be derived from inflicting pain on the lower animals when it is not intended for their own good. Are you a vegetarian?—I am not.

8686. Then you will hardly deny that you do share in an advantage which is attained by the unsexing of pigs and rams and bulls, unless you habitually ask for bull beef from your butcher?—I do not think you quite understand the words of my *précis*, "To profit knowingly by pain inflicted on another where this motive is absent makes the person benefited a coward." What I there want to bring out is this: I have in my mind such things as this: Supposing I was suffering from diphtheria, there is nothing in the world that would induce me to use or have used on me anti-toxin, because I believe the production of anti-toxin to be a sin. What I mean by being cowardly is this: I look upon vivisection in this way. Every now and then we hear with great pride, coupled with great sorrow, of some disaster, where, when every legitimate means have been taken to save the crew and passengers, when nothing more can be done, the women and children are put into boats and the men go down at their posts. I do not think anyone will say that that is done because the lives of women and children are necessarily of more value than those of men; it is because they are weaker; and therefore when everything lawful has been done the men stand at their posts bravely and go down. Now, some years ago there was a most lamentable incident which happened at a charity bazaar, where the men lost their heads and beat the hands of women and children, making them loose their hold on the balustrade so that they might save themselves.

8687. (Colonel Lockwood.) That was in Paris?—It was. That was an instance of panic. Now, vivisection is permitted, to my mind, for two reasons: partly because people fear death, and partly because they fear suffering, and, instead of being like the sailors at their posts when inevitable suffering or death comes, and they have taken all legitimate means—instead of saying "This has been sent to me, and I must bear it with fortitude as a man and as a Christian"—they turn to what I consider unlawful means in panic. That is what I mean by its being cowardly to knowingly use a thing which is the product, absolutely the product, of what is unlawful. But now let us suppose for a minute that a lawful thing is vitiated by the sins of people, what we have to try and do is to get that sin put a stop to. But we need not cease to do a lawful thing. It is lawful to eat meat; it is, therefore, our bounden duty to see that the animals are killed as painlessly as possible. And if your facts are perfectly correct it is also our bounden duty to do our best to see that these animals are operated upon in the way you mention, under anæsthetics, but until that is done, because it is the vitiating of a lawful deed, I do not think we are compelled not to eat meat at all when it is part of our nature to eat meat.

8688. (Sir John McFadyen.) I am afraid really you misunderstand my question, which is: Do you consider it your bounden duty to abstain from eating the meat of these castrated animals, because it is undoubtedly better meat, which sells, I suppose, 25 per cent. dearer at the butcher's shop; it is an advantage, that is to say, in purchasing meat for one's diet, not merely from a nutritive point of view, but from the point of taste. Do you consider it your bounden duty to abstain from it?—If the meat itself was absolutely the product, then I should consider it my duty; but there will be meat whether that cruelty, if it be cruelty, is performed or not, and I consider that that makes a very great difference. I say, there is meat. I have animal teeth given me to eat that meat, and I am not going to abstain from eating that meat because somebody else has prepared it in a wrong way. What I will do is to try and get that wrongful preparation of that meat stopped. That is my view.

8689. Are you entirely opposed to field sports in consequence of the pain they inflict upon the lower animals?—No, but I think to take a pleasure in them is simply horrible. I think animals must be killed,

but how anybody can take pleasure in going and killing anything is simply past my understanding.

8690. I was not concerned with that at the moment. I wanted to know whether you think field sports which involve the infliction of pain on animals obviously not intended for their own good, are moral or immoral?—The animals have to be killed.

8691. No, not necessarily; they are bred on purpose to be killed many of them. Will you deal with those cases?—Take, for instance, foxes. It is plain that foxes would have been exterminated had it not been for hunting, therefore, as the fox enjoys its life, hunting has been a benefit to it.

8692. (*Chairman.*) Take pheasant shooting?—In the old-fashioned way, when a man went out with his dog to shoot—

8693. No, take the present time?—Do you mean, do I approve of battues?

8694. I mean pheasant shooting as carried on at the present time?—Personally nothing would induce me to take part in it, any more than I would go to a butcher's shop and slaughter a beast for my food. I know many sportsmen who are the kindest hearted men, they have certain rules and regulations, and they have a keen sense of fair play, but personally the way in which I look at it is this: There are many offices of life that have to be performed. I do not intend to perform them as long as somebody else can be got to do it. For instance, sewers have to be emptied. Not one of us at this table, I suppose, would wish to go into them. I venture to say, without speaking in the slightest degree contemptuously of sportsmen, that personally I look upon sport as one of those occupations I myself cannot understand the pleasure of.

8695. (*Sir John McFadyean.*) What is your defence of the pain inflicted by breeding pheasants and shooting them afterwards?—I do not understand the pain inflicted in breeding them.

8696. I did not say so. I said, in breeding them and shooting them afterwards. I mean that they are expressly bred in order to furnish sport with the gun. Is it your view that it is necessary to have that carried out?—I doubt the statement, to start with, that they are only bred for that. I think that many people like to see them about the place, because they are extremely beautiful.

8697. (*Chairman.*) You might leave out that small percentage and deal with the 95 per cent. where they do it for sport?—May I have the question once again?

8698. (*Sir John McFadyean.*) I understand you to class the shooting of pheasants and the infliction of pain which it involves, with such things as emptying sewers and slaughtering cattle in slaughter houses—that is to say, things necessary, but things you would not like to do yourself. I say, is that so; do you classify it with those things and justify it on the ground that it is necessary?—Never mind motives for breeding pheasants; as long as pheasants are in the world, and are an article of food, it is no more cruel to kill a pheasant by shooting it than it is to kill cattle in an abattoir. Personally, if you gave me my chance as a fox or a pheasant, or as an ox to be killed in a slaughterhouse, I would choose to be a fox or a pheasant.

8699. Is the use of the whip and the spur on horses justified under your code of ethics?—Certainly, because to be unrestrained, and to have one's way completely is bad for everybody. If you look at my *précis*, paragraph 6, I say: "An action involving temporary pain inflicted on another may secure to the sufferer greater moral good than would otherwise accrue." That will cover all punishment.

8700. I am not speaking about the use of the whip and spur as punishment, but as a goad to make the animal go quicker; that is the ordinary use of the whip and spur. Is that represented as being for the animal's good?—A man who uses his whip on an animal that is doing its best is a man who is not inflicting punishment; that is not punishment. If he uses his whip to make the animal go quicker it means that he thinks it can do more; he either does it by simply moving his whip, the mere movement is enough if the animal is willing; but if the animal is lazy, and needs a little punishment, that is punishment.

8701. I ask you to consider the case where an animal is not lazy, and is going at eight miles an hour, and the driver desires him to go at ten miles an hour. That is hardly for the animal's good. I want to know,

is it justified?—My answer is simply that if the horse is going as well as it can the man has no right to inflict punishment. If the man wants it to do better, if it is a willing horse he does not have to hurt it with the whip, but if it is an unwilling horse then a little punishment is needed, and if he gives it properly there is no cruelty.

8702. But there is no horse willing to go its utmost speed?—I do not know about that.

8703. Suppose a horse going eight miles an hour and the owner, for his own convenience, wants to make it go twelve miles an hour, is he justified in using the whip and spur?—I am astonished at your statement that there is no horse willing to go at its utmost speed. There are some horses you can hardly hold in.

8704. I admit the exception—1 per cent., perhaps. Please deal with the average?—I think the proper punishment of animals is lawful.

8705. It is not punishment at all. I ask you to assume the case of a horse that will go without punishment at the rate of eight miles an hour, and the owner, for his convenience, desires it to go at the rate of twelve miles an hour. Is it immoral for him to use the whip and spur?—If going ten or twelve miles an hour will inflict injury upon that horse he is wrong, but it is no more wrong to induce the horse by moving the whip, or a painless flick of the whip, to go faster than to encourage it by word of mouth. A willing horse will go by mere sound.

8706. But always assuming that he has used the whip in such a way as to inflict pain, which is quite a common thing, or that he has used the spur in such a way as to inflict pain—very little pain, I admit, but still much more than is inflicted by some vivisection operations—is that immoral?—The horse is a servant; it is paid wages to do certain work, and if work is not demanded from that horse which it is unable to do there is no cruelty in inducing it to do it. If your means of communicating with that horse which that horse best understands is your whip, you are simply making a justifiable demand upon it; you are not asking it to do too much. There are many horses that you do not have to touch with the whip.

8707. In this case you draw a distinction between man's duty to his fellow-man and man's duty to his fellow-creatures, because you would hardly justify the use of a whip on a paid servant who is not working to the satisfaction of his employer?—Perhaps you will forgive me for pointing out that there are different means of communication. There are three forms of speech: There is articulate speech, there is tone, and there is gesture, and with the animals it is tone and gesture that generally convey our meaning to them, and therefore just as we have practically by tone and gesture to convey it to them, so it may be necessary to convey it by the whip. And, after all said and done, I expect most of us round this table, when we were boys, felt the whip when we were lazy. I do not know that there is any cruelty in that. I should put animals in the same category as children.

8708. (*Chairman.*) We are getting a long way, I think, in these refinements about cruelty—about how far we are entitled to whip a horse or not. You called it punishment just now; but I did not know that the horse had a moral duty, for the breach of which it ought to be punished, from your point of view.

8709. (*Sir Mackenzie Chalmers.*) Have you a list of your Council. Would you hand us in a list?—Here is the sixteenth report (*handing in the same*).

8710. Are you a member of the Council?—No, I am an ordinary member of the Society.

8711. But you were chosen to represent it?—Yes.

8712. Are you a beneficed clergyman?—No; for the last seven years I have been a curate in the East-end of London.

8713. As you know, the Commission is appointed to investigate the operation of the Act of 1876, and to see whether any or what alterations are required. What does your Society suggest in the way of altering the existing law. That is the point we have to consider?—I am here to give moral evidence only. It would be better to ask other members as to that.

8714. But in consequence of the moral evidence that you give do you suggest any legislative changes?—That is not in my *précis*. I was told I was to keep myself absolutely to moral evidence. I prefer to do so.

8715. Somebody else will answer that?—I may just

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say for a minute that the Society itself is pledged to try and obtain the total abolition of vivisection.

8716. That is what I want to know?—I thought you wanted a technical point.

8717. No. You want to abolish vivisection altogether?—That is the aim of the Society.

8718. And by vivisection what do you mean, because the word "vivisection" does not appear in the Act. Do you mean only experiments causing pain, or all experiments on animals?—Personally, I have explained that I want the total abolition of pain.

8719. For instance, feeding experiments and any experiments which might be useful but do not cause real pain you do not object to?—It is very difficult (I am talking for myself, not for the Society) to draw the line apparently, and some of the questions asked in Parliament and correspondence with the Home Office arising therefrom have shown the extreme difficulty there seems to have been in drawing a distinction between some experiments inflicting pain and some not, and, personally, the very thing which a vivisector has called the mere prick of a needle is one of the things I am most opposed to—namely, inoculations. I think the suffering is very great after them. I have already read one statement which I brought here among others.

8720. They may or may not be followed by great pain?—Sir James Russell, the Assistant-Inspector, at Question 543, before this Commission, spoke about a cat, for instance, which suffered "severe pain" after the insertion of Daboya venom. He spoke about pain from drugs. He said that some rabbits suffered "much pain," and then he spoke of some rats as "miserable and dying evidently in suffering."

8721. I asked those questions for this reason: Rule 2 bases the League's action upon the conviction that morality and religion have the right to impose and maintain limits upon research and experiments in human nature. I did not know whether you meant that there ought to be restrictive legislation, as there is at present, or whether you would carry that restriction much further and abolish experiments on animals for the purpose of research?—The object of the Church Anti-Vivisection League is total abolition. I do not believe there is any anti-vivisectionist living who would object to an animal being experimented on and destroyed without any suffering.

8722. If he was sure that anaesthesia was complete?—Yes. But many antivivisectionists say what Lord Shaftesbury said—I think I have Lord Shaftesbury's words here, and I should like to quote them; he said, "We are bound in duty, I think, to leap over all limitations and go in for the total abolition of this vile and cruel form of idolatry, for idolatry it is, and like all idolatry, brutal, degrading and deceptive." That is an extract from a letter to Miss Cobbe.

8723. Now, as regards the moral question, I want to ask you this. You are aware that a large body of opinion, opinion of men whom we highly respect on moral grounds, thinks that vivisection is justifiable?—Yes, I am aware of it.

8724. Practically the whole opinion of the medical profession substantially?—We have had some great exceptions, and it is very hard for a man to swim against the prevailing current of his profession; therefore when you say that you have practically got the whole it makes those exceptions you have of very great value.

8725. What I was coming to was this: We have, at rate, in evidence here, conflicting moral views. What do you suggest as the objective test for the Commission to apply. We have had evidence on both sides here from the moral point of view. What is the Commission to take as a test?—What I have tried to lay down; that I consider we can abundantly see that the intention of pain was to preserve the individual, and therefore, unless that partly enters into the infliction of pain, pain ought not to be inflicted. But I say that vivisection is a perversion of the intention of pain, and therefore immoral. Sometimes I have heard people liken vivisection to the Inquisition. I think it does a very great injustice to the Inquisition; because, disastrous as the Inquisition was, they at least had this in their minds, that they believed that the pain they inflicted was for the good of the soul of the person upon whom it was inflicted. No such claim can be made for vivisection.

8726. That hardly answers my question. Is there any objective test by which we, as a Commission, can apply a standard when we have these conflicting views of morality. The vivisectionists, as I will call them,

the people who justify experiments on animals, say that the infliction of pain is justified when there is an adequate return for it?—You have said that the greater part of the medical profession is in favour of vivisection. The medical profession are not specialists on the moral question. That is not their province; and I would refer you to the great thinkers of the world, Shakespeare, Browning, Tennyson, Dr. Johnson, Voltaire; in the law, Lord Chief Justice Coleridge, Lord Brampton; and I have with me (I thought, perhaps, it would be better to bring some; I should like to read them, if I may) the opinions of those men and others.

8727. I only want to get the principle?—Exactly.

8728. (Chairman.) That hardly arises out of the question. We have had all the opinions of those who have expressed opinions?—My point is this, that on the one hand you have the doctors, who are not, it is not their province to be, specialists in morality; they plead as to the utility or not; it is men who are moralists who plead as to the morality or not; the jury is the public. I want to show that besides those great thinkers I have mentioned, I have the opinions of two Bishops of Durham, Drs. Westcott and Moule, Cardinal Manning, and Bishop Mackarness of Oxford, Bishop Moorhouse of Manchester, Bishop Harold Browne of Winchester, as moralists strongly condemning vivisection, and if I wanted a guide for morality I should go to those. If I wanted a guide for utility I should go to doctors.

8729. I was not objecting to your stating that there were these number of people expressing an opinion; I rather objected that we should hear all their opinions read out?—They are rather strong and important. I do not know if I may read any of them.

8730. (Sir Mackenzie Chalmers.) We have practically to recommend legislation. In recommending legislation you say that we ought to consider the opinion of certain people who have studied morals?—Then do not you think I have the right to ask, as I am giving evidence on the question of morality, that you shall hear very short statements by those very great specialists in morality, the bishops.

8731. I want simply to get at the test which we have to apply?—I should be guided best in my morality by moralists, and in my idea of utility, by doctors.

8732. By bishops?—Yes, and the other great men I have mentioned.

8733. I will take that. I only wanted to know what your test was. There are one or two tiny points arising out of your evidence as to which I just want to see how far you go. You said that you would refuse to be treated with antitoxin for diphtheria?—Yes.

8734. And you think it really cowardly to adopt a remedy which might possibly save your life because you think it is wrong?—Some years ago I thought I was going to have diphtheria. I went back and wrote down a request and left it addressed to my father that if I became unconscious he was not under any circumstances to have antitoxin applied.

8735. Now, I want to put this question to you: I suppose most of us have seen children die from diphtheria—helpless children. There is not great courage in letting a helpless child die from diphtheria. Do you think you are justified, to save the life of a helpless child, in using antitoxin or not?—I would not allow my own child to have antitoxin administered to it to save its life. You should not do evil that good may come. The thing is wrong in itself; it would not exist in the world if it had not been for vivisection; it is not the sort of thing that might have been discovered by something else.

8736. That is quite true, as an instance?—Therefore I regard it as absolutely immoral to use it under any circumstances whatever; that is my view.

8737. You would let the child die?—I should.

8738. I only wanted your answer?—May I point out that that opinion was shared by Robert Browning, the poet—he distinctly expressed it—and it is shared by Bishop Moule. Bishop Moule says: "I cordially echo the words used by the late Dean Close, of Carlisle, that he would rather undergo suffering himself than have it inflicted on animals. Even if the suffering of one very dear to me could be alleviated by the conscious torture of dumb animals, I think I should have the courage to say that the animal was sacred to its Creator, and that not even to relieve my dear one should the unresisting animal suffer. Having said

this, I think I have gone as far as man can go." Those are the words of Bishop Moule, of Durham.

8739. Although the animal suffers very little, and the child suffers, from what one sees, an intensely painful death?—The question of how far the animal suffered is again a matter of opinion. One has heard dreadful things about animals that die in getting immunised.

8740. Exactly the same principle would apply to vaccination?—Yes; it is not so serious—not so terrible, to my mind, but I am an anti-vaccinator; all anti-vivisectioners are not; but I cannot see how anybody can be an anti-vivisectioner without being an anti-vaccinator—that is logical.

8741. Now, I want to put this case to you, just to test it: We lose in India a great many lives from snake bites, as you know—20,000 lives a year. By experiments on animals lately, as we have heard in evidence, a new method of applying a chemical remedy has been discovered. Are we justified in applying that remedy now, although it was derived from experiments on animals?—Certainly, because it is not the product itself of vivisection.

8742. The experiments themselves were wrong, were they?—The thing itself has been misused, in my opinion. It should have been tried on animals or people who had been already bitten by snakes, by the act of Providence—by accident.

8743. Then take this case, as a matter of right and wrong. Here is a remedy, you do not know its effect; you believe it may be useful. Are you justified, when a man is dying from snake bite, in trying an experiment on him?—If it is for his own good, then it is noble, like the art of the surgeon; if you inflict pain for the good of the person. If you tell him you are running a risk, if you think the man is going to die without—if you say to him "We know of nothing else, would you like this experiment made?"; that is the proper person to make it on. If God sends me suffering it is for me to bear it and take all possible chances of recovery, but not in a panic to go and cause all sorts of wretched creatures to have it inflicted upon them that I may escape a little suffering and death, which must come some day.

8744. Do you think it is justifiable, not knowing whether it would be successful or not, to try it on man, rather than taking a certain number of animals, and finding out that it was a real remedy?—I was talking about the case of a man whose life was in danger.

8745. You have a list of remedies that you may apply. Are you justified in trying a new remedy and rejecting the old ones in order to find out whether the new remedy is a good one? There are several remedies for snake bite; a new one is suggested, are we justified in neglecting the old remedies and trying the new untried one for the first time on a human being?—Certainly not if the doctor thinks there is any chance with the old one; but when the doctor knows there is not, then let him make his trial if he thinks he has a more powerful remedy, if he can get the consent of the person. One of the things about inflicting experiments on animals is that they cannot speak, and do not know that it is being done. If suffering comes to anything naturally, then let us do all we lawfully can to relieve it; but do not let us take the creatures which enjoy their lives before God and inflict horrible diseases upon them.

8746. (*Mr. Tomkinson.*) I understand that you object *in toto* to the principle of vivisection of living animals?—I do.

8747. On the ground that pain must necessarily be inflicted thereby?—As I have said before, I am for the total abolition of pain. I object to making living miserable.

8748. In paragraph 12 of your *précis*, I understand, you say "That being so, while I might painlessly sacrifice the life of many animals to save one valuable human one, I would not have one mouse vivisected to save the greatest of human beings nor the life dearest to me"?—I added in my copy the word "painfully"—"painfully vivisected."

8749. You mean without anæsthetics?—Yes, and without being destroyed before it recovers. I say I am afraid that is impossible.

8750. You appear to have a great disbelief in the total invariable application of anæsthesia to these

operations. You can hardly believe that in all cases, especially in operations, the anæsthesia can be complete?—That is a scientific question that I am not qualified to answer; but I think I may say this: that in questions of inoculation where the animals have to be watched for weeks sometimes and many days, of course it is impossible; and those are some of the most cruel of all probably. There are other cases in which we are told that the very administration of anæsthetics vitiates the experiment. And then there is a certificate on purpose to dispense with anæsthetics; and if there is a certificate which is used, we imagine that there are operations performed without anæsthetics; and we have evidence that it is so.

8751. We have had it stated to this Commission that rinderpest, which has been a tremendous scourge in South Africa and in India, has been practically guarded against by experiments upon a very few animals—by inoculating a healthy animal with the blood of an animal that has died of it, and obtaining from the blood of that animal experimented on a serum which has proved an effectual prophylactic against the scourge. Does that commend itself to you?—I do not think we have the slightest right to do that; it is against the principle I have enunciated several times. And I may just add that I happened to be at an anti-vivisection debate the other day, when that very point was mentioned, and there happened to be a South African lady sitting next to me, who challenged, from some statement she had lately had from South Africa, the statement that rinderpest has been combated. We are so accustomed to have all sorts of cures boomed; they all come with a great flourish of trumpets, and then they all pass away, like Koch's cure for consumption, and the rest.

8752. (*Chairman.*) But you would not say that when a physiologist or a well-known veterinary surgeon made a statement of that sort about the effect of antitoxin in South Africa, its value was very seriously deteriorated from because a South African lady contradicted it?—The real value, I should think, was that the process of time is beginning to show that he had made a mistake, if she was correct.

8753. Of course, he may have made a mistake, but it is not much evidence that he did?—I do not say so; as I say, I am not giving scientific evidence.

8754. (*Mr. Tomkinson.*) But surely, you would think it right to inquire how far that is a question of degree; how much suffering was caused to the animal from which the serum was obtained, and whether, by what may have been only inconvenience or a little pain and illness, the lives of thousands of that animal's fellow creatures were saved. You would surely take that into consideration, would you not?—Evidence of that sort, you know, first of all, is always very dubious; but leaving that quite alone, I say, no. You must have some guiding principle in life, and if my principle is right, then that act is wrong. May I just say that I have evidence of some dreadful operations which were performed by Professor Neisser?

8755. (*Chairman.*) That is getting into scientific questions, you know. I thought you rather refused to go into them?—It is a question of morality. He has made a statement on morality.

8756. It is a statement made by some foreign professor?—Professor Metchnikoff, Director of the Pasteur Institute, in his new book, "The New Hygiene"—it is a question of morality—says: "In questions of health morality should not attempt to lead hygiene, but should rather follow her."

8757. I think we have had that quoted to us before somewhere. I rather protest against the opinions of foreign physiologists, who are acting under totally different laws altogether from ours, on the question whether or not we ought to alter our laws. I do not care for Professor Metchnikoff's views about morals; his views on morals will not influence me any more than those of the man in the street?—But Professor Metchnikoff's statement is that morality is to be secondary to hygiene, and when he was over here an English gentleman got up and proposed a vote of thanks to him for his lecture, and yet he had made this quotation in support of the most horrible inoculations of women and children and monkeys with a terrible disease—syphilis.

8758. These are things which could not have been done in England?—But, at any rate, when these foreign men come over, we do not have instances of English

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physiologists repudiating what they do. On the contrary, they meet them with acclamation, and honour them in every possible way.

8759. I do not think we need keep you any longer?—I do not know whether I ought to say this. In my 13th paragraph I say: "The logical outcome of experiments on animals is experiments on man. The right to perform such has been claimed and carried out." I do not know whether I ought to substantiate that. I have got the materials here.

8760. I do not think that quoting a number of opinions of people to the same effect helps us; you are giving your evidence, but not the evidence of all these people?—I am referring to my paragraph 13.

8761. I think we have had that before. Probably the statements which you have are the same statements which we have had of certain physiologists to that effect, that some physiologists have made experiments and have actually carried them out on themselves?—But these are very different; these are experiments carried out in asylums and hospitals.

8762. In England?—In Germany and America, and some in this country.

8763. Were those in this country a breach of the law?—There you ask me a big question. In America, for instance—

8764. I do not think we can go into that?—Not to show the danger? As a clergyman, who has been giving my life up to the poor, I can assure you that I am very much affected indeed in my opposition to vivisection by the danger which besets them. If we are going to have experiments on animals, the time must come when, logically, they must be tried upon men, and I have here serious admissions, such as giving people epileptic fits and different things, which I consider are most certainly most dangerous.

8765. I do not think we can have all these views of foreign gentlemen.

(Sir William Collins.) I understood the witness to say that some were in this country.

(Witness.) I should like to have given the whole lot—Ringer's Handbook of Therapeutics, for instance, New York edition, pages 585-8, 590, 591, published by Wm. Wood and Co.

8766. (Chairman.) That seems to be an American work?—No, I believe—I am not quite sure of this statement—that after the anti-vivisectionists made a great stir about it, an expurgated edition was issued, when some of the experiments were objected to because of the outcry, but Dr. Sidney Ringer, Professor of Therapeutics in University College, London, tried the poison with salicine, "Using healthy children for our experiments"; those are his own words. I can read the description, if you wish it.

8767. Were these experiments experiments which were likely to cause any serious illness?—I can read you what was said: "Poisoning with salicine. 'In conjunction with Mr. Bury, I have made some investigations concerning the action of salicine on the human body, using healthy children for our experiments, to whom we gave doses sufficient to produce toxic (poisonous) symptoms. Our first set of experiments were made on a lad of ten. . . . He was admitted with belladonna poisoning, but our observations were not commenced until some days after his complete recovery.'" I want to dwell upon that—"some days after his complete recovery." One would have thought that he ought to have been sent to his parents. Then, "Among the effects recorded during this experiment are 'severe frontal headache, so severe that the lad shut his eyes and buried his head in his arm'; 'became very dull and stupid, lying with his eyes closed'; 'complained of tingling like pins and needles'—and other symptoms indicating severe depression."

8768. Was this on children in a hospital?—You see it is on "healthy children," I believe in a hospital; but that is a point one could look up. The fact that patient I was speaking of as being "admitted" points to it.

8769. You do not know?—No. "In case II. the experiments were made on a lad who had recovered from pneumonia, 'his temperature having become normal ten days previously.'" After he had this drug, the symptoms included "vomiting, dulness, deafness, laboured breathing, spasmodic movements, and

quickened respiration and pulse." The report concludes with these words: "We must confess we felt a little relief when the toxic (poisonous) symptoms, which became far more marked than we had expected, abated." That is one example.

8770. (Colonel Lockwood.) What is the date of that?—Sidney Ringer's book, about 1883, I should think.

8771. (Sir William Church.) You must mention, if you please, that it was the New York edition?—I have mentioned that.

8772. Then I ask what is the date of this New York edition?—That can be ascertained.

8773. But Dr. Sidney Ringer's original work was long ago, before the Act?—Then there are some other cases I can bring forward.

8774. I mean, have these cases been added in the American edition?—I cannot say. I said that I could not be quite sure of that point; I said so before I read it.

8775. (Chairman.) The date being 1883, what Sir William Church suggests is that Dr. Ringer, in his original book, gave an account of various experiments which did not include some of these. I understand that the New York contained something more than the English edition.

(Sir William Church.) I thought it might be so.

(Witness.) What I have gathered some time ago, but how far it is correct I do not know, is that there was a considerable outcry at the time, and the next edition of the book had some of these cases not in it. There is another case which was reported in the "British Medical Journal" of January 7th, 1882, page 5. This was a report of a lecture by Jonathan Hutchinson, F.R.C.S., Senior Surgeon of the London Hospital, Consulting Surgeon to the Moorfields Ophthalmic Hospital, Professor of Surgery and Pathology in the Royal College of Surgeons, and that was the case of a man kept in a shocking state of eruption—to use the words of Jonathan Hutchinson, "he was in a miserable condition from pain and irritation." The man was kept purposely uncured for a few days in order that the students might see that the disease would not cure itself. Then afterwards, to use Jonathan Hutchinson's words, he waved the magician's wand and the man was cured. I say that such a thing is unjustifiable, that a man should be kept in a miserable condition from pain and irritation. The eruption had been out about ten days, and "affected the mucous membrane of his mouth as well as the skin." I am quoting the exact words. "You may remember that we kept him in bed for a few days before we used the magician's wand, in order that all might see that there was no natural tendency to amelioration. More bullæ came out." He mentions that the limbs, head, and other most sensitive spots were so covered with bullæ that there was nowhere a space as large as the palm free from them.

8776. (Chairman.) I do not think we can follow these out at length. We should all agree, I think, that to experiment upon children in a hospital, not with a view to curing them of the complaint that they have, but with a view to experiment upon them for other purposes to a degree that would inflict pain and possibly risk upon them, would be absolutely wrong, and not only wrong in itself, but most prejudicial to the interests of all hospitals, and it would also be a legal offence to do it. I think having stated what you have done is enough to show that if that is an accurate statement in that New York edition of what Dr. Ringer did on that occasion, it was a thing which most people would entirely disapprove of?—I have two very strong cases in England, one, the giving of epileptic fits, which is mentioned without any reprobation in the Gulstonian Lectures by Dr. Gowers.

8777. If these were cases of a breach of the law relating to vivisection the evidence would be most germane, but as it is they only indirectly come in, and therefore, I think, if we have that general statement and one or two instances to illustrate what you mean, that is all you can do?—May I give a case here of a man who after experimenting on cats, the first of which died in 20 minutes and the second in 33 minutes, and other animals, goes on to say, "We have made some observations clinically. To 18 adults, 14 men and 4 women, we ordered 10 grains of the pure nitrite of sodium"?

8778. Who made the experiment, and where?—This again is a report by Dr. Ringer and Dr. Murrill in the "Lancet" of November the 3rd, 1883. After trying it

on the cats, the first of which died in 20 minutes and the second in 33 minutes, they then turned round and tried these experiments on human beings, and here are the most painful results to read of what the people came back and said who suffered from an experiment which was the direct result of vivisection, apparently.

8779. I am afraid that these cases cannot be gone into at this length. I understand that your general principle is that vivisection might lead up to experiments on mankind. We have already had a great many of these cases introduced on statements made in papers, and when we have come to have the actual persons before us who were present to tell us what really took place, the effect of the whole account has given quite a different complexion to the particular passages quoted, and as this is not germane to vivisection, but is only being used as an illustration of what experiments upon animals might lead to, I think we cannot have it in detail, because we cannot have it in such a shape as would involve our having to investigate all these cases ourselves?—Then, my Lord, may I end with this. I have said, and I have tried to establish it, that I am afraid we have grave ground to fear that vivisection on animals will naturally lead to experiments on patients. May I conclude with reading the opinions—it is very short—of important people which support that view, for instance, Lecky.

8780. I really think that reading the opinions of a number of people, or saying I have got a number of opinions or letters written to me by people who agree with me, is a thing that we cannot have. We cannot investigate all their opinions?—But these are public statements in public books. Lecky, the great historian, in his *History of European Morals*, says "It is abundantly evident"—

8781. I see, Mr. Lewis, you do not take a hint from me. I think we must make an end of these quotations on a subject which is not really before us?—It is

Miss A. L. WOODWARD, called in; and Examined.

8786. (*Chairman.*) You also appear as a witness on behalf of the Church Anti-Vivisection League?—That is so.

8787. Of which you are Secretary, I think?—I am one of the Assistant Secretaries. I am the Acting Secretary, in fact.

8788. You were in the room when I explained to Mr. Lewis that we wished to take the ethical question somewhat more shortly than we have done with some of the previous witnesses, who have gone into it at great length, and perhaps you will bear that in mind?—Yes, I do not want to dwell very much on the ethical side. My evidence is rather a statement of matters of fact, which I have tried to arrange as briefly as I can, so as to take up as little time of the Commission as possible.

8789. You have sent us in a *précis*, I see, and the first thing you desire to give us is a statement showing that illegal vivisection has been practised in England?—The special point that I desire to bring before the Commission is the fact that the absence of convictions is no proof that the law has not been broken, and, further, that the Act has been so administered as to make such convictions a matter of practical impossibility. Thirdly, I would point out that a former witness was mistaken in saying that no complaints have been made of unlicensed vivisection, because such complaints have been made; and, moreover, that, so far from complaints of vivisection having been vague and general, and "nothing that one can lay hold of," as stated by another witness, they have, in some cases at any rate, been most clear and defined and circumstantial. The case which I would especially emphasise in regard to that, which is mentioned in the second paragraph of my *précis*, occurred in connection with a meeting of the Church Anti-Vivisection League in the Technical College at Bradford on September the 30th, 1898. The meeting was a somewhat rowdy one. I am now going to quote from "The Standard" of the next morning, which said that the meeting was "completely taken possession of" by our opponents ("The Standard" is not an anti-vivisection organ, as you know), "who set aside all ordinary rules generally governing the conduct of public gatherings." I am merely giving you an idea of the state of the meeting.

8790. What you are coming to is to show that Dr. Hime, of Bradford, had performed operations without a licence?—Yes, I am coming to that as rapidly as pos-

very hard to give moral evidence without giving quotations to support it.

8782. It has little or nothing to do with our Inquiry?—Dr. Johnson warned us that it would lead to this sort of thing, and Lecky and some of the great thinkers of the world tell us that it is one of the great dangers of vivisection that a man who gets callous to pain, and does not inflict it for the good of the sufferer, will not be over particular when he gets straight from the vivisectional laboratory to the bedside of a hospital patient. That is what I have tried to explain.

8783. (*Sir John McFadyean.*) Might I ask one question with regard to this new evidence? You state that the logical outcome of experiments on animals is experiments on man. Is that true, just in the same sense as the logical outcome of running horses in cabs would be to run men in cabs?—Not in the slightest degree. I cannot see the logic of the second proposition. I can see the logic of the first.

8784. It is because I do not see the logic in the first one that I asked you to show me the defect in the logic in the second one?—Is it not plain that the vivisection of animals is performed for the sake of mankind, to cure the diseases of mankind?

8785. That is so?—Very well, then; that being so, sooner or later those experiments must be made upon man. It is very difficult, of course, to argue from an animal to a man; for instance, a pigeon, I believe, can take enough opium to kill about 80 or 90 people; therefore, considering that the time must come when experiments are made on man, those experiments will be made by a man who has deadened his feelings if he has been a cruel vivisectioner, or has not cared what pain he has inflicted upon the animals, so that he may attain his end; and I may say that the danger to patients, especially to the poorer patients, and to the community, is enormous, when they are in the hands of a man who has deadened himself in that manner.

sible. The meeting was packed with medical students, and we had an instance in the Portman Rooms the other day of what that causes. There were also 26 doctors present, a whip having been sent round to over a hundred doctors in Bradford and Leeds. Having given you an idea of the state of things which accounts for what was said, I will suppress my own account of it and pass on to the statements of three persons who were present which were sent in to the Home Office.

8791. And Dr. Hime was present?—Yes.

8792. (*Sir Mackenzie Chalmers.*) Were you present yourself?—Yes. If these statements are not sufficient I can answer any further questions, but I hope they will sufficiently explain the matter. The first statement is by the Reverend Conrad Noel. These were properly attested statements, of which the originals, I suppose, remain at the Home Office. "Dr. Kerr, a Bradford doctor, arguing against the proposal that we should work for the total abolition of vivisection, i.e., of all licences granted to vivisectioners, as it would only result in illegal experiments being made, stated that such (illegal unlicensed) experiments were already being performed by his fellow doctors in Bradford. He further said that the reason for these unlicensed experiments being performed was that it was necessary in the cause of science that such experiments should be made, but that Dr. Hime, of Horton Road, Bradford, was the only experimenter who carried on such researches under the necessary licence. He alone held the licence. The Secretary of the Church Anti-Vivisection League handed him the Government returns, which made no mention of any licence held by Dr. Hime; and Dr. Hime, who was himself present, rose, and admitted that it was a mistake for Dr. Kerr to have said that he held a licence. Dr. Hime did not attempt to deny that he carried on experiments. I drew attention to these points in a letter to the 'Bradford Observer,' appearing on the following Tuesday, concluding by saying: 'Surely some explanation should be forthcoming from the doctors!' To the best of my knowledge no answer has been given by them excepting that Dr. Kerr at once wrote me a postcard, marked private, the contents of which I suppose I should not divulge, but which I may at least say does not in the slightest degree contradict his statement as to the illegalities being practised, but rather confirms these statements.—(Signed) Conrad Noel, November 29th, 1898, 279, Tamworth Road, Newcastle-on-Tyne, Curate of St. Philip's, Newcastle-on-Tyne," one of the

Rev. L. S. Lewis, M.A.

29 May 1907.

Miss A. L. Woodward

Miss A. L. Woodward. "The above is drawn from notes made at the time compared with newspaper reports of the following morning."

29 May 1907. 8793. Is there any evidence that Dr. Hime, while he held no licence from the Home Office, did perform operations except the fact that you state that he did not state that he did not do it?—If you would kindly allow me to read the statement of the second witness it will perhaps make it clear to you. If not, I can answer any questions then.

8794. You say the second witness. You are the witness at present. I am asking of your knowledge, have you anything in your knowledge to show that, because if you say that a doctor performed operations contrary to the Act without a licence we ought to have evidence of that?—He made no attempt whatever to deny it. At the meeting I told him the matter would go to the Home Secretary. The second person who makes a statement goes a little further than that.

8795. Was the person present who makes the statement?—Yes. Dr. Hime throughout never attempted to deny it. There was a good deal of correspondence in the Bradford papers, and so on. He had every opportunity of denying it if he was flustered at the time.

8796. The charge was put in quite a different way—that he had no licence. But you say that you have a second statement?—I have it here.

8797. But in order to show that Dr. Hime operated without a licence, if that is what is intended to be proved —?—I think I have already stated that the second statement is rather more emphatic on that point and will give you a little more information as to what he did say.

8798. But if you will allow me also to put what I have to say, it is that a statement made by somebody to the effect that Dr. Hime operated without a licence is not a thing that we can have except from the witness who makes it?—I am not asking you, who are not like the Home Secretary the judge of this case, whether you think it proved or not that Dr. Hime did commit an illegal vivisection, my point is that complaints of illegal vivisection were brought to the Home Office and disregarded by them; it is the second point. Whether in your own mind you believe Dr. Kerr to have spoken the truth or falsehood is another question altogether.

8799. Nobody suggests falsehood in the sense in which you suggest it, but you are going to make a complaint of the Home Office?—I am coming to that.

8800. Was this statement which you are about to read sent to the Home Office?—Yes. This is from Mrs. Ring: "I was present at the meeting of the Church Anti-Vivisection Society held in Bradford during the Church Congress Week this year, and heard a Bradford doctor (whose name I do not know) state that there were 'hundreds of vivisections done in Bradford'; he also said that Dr. Hime was the only licensed vivisector. Dr. Hime did not contradict this, but said he had 'performed hundreds of vivisections, and should continue to do so.'" That is the point, that Dr. Hime according to this statement distinctly admitted it.

8801. That is the statement of some person which was sent in to the Home Office?—Yes. "Then later on, when the Secretary of the Anti-Vivisection Society produced a list of licensed vivisectors, and asked to be told under what licence he did them, as his name was not down—he called out that it was a 'mistake,' he was not licensed.—(Signed) G. A. M. Ring, Rawmarsh Rectory, Rotherham, November 30th, 1898." And this is the third brief statement.

8802. Was it sent in to the Home Office?—Yes; they were all sent in. "I also was present at the Bradford meeting, and can testify to the accuracy of the above statement.—(Signed) T. P. Ring, Rector of Rawmarsh." Therefore two people, if you lay stress upon that point, speak to the fact of Dr. Hime having said that. My own written statement had been previously sent in.

8803. What happened upon that?—A good deal of correspondence with the Home Office, and this is the final letter of the 11th January, which I will with your permission read.

8804. Thank you?—"Whitehall, 11th January, 1899. Madam,—With reference to your letters of the 19th October and 1st December, 1898, drawing attention to a supposed breach of the Act 39 and 40 Vic., cap. 77, by Dr. Hime, of Bradford, I am directed by the Secretary of State to inform you that he has caused

inquiry to be made, but that no case of illegal experiments on living animals has been discovered or brought to his knowledge by Dr. Kerr or by any other person.—I am, Madam, your obedient servant, (Signed) Henry Cunyngame." I venture to say, my Lord, that to require Dr. Kerr himself to come and give this evidence was a very extraordinary excuse to make. It was strange enough that in a moment of excitement he should have betrayed his friend, but even a person of most ordinary intelligence would see that it is preposterously absurd to ask that he should be expected to come in cold blood and repeat that action without any motive moreover for doing so. It seems to me, with all due respect to the Home Office, that it would be very much as if the police should decline to act in a case of alleged burglary until one of the burglars was kind enough to call at the police station and give evidence of the details of what had been committed. However, the matter was not allowed to rest there. I wrote again, and very strongly, to the Home Office, and said that questions would be asked in Parliament. I have here a copy of extract from a proof of the "Parliamentary Debates" of the 2nd March, 1899, which with your permission I will read:—"Mr. MacNeill (Donegal, S.): I beg to ask the Secretary of State for the Home Department whether he is aware that at an anti-vivisection meeting held at Bradford on the 30th September last, Dr. Kerr, speaking against the proposed withdrawal of licences granted to vivisectors, said the effect of the proposed step would be to increase the number of illegal experiments on the bodies of living animals, and that such experiments were performed by many of his fellow doctors at Bradford, as it was necessary in the cause of science that such experiments should be made; whether he is aware that Dr. Kerr stated that Dr. Hines"—(it should be Dr. Hime)—"of Horton Road, Bradford, was the only vivisector in Bradford who carried out experiments in vivisection under the licence required by law, and that Dr. Hines, who was present, stated that he had performed hundreds of vivisections and should continue to do so; whether he is aware that the Secretary of the Anti-Vivisection Society produced a list of licensed vivisectors, on which the name of Dr. Hines did not appear; and whether, in view of these facts, he intends to take any, and what, steps for the enforcement of the law against the practice of unlicensed vivisection." Then Mr. Jesse Collings replied: "The Secretary of State for the Home Department believes that at a somewhat excited meeting in Bradford on the date mentioned statements to the effect suggested were made. His attention was drawn to the matter at the time, and he caused inquiry to be made, but could not elicit any clear statement of what was actually alleged, nor any evidence of illegal vivisection being performed by Dr. Hime—not Hines. There are, therefore, no steps which he can take in the matter. I may add that it should be remembered that performing upon an animal an experiment calculated to give pain without a licence is an ordinary criminal offence which any person may prosecute." The important part of that answer appears to me to be the admission that the Secretary of State believed that statements to the effect suggested by Mr. Swift MacNeill were made.

8805. At the meeting?—Yes, at the meeting. Therefore he absolutely disregarded the testimony of four people, two of them clergymen and presumably men of education and accuracy.

8806. Do you know whether the Secretary of State applied to Dr. Hime for an explanation?—I have no certain knowledge. I heard so, but that is not a matter that I could prove.

8807. Do you know whether any other evidence except statements by Dr. Kerr and the statement of Dr. Hime at the meeting were before him?—That I could not say.

8808. At any rate he said that he had investigated the matter and came to that conclusion?—Yes.

8809. (Colonel Lockwood.) Did you hear Dr. Hime make these statements?—That is why I wanted to lay stress upon the rowdy character of the meeting. There was a great deal of noise going on, and it was difficult to hear on account of the students with their usual proceedings. I could not swear from my own knowledge that he said that he would continue to perform them. There is the testimony of two people to it that he said he had performed hundreds and would continue to do so.

8810. (Dr. Wilson.) Do you know whether any inquiry was made as to whether this was in connection with the production of vaccine lymph?—We had no opportunity

of making a legal inquiry. It is useless to tell you the reports in Bradford. I heard reports of very different things about dogs, but being merely report it is not a thing that I wish to bring before the Commission.

8811. (*Chairman.*) The fact that you desired to bring before us was the conduct of the Home Office?—First of all, the mistake in saying that cases of alleged unlicensed vivisection were not complained of; and, secondly, as to the past administration of the Act. The next case I have is not so flagrant an instance, but it still does prove again that complaints were brought. This has reference to a Lecture on Physiology by Dr. Langdon-Down in University Hall, Gordon Square, on December 9th of the same year. A lady who was present at it wrote to us complaining that he had vivisected a frog (I cannot say that personally) at a meeting in connection with the Child Study Association, a society which had a licensed vivisector for the president of one of its principal branches, and as the hall in Gordon Square was not registered for vivisection we in reply advised this lady to communicate with the Home Office. She did so, and this was the reply received:—"Whitehall, 28th January, 1899.—Madam,—In reply to your letter of the 14th instant drawing attention to demonstrations given by Dr. Langdon-Down in the course of a lecture on 'Physiology and its relation to Child Study and Education,' I am directed by the Secretary of State to say that he has made inquiry and is informed that the demonstrations you refer to were not 'experiments calculated to give pain' within the prohibition of Sec. 6 of the Cruelty to Animals Act, 1876.—I am, Madam, your obedient servant, (Signed) Charles S. Murdoch."

8812. What is your complaint against the Home Office there? Supposing what Mr. Murdoch says there were true, that would be an answer, of course?—I make no complaint against the Home Office there.

8813. (*Sir Mackenzie Chalmers.*) Then what is the point of the evidence?—To show that it is quite a mistake to say that complaints of illegal vivisection were not made. I give a second instance of that.

8814. You mean that a false complaint was made in that case?—No, I do not. I think the letter itself is an admission that a frog had been vivisected there.

8815. (*Chairman.*) A complaint was made about something which, on investigation, the Home Secretary satisfied himself was not a breach of the Act?—He satisfied himself, or, rather, he believed the report of the gentleman, that there had not been sufficient pain caused.

8816. He satisfied himself on such materials as he had before him that there had not been a breach of the law?—If you ask my opinion about the Home Secretary, I would say that he is very easily satisfied, but I do not wish to lay stress upon that. My only point there is that a complaint was made.

8817. (*Colonel Lockwood.*) Two complaints?—Yes. That was with regard to the Child Study Association. There is not the same volume of evidence there, and personally I do not see that the Home Secretary was to blame for not acting upon it; but it proves that Mr. Byrne was mistaken in saying that complaints are not brought.

8818. Could you give us the reference to Mr. Byrne's evidence?—It is at Question 147.

8819. (*Dr. Wilson.*) But are you sure that the frog was vivisected, because the web of a frog is often used to illustrate a lecture without wounding the frog at all?—I am quite sure, from what people said who were there. I was not present myself, as I was on the other occasion.

8820. (*Chairman.*) Your evidence has been addressed to showing that Mr. Byrne's answer to Question 147, in which he says that any alleged breach of the Act has always been brought before the Home Office by the operator himself, is not correct, and that complaints have been made from outsiders?—Yes, exactly; and, furthermore, as our League is only one of many anti-vivisection societies, it is so extremely improbable that every case would be brought before us that I think you may reasonably suppose that these are but typical cases of those occurring from time to time. Just as an instance of that, I may mention the well-known case of unlicensed vivisection at Burroughs and Wellcome's Laboratory at Herne Hill, which is so well known to the public that I do not wish to take up your time by going into it in detail. In that case the Home Office,

instead of punishing the breach of the law, gave them a licence afterwards.

8821. Mr. Byrne, of course, clearly made that answer, and those letters which you have read show that it is not quite accurate—at least, they seem to show that; but, of course, they both come to nothing in the opinion of the Home Secretary?—I am afraid that accusations always have hitherto come to nothing in the opinion of the Home Office. That is the unsatisfactory state of the administration of the law.

8822. (*Colonel Lockwood.*) Still complaint was made all the same?—Yes, that is my chief point.

8823. I was going to ask, whether it came to nothing or not, complaint was actually made and lodged with the Home Office and answered by them?—Yes, that is my point.

8824. Therefore, that answer of Mr. Byrne's is incorrect?—With regard to complaints being vague and general, and "nothing one could lay hold of," I have read with surprise that those statements emanated from Professor Thane. I allude especially to his answers to Questions 1284 and 1285, in the latter of which he stated that he could not specify anything as to whether such complaints were general objections as to a matter of principle or to any nuisance. The reason for my surprise is that I am informed that Professor Thane has for years held the position of Professor of Anatomy at University College, and it is very difficult, therefore, to understand how he could have failed to be cognisant of a very extensively signed memorial presented in the autumn of 1903 to the head of the college, Mr. Gregory Foster, by the great majority of female students in the Slade School of Art, as to the very great hindrance to their studies caused by the terrible cries of the vivisected animals. Anyone who knows the dread which any body of young people have of approaching the authorities of their college will understand that things must have been very bad indeed before so marked a step was taken. My special point, however, is that if Professor Thane, the Inspector under the Act, had really wished for information, he had every opportunity of obtaining any amount of information at first hand, because the names of the signatories were all open to him. Our League, as is expressed in its constitution and as one member of the Commission has remarked it to-day, bases its opposition to vivisection solely on moral and religious grounds, and that being the case, we have made a special feature of our warfare against the practice that it is a system of corruption of youth. In connection with that, I may mention that a very short time ago I was interviewing a young man who, at the age of 14, was employed in Professor Victor Horsley's laboratory. He threw up his situation at the end of three months, saying to me that he felt that if he continued it (I am using his exact words, and you must please excuse the English) "he would become one of the most cruel-hearted devils alive." I think if the Commissioners could have heard the cry wrung from that young soul they could not have helped asking themselves whether, even if the claims made for this pseudo science to utility could be made good, the game is worth the candle. Again, in University College a few years ago, the dogs for vivisection were bought at a shilling apiece from the caterer's boy. Now, I hold that it would be an insult to the intelligence of the purchasers to suppose that they did not perfectly well know that such animals must have been stolen. I do not know the present fate of that lad, but if he has become a thief, and if he is practising in his manhood what he learned in his boyhood at University College (and there was such a case, if you will allow me just for one moment to mention a case which is not English, not so long ago in the police courts in America, where the excuse made by the criminal was that he had been taught to steal animals when a boy for a similar laboratory), if that young man has had a similar fate, I say boldly that before God and man the authorities of that college are responsible for his moral ruin and for all the evils brought by the habitual criminal upon the community at large—as responsible as if they themselves had practised the profession of Fagin. I have only one last point to bring forward as to the corruption of youth. I hold in my hand a book which was used by the London School Board for pupil teachers. It took our League a hard fight of six years to get it properly expurgated. The book is by their special science teacher, William Furneaux. He there recommends those young people in a very atrocious ex-

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periment to irritate the exposed nerves of the victim, saying that when they do that at certain points of the experiment the animal will show signs of most acute pain, and in another case he says of intense pain.

8825. (*Chairman.*) Are you quoting that from his writings?—Yes, I have the book here and the page marked.

8826. Will you just state the page?—It is page 189 of "Animal Physiology," by William Furneaux, Fourth Edition, 1892—the special science teacher of the London School Board. I think, my Lord, those words of his form a very curious commentary on the assertions as to the painlessness of vivisection so freely made to this Commission. I have listened, of course, to what has been said this morning, and I venture to think that they supply a very good answer to the insinuation made that we anti-vivisectionists ought to turn our special attention towards other forms of cruelty. I think they give us a right to say that beyond all other forms vivisection is a danger to the community and a foe to humanity, and that therefore our very special attention should be directed against it. That is all my evidence in chief. I want, if I am allowed, to add one word about the work of our League on one small point. It has been stated here that the "Shambles of Science" has been withdrawn, and it has been stated that it has not been withdrawn. Our League is in a position to give a little information upon that point.

8827. I think we quite understand the position from Miss Hageby?—I will not keep you one minute. Possibly Miss Lind-af-Hageby did not know this. Every year we have offered that first edition publicly for sale to the public at our stall at the Church Congress, and we should not have done that if we had not been very sure of what the Lord Chief Justice said—that is all.

8828. (*Sir Mackenzie Chalmers.*) I want to ask you about Dr. Hime's experiments. What experiments did he perform?—That I cannot tell you; they were not mentioned.

8829. But really, when you accuse him here of having performed unlicensed experiments, surely you must have inquired what they were?—No, I did not; it was not my business. I brought it before the Home Secretary, whose business I supposed it was. Of course, Dr. Hime might naturally have said that it was a piece of impertinence if I had asked him. He was not bound to give me information; he was bound to give it to the Home Secretary.

8830. Why was he bound to give it to the Home Secretary. He was not a licensee?—Because the Home Secretary is the person whose business it is to ask questions.

8831. I beg your pardon. An unlicensed person who performs experiments is like any other person who commits a criminal offence. Will you show me anywhere in the Act where it is the duty of the Home Secretary?—Then if it is not, it only goes to show how unsatisfactory the present Act is. But the Home Secretary himself did not make that excuse for not acting.

8832. He made inquiry?—Yes, he accepted his responsibility. A good deal of correspondence went on.

8833. He may have tried to satisfy himself and to satisfy you, because, as it turned out, there was nothing in it. But you made no inquiry yourself, and even your society made no inquiry, as to whether any experiments had been performed?—We were not the people to do that, but we did do this, and I think we were about the only people to do it. Mr. Noel and I went before a magistrate the next morning and asked if we could have a search warrant to search Dr. Hime's house. That is provided for in the Act, and we were on the point of getting it until another magistrate came in who was more hostile to us and refused it.

8834. Did you swear an information?—No, because we had not come to that point when the other magistrate came in.

8835. Before you can get a warrant you have to swear an information, have you not?—Yes, we had a copy of the Act with us.

8836. At any rate, you applied for a warrant and the magistrates refused it?—Yes.

8837. Are you aware that if an offence is committed by an unlicensed person anybody can prosecute?—I am aware that that is the theory of the law, but you must perfectly well know that as long as vivisection is per-

formed in secret no private person can prosecute; it is a practical impossibility.

8838. Then how can you say that the Home Secretary is in a better position than any other person?—I do not think it is my business to answer that. I must respectfully ask that the questions are such as are reasonable, and can be replied to. I am not here to answer on a legal point. I am only here to speak to facts. That is a question to be asked of a barrister.

8839. Then I will put it in this way. I am not asking a legal question, I am asking a matter of common sense. If an offence is committed in secret how is the Home Secretary in a better position to find it out than any private person?—He is in a better position to find it out because he is more likely to have his questions answered, but he cannot always find it out either. It is precisely one of the strong arguments against allowing vivisection that it is incapable of regulation.

8840. We are discussing now vivisection by an unlicensed person; that is illegal at present. How would you make it better by making vivisection illegal? Would it not go on in secret?—Some would, but, of course, very much less, because people would be afraid of its being found out, and, moreover, all the corruption of students would be done away with then. I think the system would mostly die out. Take a case that the law does forbid now. The law forbids murder. Unfortunately murder is committed, but if the law legalised murder I think you would find that there would be a great many more murders.

8841. Coming back to Dr. Hime, you say that you do not know what experiments he performed or whether he performed any experiments?—No, I have no knowledge. It is no use to repeat reports that went on in Bradford.

8842. You have told us what was said by the Home Secretary; you have not told us what was said by Dr. Hime himself. You know that he wrote to the Bradford newspapers?—Yes.

8843. Why have you not given us his letter in which he repudiated the statement altogether?—I have the letter here.

8844. Will you kindly read it?—I said to the Chairman that I wanted to be as brief as possible.

8845. In being brief you omitted to read Dr. Hime's letter in which he contradicted all the facts?—I do not admit that he contradicted all the facts. If I read his letter may I read my reply in the Bradford paper? It is only fair.

8846. (*Chairman.*) Is this a letter from Dr. Hime to you?—No, it is a letter to the paper. We both wrote to the paper. This is his letter: "Sir,—I ask your favour to permit me to reply to a statement under the above heading, which appeared in the 'Observer' of March 2nd and again on March 3rd. On March 2nd it was stated that Mr. MacNeill, M.P., was to ask the Home Secretary in the House of Commons, *inter alia*, if he was aware that I had stated at a public meeting that I had 'performed hundreds of vivisections, and should continue to do so,' though I had no licence for experiments. I may say that Mr. MacNeill's question, as printed in the notice paper of the House of Commons, referred to 'Dr. Hime's,' which is transformed in your paper into 'Hime.' If Mr. MacNeill intended the above statement to refer to me, I repudiate it, and deny that there is one particle of truth in it. It seems to me a gross abuse of the privileges of a member of the House of Commons that, while shielding himself under the form of a question to a Minister, a member may make an utterly unfounded charge against a person, who, knowing nothing of his intention, cannot take the necessary steps to have the falsehood killed when and where it is uttered. Mr. MacNeill's proceeding is not one whit justified by his statement being founded on the untrue remarks of a third party. No fair-minded person will approve the conduct of the Minister who, before replying to such a question, involving slander of an absent person, who knows nothing of the liberties being taken with his name in the House of Commons, failed to communicate with the person referred to in the offensive question.—I am, etc., THOS. WHITESIDE HIME." This was my letter:—"Sir,—If the subject were less sad Dr. Hime's assumption of injured innocence in his letter to you would be amusing. He must be aware that newspaper paragraphs are not always absolutely accurate. Has he forgotten that one, if not more, of the Brad-

ford papers announced, on Mrs. Hime's authority, that on the night of the historic meeting he was ill in bed, and had lost his voice, although all of us who were at that meeting can testify to his being in very good voice when he appeared at the head of the rioters? I make no doubt that what Mr. Swift MacNeill really said was that Dr. Kerr had stated that Dr. Hime was performing a great many vivisections ('a lot of vivisections' was Dr. Kerr's actual expression), and that when it was proved he held no licence, Dr. Hime made no attempt to deny the vivisections, even when informed that the matter would be brought before the Home Secretary. I do not see myself that Dr. Hime gains much by the publication of the correct version, and, if he will take the advice of an opponent, I should strongly counsel him not to perform illegal vivisections in future. The Home Office has shielded him this time in defiance of a large amount of evidence, the Home Secretary having been supplied with the written statements of four persons present. But should Dr. Hime venture in the future to torture helpless and confiding creatures, it is possible that he has not yet heard the last word as to this matter.—I am, etc., A. L. WOODWARD." The first part shows us, I am afraid, that reliance is not always to be placed on the word of that gentleman—I refer to his saying that he was laid up with a sore throat.

8847. (*Sir Mackenzie Chalmers.*) Did he say so?—The letter, on the authority of Mrs. Hime in one of the Bradford papers, said so. Again, if it was not true it is peculiar that he did not deny these various statements publicly made. Still, that does not affect my main point, which is that complaint was made and that every obstacle was thrown in the way by the Home Office.

8848. Did you produce any evidence of these experiments to the Home Office?—I have told you precisely what we did.

8849. Will you answer my question, please; did you produce any evidence of these experiments to the Home Office?—Beyond what was said at the meeting, certainly not.

8850. (*Chairman.*) I did practically ask that question, and I understood Miss Woodward to say that the evidence produced was the statements made by these three gentlemen about what took place at the meeting. (*To the Witness.*) You did not produce any evidence from Dr. Hime's laboratory to show it?—We tried to get that evidence, but the magistrate would not allow us to go in, you see.

8851. (*Sir Mackenzie Chalmers.*) You were as unsuccessful with the magistrate as you were with the Home Office?—Precisely, as everyone will be while the law remains what it is now.

8852. That was in 1899. Then you referred to another case in 1899; what was the experiment in that case; at any rate, you made an allegation in the case of an experiment at a lecture?—One of the persons present did.

8853. Will you tell me what the experiment was?—No, I cannot tell you the details of it; I was not present there and I am not a physiological person.

8854. Then why do you bring it before us now?—To show the fact that complaints were made from time to time.

8855. Founded or unfounded?—That I leave to the Commissioners to judge for themselves. I am not the jury in this case.

8856. Now we will go on to Dr. Langdon-Down. You say there was a memorial from the students of the Slade School of Art?—Yes.

8857. You have not got a copy of it, have you?—No, I never have had a copy. I have been told of it by several people at first hand.

8858. How far is the art school from the laboratory?—The women's part is very much nearer than the men's, which accounts for its being female students chiefly. I have been told by people who have been there for years that the male students cannot hear the cries of the animals except in one or two rooms, in one of which they study the antique, as my informant called it.

8859. What I want to get at is this. You talk of the cries of vivisected animals; what is your authority for saying that these were the cries of vivisected animals?—It is the students who say they are the cries of the vivisected animals.

8860. What is the use of your coming here to tell us what somebody else says?—Because my point was how very easy it would have been for the Inspector to have information if he had really wished for it. Other people have told me the same. I can mention the names of other people who have also heard those cries.

8861. You heard cries from an animal, but I want to know why you think they were the cries of a vivisected animal?—The sounds came from the laboratory, and they were the cries of animals apparently in terrible pain. No other animals were likely to be in great pain there except vivisected ones, and if I heard horses make a sound in a stable I should naturally infer that they were the sounds of the horses kept in that stable, if I knew that there was a stable there.

8862. You know nothing about it yourself. I will take that?—I never said that I did.

8863. Can you give us any case since 1899?—As I have been asked about University College, if I might mention for one moment quite other work, in interrogating candidates for the Borough Council last November, two of their own accord told me that they were strong anti-vivisectionists, because of the cruelties they witnessed there.

8864. (*Chairman.*) We cannot have that. If these gentlemen connected with the Borough Council think the laboratory is badly conducted they can come and say so?—You can have them before you. I have no doubt they will be willing to come.

8865. We have had many witnesses on both sides?—I say it in answer to the Commissioner, who rather insinuated that there is not sufficient evidence. He can have plenty more at first hand if he wishes it.

(*Sir Mackenzie Chalmers.*) First-hand evidence here is the only evidence that is of use to us.

8866. (*Sir William Collins.*) Did I rightly understand, from the letter that you read of Dr. Hime to the "Bradford Observer," that he complained that he had not been communicated with by the Home Office in regard to what he called an offensive question?—I gather that, certainly.

8867. Then as regards the book that you quoted by Mr. Furneaux, did I correctly understand you to say that some change had been made as the result of a representation?—Yes, twice.

8868. What were those changes?—The first change was simply that he suppressed the words "intense pain" and "most acute pain." We, of course, were not satisfied with that as he described the same operations. We fought it at three elections, and at the last of those, which took place six years after our first fight, a majority of the Board were returned pledged to us, and asked us what we wished done as soon as they were elected. We suggested the entire alteration of that chapter, as with that exception, the book is an excellent manual on physiology.

8869. Did you gather that the passage in that book to which you take exception was an indication or intimation to students to practise that painful experiment, or was it a record of previous experiments which had been made?—I think if you read it yourself you will see that there is a decided suggestion that they should practise it. The Board themselves took it as such, because they first of all, so I was told by a member of the School Board, if I may repeat second-hand evidence, sent for some head teachers and told them they must take care that such experiments were not practised; so that evidently they recognised the danger of its being acted on.

8870. Is that manual still in use?—I am not quite sure.

8871. (*Dr. Gaskell.*) Was that a manual for children?—No, for pupil teachers. There have been some very extraordinary things in manuals for children too, but this was for pupil teachers. There was nothing so bad in the manuals for children.

8872. The animal experimented upon was a frog, I suppose?—I should think it was a monkey. That is rather insinuated, because the fingers were spoken of.

8873. Then am I to understand that your pupil teachers are so rich that they can afford monkeys to practise upon?—Will you allow me to say that I have no pupil teachers.

8874. The pupil teachers belonging to the Child Study Association, is it?—No, it was the pupil teachers of the London School Board.

Miss A. L. Woodward.

29 May 1907.

Miss A. L. 8875. I thought you said Mr. William Furneaux?—
Woodward He was their special science teacher. (*Handing the
book to Dr. Gaskell.*) The passage as to the fingers is
29 May 1907. on the next page. That is in the experiments I
described.

8876. Yes. I have looked at it, and I should have thought it was simply a statement of fact as to what happened when you stimulate the posterior root of the nerve, so that it could not have been intended really that they should carry out the experiment?—If we judge by the ordinary rules of English grammar it is the imperative mood that he uses—"irritate."

8877. It is a statement of what happens when you do irritate the posterior roots of the nerve?—Yes.

8878. Is it a recommendation to students to get a monkey and irritate the posterior roots of the nerve of a monkey?—If it is not an injunction to do that, then Mr. Furneaux' grammar is very much out, as he uses the imperative mood.

8879. It is "if"—it is not imperative?—No; pardon me, it is "irritate" in two cases—the imperative is clearly used.

8880. I can only see "if"?—I can show you where the imperative is used.

8881. In the case of Burroughs and Wellcome's do you understand that there is a difference between vivisections which are done for commercial purposes and those which are done for experiment?—I do indeed. I think they are far more dangerous when they are done for commercial purposes.

8882. You speak of Burroughs and Wellcome as being unlicensed vivisectioners?—Yes.

8883. Is not that still going on now?—But they have had a licence given them; it was brought by the International Anti-Vivisection Council before the Home Secretary in the year 1900. He then asked the advice of the Colleges of Physicians and Surgeons, and both of them reported unfavourably to granting a licence to Burroughs and Wellcome; but when the doctors happened to be on the other side they were not attended to, and the place has been registered for vivisection ever since.

8884. But for a number of vivisections that are done in commercial schools you do not require a licence; is not that so?—It was held that Burroughs and Wellcome's vivisections did require a licence. I should not think they would be legal without, but there again I am not here to answer legal questions.

8885. You are not aware, apparently, that Dr. Hime is one of those who supply glycerised calf vaccine at the present time?—I was not aware of that fact.

8886. And it is taken up by the Local Government Board and supplied throughout the kingdom, so that Dr. Hime's experiment, this vivisection that you are speaking of, was mainly for the purpose of obtaining calf vaccine—you were not aware of that?—Pardon me, I do not think that he held any such appointment at that time. I have not followed his career very closely of late.

8887. But when he spoke of hundreds of experiments

was not that for providing vaccine?—I cannot say what he meant, but I think it very unlikely, as I suppose no licence would be required for that.

8888. (*Sir John McFadyean.*) He never had an appointment; he did it in his private capacity. (*To the witness.*) He might do it in his private capacity, might he not?—That would be vaccination rather.

8889. No; the manufacture of vaccine can be carried on by a private person?—That would hardly be called vivisection, although it is so, we know.

8890. (*Dr. Gaskell.*) When Dr. Hime spoke of the number of vaccine experiments, and said that he was going to continue them, was not that simply in the preparation of calf vaccine?—It would be very peculiar if it were so.

8891. (*Chairman.*) Do you know whether it was so or not?—How can I know? I have told you exactly all I know.

8892. If you do not know anything about it you can say so to the Commissioners?—Exactly, I say so; then I only added in courtesy that I think it is exceedingly unlikely, because vaccination is not what we ordinarily call vivisection. I know it is really, as the animal is now prepared, vivisection; but it is not generally called that. It ought to be, for it is much more painful, I believe, than antitoxin. Antitoxin, though we hold it to be unjustifiable, is not by any means the most painful of experiments. I rather differ from Mr. Lewis there, but the poor calf has its head fixed in a kind of vice in order that it may not, in the irritation of a number of pustules (which they tell me vary from 40 to 200) on the stomach, have the relief of rubbing them off.

8893. (*Sir William Collins.*) Do you happen to know whether Dr. Hime, besides raising stocks of vaccine lymph, also made experiments on calves with a view to seeing whether he could get vaccine lymph by inoculating calves with small-pox?—I know nothing of it.

8894. (*Dr. Wilson.*) In referring to that book of Miss Lind-af-Hageby, you said that you had the first edition on sale?—Yes.

8895. Where?—We have a stall at the Ecclesiastical Art Exhibition of the Church Congress. I have always been very anxious to show that it has not been withdrawn. I refused to return the copies I had, and I have always had it for sale.

8896. You want to test the question whether it is libellous or not?—I want to show that it is not libellous.

8897. (*Chairman.*) Is that a reprint of the first edition?—No, it is the very first edition. When Mr. Bell, the publisher, called all the copies in I refused to send in the copies I had bought. I hold them. I am very precious over them because they are very few, and I am not anxious to part with them, but I am anxious to show the public that they can buy it, and I have it there if they wish for it.

8898. (*Sir John McFadyean.*) Have you actually sold a copy?—Yes, I have sold one.

Mr. G. H. BURFORD, M.B., called in; and Examined.

Mr. G. H. 8899. (*Chairman.*) You come to give us evidence at
Burford, the request, I believe, of the World League of
M.B. Opponents of Vivisection?—Yes.

8900. Is that a London Society?—I believe so.

8901. Is London its capital?—That is my impression.

8902. Are you a member of it?—I am not a member.

8903. But you come at their request?—I do.

8904. Do you know anything of the Society—the numbers of it?—I am afraid I cannot give you any information on that point except that I have reason to believe that it is a society of some consequence.

8905. It has a wide-spreading title. I do not know whether its members cover all the world or whether they are mainly in London?—I am unable to give you any explicit information on that point; I know some of its members, and they are, in my judgment, persons of some consequence.

8906. But whether the numbers are 100 or 1,000 or 5,000 you do not know?—It is perfectly immaterial to me.

8907. It may be to you, but to us it may be material,

because, as I understand, you come at their request and to represent their views?—That is so. I am not aware of the magnitude of membership.

8908. You have sent a very short note of your evidence, and I understand that the important point that you wish to bring before us is your objection to experiments for purposes of pharmacology and the statement that experimentation might be superseded altogether without any cruelty and with as good effect by following the methods of homeopathy?—That practically is my position.

8909. We are not here, of course, to inquire into the merits of the homeopathic form of treatment, but only as regards this question of experimentation on animals. Would you just tell us what is the method by which the homeopathic pharmacologist would test drugs?—For the purpose of investigating the remedial value of a drug from the homeopathic point of view, the experimenters are always healthy human beings; healthy in the first place in order that the vitiating results of sickness may be eliminated, human in the second place in that the major part of a medical man's work deals with human beings; and conscious human beings in the third place in that the great variety of subjective

symptoms and mental conditions which are unattainable by experimentation on animals may be added to the objective findings.

8910. Are you speaking of these experiments as being carried out on those who are already patients, or purely as experiments on people whom you are not seeking to cure of anything?—Only on volunteer experimenters.

8911. That is to say on persons who are not coming there for the purpose of being cured?—That is so. Investigation on the nature and scope of drug action is carried out solely on what I might call volunteer experimenters. Of course, in many instances these volunteer experimenters are medical men and women, and so are persons competent to observe their own condition. They must be of average health; that point, of course, is carefully gone into. The results, often of course very unpleasant to the experimenter, are recorded in part by the individual him or herself, and in part by the supervisor of the treatment; that is to say, the detail is committed to paper in as thoroughgoing a fashion as possible. The drug for investigation is taken first in small and then in larger and repeated doses, covering oftentimes a long period—weeks, months, sometimes longer than that.

8912. When you say small and then larger doses, we know that drugs in homeopathy are usually given in very minute doses, although I think most of us know that that is not the essential principle of homeopathy, but when you speak of small and larger doses what would you call a large dose; do you mean a dose as large as the extreme dose that could be given by way of experiment in ordinary drugs?—For the purpose of this investigation homeopathy is left entirely out of the question. We are committed only to the investigation of the action of a drug upon the human organisms before us, totally independent of the account to which this investigation may be turned.

8913. Totally independent of the methods of homeopathy in treating a patient?—Yes. In regard to a dose, the dose to begin with is always one which would produce obvious effects upon the organism; there must be something to record; after the patient has recovered from the effect of the first dose, a second is administered, then a third, then a fourth, usually in increasing quantities. The quantities bear no relation whatever to the quantities usually employed by homeopath. Besides the obvious detail produced by the action of the drug to the experimenter, him or herself, and to the ordinary supervisor, these experimenters repair during the course of the investigation to certain skilled observers—specially skilled observers—who each take in hand the various organs and parts of the body for which they are specially competent to speak, now the eye, now the ear, now the nose and throat, now the chest, now the abdominal condition, now the state of matters in the skin, and so forth; so that over and above the generalised disturbance which the drug produces there are also special reports of the special examiners detached for the purpose of special observation on the parts of the economy for which they are most competent to speak. We have, for instance, carried out in this country—that is, our Homeopathic Association—investigation on the drug colchicum in this way, and with your permission I should like to put in some extracts from the observations made on this drug so as to convey some idea of the character of the investigation and its thoroughness (*handing in some papers*).

8914. Was the object of those experiments to ascertain the effect of colchicum on particular diseases?—No, the pure and unsophisticated effect of colchicum given in material doses on the healthy human body as the basis for remedial application.

8915. Without reference to any particular complaint?—Without reference to any particular complaint. It was a search for pure knowledge apart from the special purpose to which the knowledge might be turned.

8916. Perhaps you can give us an explanatory statement of these papers in your own words?—In the course of that experiment there were five adult individuals engaged, each of whom noted day by day the different perturbations in health to which each was not accustomed, and when the experiment was over all their statements were summarised, indexed, and taken as manifestations of the action of the drug on their economy.

8917. How did you measure the effect upon the body; in what way did you ascertain the effect,—by the

pulse or by the temperature?—By all the methods which are usually employed for the examination of disease in a sick person.

8918. By what you would call clinical observation?—Exactly; observation of the temperature, the pulse, the blood pressure, an exhaustive examination of the blood character itself, and a very exact volumetric estimation of the urinary constituents passed day by day, together with the objective facts observed by the various special examiners to whom the experimenter in question was referred from day to day.

8919. But without any local observation, of course, of the internal organs while it was going on?—Without any local observation of the internal organs, I mean that there was no vivisectional procedure in so far, of course, as the researches of ordinary medical or surgical examination would apply. For instance, the eye was examined instrumentally, the ear was examined instrumentally, and certain other internal organs which could be got at were examined; but the same methods for examining a patient suffering from a drug disease were used as are customary for examination of patients suffering from an idiopathic disease.

8920. Colchicum, of course, is a drug which has been employed for a great many years?—Yes.

8921. It was not a new drug?—No.

8922. Will you tell us what new knowledge you derived from those experiments?—There were various technical points brought out so far as new knowledge is concerned which I am unable to summarise at present, except that in the judgment of myself and others the knowledge gained and acquired of a technical character was worth the experiment. With reference to another experiment of a similar kind carried out in America recently on belladonna, no less than 53 experimenters were engaged over a period of a few years in a similar investigation. In that instance there were no new facts of any magnitude or of new material character brought to light, but the old facts already known, or, at least, the more important ones, were accentuated and emphasised, and that gave us a clear idea that there was very little more to be got out of a new experiment with belladonna even with all the modern instruments of precision. Negative information of that kind we regard as satisfactory, although not as satisfactory, of course, as if new facts had been brought to the light of day.

8923. As the result of those investigations, have colchicum and belladonna now been imported into practical homeopathy at all?—I have here a book containing the belladonna investigation, if I may submit it to your notice. (*Handing in the same.*)

8924. It is too big for reference as we sit here, but we will keep it?—May I go a little further and say with regard to other remedies that many have been introduced into professional use through homeopathic methods which previously were entirely either unknown or practically ignored. Such remedies as nitroglycerine, for example, first came into the ranks of the profession through a homeopathic medium; and snake poisons, the crocalus snake poison, the lachesis snake poison, and the naja snake poison have also been introduced into professional use through homeopathic media, and the action of metallic gold also owes its professional usage to the same origin. The original observations were made on metallic gold by Hahnemann a century ago, and no material addition with regard to the virtues of gold as a remedy has been made since his time. The secretion of the cuttlefish, the virtues of the sepia as a remedial agent came through a homeopathic source. Original observations on bichromate of potash and its value in disease were made by Dr. Drysdale, of Liverpool, some 40 or 50 years ago, long anterior to its present revival; the bean ignatia came into professional use through a homeopathic source. The honey bee, *apis mellifica*, although investigated of late, had its action first exhaustively determined many years ago in America under homeopathic auspices. Cactus for cardiac affections, first introduced by Dr. Rubini, came into use through the medium of homeopathic experimentation; *Rhus Toxicodendron*, a favourite remedy nowadays for rheumatism and skin diseases, also owes its introduction to homeopathic auspices.

8925. You are taking these as being homeopathic remedies or drugs which are used generally in what homeopaths call allopathic treatment?—As being used by the general medical profession, but owing their introduction to the methods which I have in-

Mr. G. H. Burford,
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icated as practised exclusively by homeopathy. Calcium sulphide, a common favourite remedy nowadays for furunculosis, and used for this affection for very many years; nitrate of uranium, a common remedy for diabetes, which has found its way into professional use through homeopathic sources; and another remedy, lycopodium, which is used for fire-works or the coating of pills, has on the homeopathic method, that is to say, experiment on a healthy human being, been discovered to have curative virtues of an important and extensive character. This is merely an excerpt of the services which homeopathy has rendered to the profession at large in introducing a large number of new drugs of the virtues of which the general profession were previously not aware.

8926. Has homeopathy adopted the discoveries in drugs made by physiologists or pharmacologists who have followed the experimentation on animals other than man?—Homeopathy finds that experiments on healthy human beings yields results more ample, more direct, more fertile, and more reliable than the details obtained by experiments on animals.

8927. I may perhaps take an example which Dr. Cushny gave. You know Dr. Cushny by name?—I know Dr. Cushny by reputation very well.

8928. He gave us evidence at considerable length of importance as a pharmacologist and as a pharmacologist pursuing his inquiries by means of experiments on animals, and one instance I remember that he gave us was the use of digitalis, which he explained. I am not going to attempt to explain it in medical language at all, but he explained that, whereas digitalis was recommended as a remedy and used as a remedy or as having a particular effect on one particular complaint, it was found that in certain cases it would have a dangerous effect, while it affected other organs beneficially if properly applied. That was discovered by experiments. Supposing you were making an investigation on the operation of digitalis homeopathically, how would you do it?—I should begin by giving digitalis in a substantial dose (we know something of digitalis to start with) to a healthy adult, noticing the disturbances that it created upon his organism, taking his temperature and pulse, and having all his organs, so far as they could be individualised, examined by a corresponding expert; in fact, all the detail that has served the medical man for the diagnosis of a disease would be applied to the diagnosis of the drug conditions induced by digitalis; and the conditions which are produced in animals by digitalis given in this way would find more than their correlatives in a human being when sufficiently big doses of digitalis were taken for purposes of experiment. Then after the man had been made sufficiently ill, when it was obvious that there was nothing more to be got out of the investigation so far as he was concerned, the experiment would be stopped. Probably it would take him some months to get quite well again, and all the various instrumental observations on his pulse, on the beating of his heart, on the varying size of the heart musculature during its operation would form part of the supervisor's report; that is to say, the investigation would be carried out with the same thoroughness and the same detail as an investigation on a corresponding animal.

8929. I do not know whether you have read Dr. Cushny's evidence given here?—I have.

8930. Then you will remember, I daresay, what he said about the ocular demonstration that was given by experiments of the effect of digitalis on the blood vessels in certain cases?—Yes, I do. I can only say—Dr. Cushny is not here, therefore I must speak guardedly—that I am sure Dr. Cushny spoke in ignorance of the very considerable homeopathic literature that has accumulated since Hahnemann's time as the result of the observation of the action of digitalis on the human body; it fills volumes; and so far as the addition of modern methods of precision by instrumental appliances is concerned, it is found, in carrying out these investigations, that all the essential points as to the remedial action of digitalis can be obtained by observation on the healthy human being.

8931. After reading Dr. Cushny's evidence, then you consider that the experiments of which he gave a full description did not add anything to the knowledge which you already possessed. I am reminded that the experiments which he described were not his own experiments?—They added something to the knowledge

we already possessed, but nothing of any material importance; they added something in point of academic completeness to this knowledge, but they did not add anything to our knowledge of the remedial virtues of digitalis. The experiments cited by Dr. Cushny placed no additional weapon in our hands so far as digitalis is concerned for the treatment of the disease.

8932. You mean no additional knowledge as to the operation of digitalis in such and such quantities and in such and such diseases?—No other knowledge than was already at our disposal.

8933. I daresay some other gentlemen who are much more competent to ask you questions than I am will ask you questions upon that?—I am sorry that I have been unable to express myself with greater precision.

8934. No, I quite follow what you say. Does that answer which you have given about digitalis apply to other discoveries made by animal experimentation—that it has added nothing to your knowledge for practical purposes?—If I may say nothing material, nothing of importance to our knowledge, that answer in general, with certain exceptions, would apply.

8935. I think that is the gist of your evidence; other gentlemen will ask you questions, I daresay, and elicit more information, but I will not myself ask you any more questions.

8936. (Mr. Ram.) Have you yourself ever seen any vivisections?—Yes.

8937. In England or on the Continent?—Both.

8938. (Chairman.) I did not ask you the question—are you a practising homeopathist?—Yes.

8939. (Mr. Ram.) How long ago is it since you have witnessed any vivisections in England?—During the time that I was a student.

8940. That was before the present Act?—Yes.

8941. Have you witnessed any since the present Act, in England?—No.

8942. Will you kindly tell me the deductions that you draw with regard to the use or abuse of vivisection, from what you have been telling us just now?—With regard to the abuse of vivisection, my deduction is that the major part of the detail derived by vivisectional procedure on animals has still to be verified in its application to man; and that being the case it would be better, in our judgment, to obtain that information at first hand from man himself, and to leave the introductory information from animals on one side, as being sometimes contradictory and occasionally very misleading.

8943. You spoke of verifying the results which have been obtained by examination of animals?—Yes.

8944. Then you think that there has been something obtained, though you may think it may need verification?—Something without doubt has been obtained, and, in some respects, something very considerable; but it is not the best way of determining the remedial powers of drugs.

8945. Have you informed yourself, otherwise than by reading the evidence which has been given before the Commission, of the discoveries which are supposed, at any rate, to have been obtained by experiments on animals?—I am a tolerably diligent reader of the Medical Press, both English and foreign, and I do not think much of moment has escaped my observation which has been recorded of that character.

8946. I may take it then that you have informed yourself of the literature. Do you think that there has been useful knowledge arrived at by experimentation on animals, or not?—I believe that it is impossible to obtain knowledge from vivisection on animals which cannot be turned to some useful account; knowledge is in its nature, of course, a thing which can be utilised; but I maintain as regards drugs that knowledge would be better and more completely obtained in another way. It is a case of relative importance.

8947. You admit then that there has been some useful knowledge obtained by experimentation on animals?—I admit that there has been useful knowledge obtained. No medical man I imagine could take any other view.

8948. You spoke of making experiments on different patients of different drugs?—Not patients; experimenters—healthy people.

8949. I use the term patients in the sense of those who endure what they have to bear?—Exactly; but they were not experiments on sick people.

8950. I take it so. You spoke of trying digitalis, for instance; have you ever had the chance of verifying by post-mortem the conclusion you arrived at by any of these experiments of yours on living people?—If you mean the school to which I belong—yes, many; chiefly from involuntary experiments in cases of chronic poisonings.

8951. Have you found by post-mortem examinations useful knowledge of the effects of the drugs you had been administering?—Useful in a way, but not nearly as useful as that obtained by clinical observation of the experimenters whilst the experiments were being conducted.

8952. You spoke in one case of a man who might be in such a state that it would take him three months to get well again?—Yes.

8953. Was he a volunteer?—Yes.

8954. Conscious of what he was submitting himself to?—Yes, a medical man, and doing it with open eyes. I am pleased to say that hundreds of medical men since the time of Hahnemann have done the same thing.

8955. This man took several months to get well again?—He is not well yet—but that does not count.

8956. Not in your view?—No.

8957. Would you have the right to try an experiment on a man who was not a volunteer?—As an ethical question, in my view of ethics, it would be decidedly immoral.

8958. Do you think that in all cases it is wrong to try an experiment on an animal even if that animal suffers nothing whatever, but is under anæsthetics the whole time and dies under anæsthetics?—That question is rather a large one, and I think the answer would depend entirely upon the specific purpose for which the experiment was conducted.

8959. Supposing a case in which a skilled experimenter believed that he could obtain useful knowledge, either as to the action of a drug or the nature of a disease, and he puts an animal under anæsthetics, and the animal dies under anæsthetics, and suffers nothing, is it right or wrong for the experimenter to try and obtain that knowledge?—Do you wish me to answer an ethical question?

8960. I thought you were here to do so; you have been putting the ethical point of view. If you would rather not answer, I do not press it?—I am perfectly willing to submit my own answer. If the experimenter was a competent person and was convinced in his own mind that he was doing what was right, it might be a right thing to do.

8961. (*Colonel Lockwood.*) Have you any suggestions to make to this Commission as to any additional safeguards which could be provided supposing that vivisection, as it is popularly called, was continued?—I think that prior to the experiments on animals with any new drug, there should be experiments carried out on human beings, not afterwards, but before, and that any point left undecided in the experiments on human beings might then be considered possible (I say, possibly) as a fit and proper subject for experimentation on animals; but I believe that investigation with drugs on healthy human beings should in all cases precede the experimentation on animals.

8962. But you believe that all experiments on human beings should in all cases precede experiments on living animals?—That is as regards drugs on volunteer human beings, of course.

8963. (*Sir William Church.*) Do the principles of Hahnemann still remain as principles in homeopathy?—Yes, that is to say, not all of them, of course, nor all in the condition in which Hahnemann left them; homeopathy, like medicine in general, is a progressive science, but the main principle of homeopathy; that is to say, the law of similars, which is, of course, the centre and pivot of homeopathy, homeopaths hold as scientifically valid.

8964. But, I suppose, it is admitted by them that there has been progress in medicine since?—Distinctly.

8965. In our knowledge of disease?—Distinctly, very much so.

8966. So that some of his main principles you have thrown overboard?—I imagine that blood letting and mercurialisation, and things of that kind, have gone by the board, too.

8967. How about psora?—Psora was a technical term used by Hahnemann for expressing the degree of pathological enlightenment at the time, or which we now consider as diathesis.

8968. He took as his principle what he called an established fact, that there does not exist a single disease which could have a material principle for its cause. That is no longer held by homeopathy?—That has gone by the board as being not in keeping, of course, with modern knowledge; but its retention or its omission affects in no material degree the practice of medicine; it does not affect the usage of the law similia; it is not an essential part of homeopathy. It was one of Hahnemann's views.

8969. Neither was psora an essential part of homeopathy?—Not as a pathological notion.

8970. I do not want to ask you anything further about pharmacology or the use of drugs, but we have advanced beyond the use of mere drugs, or the world thinks we have advanced beyond that. Do you not agree that serum therapy is of use?—Yes, as a private individual I do most emphatically.

8971. Could that have been arrived at without experiments on animals?—It is quite possible that it could have been arrived at in another way, and that the same properties which are utilised in serum therapy might have been used without having recourse to animal experimentation. I will cite one instance of that. The virus of consumption, that is to say the tubercular virus, was prepared by homeopaths and employed by homeopaths for the treatment of consumption long before Koch's discovery of the tubercle bacillus.

8972. I will take your answer?—I believe that what I am saying is capable of verification, and I am quite prepared to fight it out elsewhere if my statement is taken exception to.

8973. (*Dr. Wilson.*) Do you mean that the tubercle bacillus was discovered?—Not the tubercle bacillus, but the virtue of the material in which the tubercle bacillus was contained. The tubercle bacillus was not isolated, but the material in which it is contained, the lung substance, for instance, was prepared by homeopathic methods and used successfully before Koch launched his discovery. I will only quote Von Behring, who will have it that all this is homeopathy, and that Hahnemann's word homeopathy should be employed to indicate it. It is the cure of a disease by a substance which it itself manufactures and as nearly an instance of a similar remedy as could very well be selected.

8974. (*Sir William Church.*) You think then that our present knowledge of such substances as all the antitoxins could have been obtained without the use of animals?—Exactly how antitoxins may best be obtained is still an open question. It is possible that researches in the next 50 years may indicate quite other methods.

8975. I do not want to know anything about 50 years hence; I want to know whether in your opinion the present position of our knowledge with regard to antitoxins could have been attained without experimentation on animals?—Using antitoxin as a remedy which cannot be bettered in any way, of course all the knowledge with regard to antitoxin has been derived through experimentation on animals. I maintain though that antitoxin in its present form is not a final and ultimate preparation. There are other methods of preparing virus which, in my judgment so far as cure is concerned, are equally valid.

8976. Have these other methods been brought into use and generally accepted?—In the case of tuberculin, yes.

8977. We will pass from that?—I take that as a typical instance.

8978. You take tuberculin as a type?—As a remedial method of nature.

8979. Was it attenuated?—It was titrated with sugar of milk in a mortar, and the same dose readily obtained that Professor Wright now advocates—that is, anything from $\frac{1}{1000}$ th to a $\frac{10}{1000}$ th of a milligramme.

8980. Any other?—Homeopathic original research is unfortunately heavily handicapped in this country. We have no school and no well-salaried laboratory, and we have had to be content with our research in tuberculin. I take that as an instance of what might be done with other serums and viruses.

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8981. Apparently you have done a great deal in the way of introducing drugs into medicine. Have you introduced a new soporific, or anything of that kind, which has been of use?—I am afraid I can scarcely answer that question in the way it is put. If you will permit me to frame my answer in this way: The method of drug experimentation on human beings which homeopathy has introduced has very largely cleared away for remedial purposes those particular labels of soporific, diuretic, and so forth, which one was formerly accustomed to hear. This view starts by assuming that a drug has only either one property or one main property, but the results thus gained on animals with any particular drug can be enormously amplified by experiments on a human being, and thus results not obtained by animal experiments can be observed and verified on human beings, which results involve a far wider area of affection than was observed in the case of experiments upon an animal. Therefore we do not label drugs as diuretic, soporific, and so forth, but by what we call their *tout ensemble*, and we endeavour to fit in that *tout ensemble* with the case of any particular disease which may be placed before us. Wherefore homeopathy knows nothing of soporifics, but it knows a good deal of similar conditions.

8982. One of the bases of your work is the study of symptoms, is it not?—The study of symptoms constitutes a part.

8983. Therefore, of course, you do not go in for experimental work on animals?—No; but we go in for the observation of objective changes on the human being at the same time as the symptomology is being evolved.

8984. Are you acquainted with anything that has been done with regard to the neutralising of snake poison?—I cannot say that I have in my memory at present anything that has been done from a homeopathic source.

8985. You know that such a substance as antivenin has been discovered?—Yes.

8986. Do you think it is of any use?—I am really not competent to pass any opinion, because it is a branch of practice in which I do not daily engage.

8987. Perhaps you do not care to answer the question whether you think that could have been obtained by homeopathic methods without the use of experimentation on animals?—I prefer to reserve my judgment on that point, because I am not acquainted with the detail.

8988. (Chairman.) Is your society, the World League of Opponents of Vivisection, essentially a homeopathic society?—No, it knows nothing of homeopathy.

8989. (Sir William Collins.) Are the views that you put before the Commission this afternoon those of the Homeopathic School of Medicine generally?—Not necessarily. The School of Homeopathy takes divergent views on many questions which I have answered; but you may take my views as expressing the views of the major number of medical men practising homeopathy in Great Britain and America.

8990. And also as the views of the World League of Opponents of Vivisection?—That I cannot say.

8991. I thought you appeared to represent them?—Yes, but I have not informed myself of what the views of each individual member may be; I can only express my own views.

8992. Is the evidence you put before us then to be taken as representing the views of the World League?—So far as regards the desirability of transferring the scene of action of experiments with drugs from animals to man as a general procedure, I believe so.

8993. Do you contest the proposition that useful drugs have been introduced into therapeutics as the direct result of experiments on animals?—I do not contest the proposition, but I say that many more useful drugs have been introduced into general medical practice through the medium of homeopathic experimentation on the adult healthy person.

8994. You think that the method of experimentation on animals should be abandoned in favour of the method that you advocate?—I believe that legitimate knowledge should be legitimately obtained from any source, but, as I said just now, in case of experimentation to be conducted on a new drug for the purpose of discovering its remedial virtues, it should be conducted in the first place on a healthy human person.

8995. But do you take up the view that experiments on animals for a scientific purpose should be restricted or prevented?—I believe, as the result of my observa-

tion and experience, that it would be legitimate to very heavily restrict them in one direction—that is to say, that information derived from animal experimentation with drugs could and should be obtained from experimentation on human beings. They ultimately have to come to it, because animal experimentation results have to be verified on the human being as the ultimate issue.

8996. It has been alleged, for instance, before the Commission, that cocaine, adrenalin, sulphonal and other remedies, have been introduced as the direct result of experiments on animals. Do you suggest that those remedies are not useful or that they have not been obtained by that method?—I do not suggest that they are not useful, and I do not suggest that they have not been obtained by that method. I should be very sorry to do anything of the kind. I do suggest that they could, and should have been, obtained through the medium of human experimentation. You get your results then unsophisticated.

8997. How would the preparation of antitoxins be affected by the method which you advocate?—As I say, the opportunities of the Homeopathic School for Research are unfortunately rather limited, but in all probability the preparation of antitoxins would undergo what one might call homeopathising—that is to say, that when homeopaths turn their alternation more extensively to the subject probably some alternative method similar to that used for the preparation of tuberculin could and would be devised; we take that as an indication of what can be done with toxins of this description when they are submitted to homeopathic preparation. So far as diphtheria antitoxin is concerned, I am unable to give you any information with regard to a better preparation after the homeopathic fashion; it has not hitherto been attempted.

8998. If we wanted to get an antitoxin for use to-day we could not get it by the methods that you advocate?—Not as yet to-day.

8999. (Sir John McFadyean.) It has been suggested as one of the objections to animal experimentation conducted on healthy animals, that the conclusions based on such experiments cannot legitimately be applied to a diseased condition; do you think there is any force in that objection?—I believe that they cannot be applied by inference to a diseased condition in man, and, of course, it is to the human being that the results of all these experiments are applied.

9000. But putting aside the question of difference of species of animals, do you think there is any force in the contention that conclusions drawn with regard to experimental conditions in healthy subjects cannot properly be applied to diseased states of animals of the same species?—No, I should not hold that.

9001. You think there is no force in that objection?—No.

9002. Your idea of the proper method of investigating a new drug is that it should be tested in increasing doses, beginning, I take it, with what you regard as perfectly safe doses, on human beings?—That is so.

9003. After all, that is an experiment, is it not?—That is an experiment, necessarily.

9004. But you think it is moral because the individual consents?—Exactly.

9005. It would be immoral without his consent?—That is my ethical position; it is a personal position, of course. That is what I personally wish here to convey as my own conviction.

9006. You also hold very strongly that results obtained on the lower animals may be misleading when applied to human diseases?—May be and often have been misleading.

9007. Does it not appear to you that you have barred out all method of investigating animal diseases and the action of drugs on animals?—For the purpose of investigating animal diseases it is quite possible that in the interests of the animals another system of ethics might prevail. So far as regards human diseases, I hold that this is the only legitimate method.

9008. It is a very important question. I daresay you can realise that in veterinary practice from the point of view of £ s. d. it is a very important matter?—Exactly.

9009. I want to ask you whether you think that the method of investigating a drug intended for human use which you advocate, might be extended to animals?—No, I should have the same scientific objection to extending observations on human beings to animals.

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9010. But you rather misunderstand my question. I understand your position to be that when it is desired to obtain information as to the usefulness of a particular drug on a particular species, it must be tested on healthy individuals of that species?—Yes.

9011. Have you any objection to allowing that general principle to be extended to the investigation of what I may call veterinary drugs?—For a knowledge of the diseases of the lower animals, knowledge obtained from the lower animals is, of course, essential, and is a scientific necessity. That is not unethical; I would not maintain that position for a moment, I think it would be untenable. I do not, as my personal opinion, regard that as immoral.

9012. So that you think that experiments on animals which are intended to increase one's knowledge as to the means of curing and preventing animal diseases, are legitimate?—Much would depend upon the object of the research and on the investigator.

9013. I say that the object is to gain increased power of curing and preventing diseases of that particular animal species?—I do not see any alternative to obtaining valuable information as to the diseases of animals except through the animals themselves.

9014. Then you do think it is legitimate? I think you are bound to make that admission, because you lay down the general principle with regard to the proper line of investigation in man, and you are bound to admit that it is the only proper method of investigating the use of drugs which are intended to be applied to the cure of animals?—That is so.

9015. You have mentioned a number of remedial agents which you said have been introduced through homeopathic sources. I did not quite understand what you meant by introduced through homeopathic sources?—I mean that the knowledge of their properties was first gained by the homeopathic method—that is, experiments on healthy human beings, and then published for the behalf of the profession at large.

9016. But in that respect there is nothing different between homeopathy and other practice. There must be something in the first instance to suggest that a drug might be tried or might be useful?—If a man goes into a laboratory, opens a bottle, and takes a good sniff of some very powerful odorous medicament—

9017. Such as gold?—Such as tri-nitrin.

9018. But please take gold?—What it was that induced Hahnemann in the first instance to take gold I do not know, but I know what induced Professor Albert Robin to take up the investigation of the remedial virtues of gold; it was the results obtained by homeopathic practice—

9019. Such as?—The cure of tertiary syphilis.

9020. The cure of tertiary syphilis with what?—With gold.

9021. Then it was not through homeopathy that gold was introduced as a remedy?—Yes, it was.

9022. What led to its introduction?—That I could not say.

9023. It could not be a knowledge of its efficacy in the cure of syphilis that led to its introduction?—What was in Hahnemann's mind when he took up and investigated the properties of gold I cannot say.

9024. My point was that there did not seem to be anything special about your principle of homeopathy introducing a drug. I can quite see that there may be something special in your method of testing it, but you have not explained anything special in the method of trying a drug or introducing it in that way?—Of course, the same impetus impels the homeopathic experimenter as affects the experimenter on animals; there is a preparation brought before his notice; and he may be and is desirous of testing whether it has remedial virtues or no. What was the motive in Hahnemann's mind in that particular instance with regard to that particular preparation, of course, I cannot say.

9025. You do not suggest that those who believe in homeopathy have any special method of investigating surgical conditions and diseases?—The same method that is open to the profession at large.

9026. It did not appear to me to be quite clear why you were so emphatic in stating that when it is desired to investigate the action of a drug which is intended for use in man the experiments on human beings should

precede those on animals? Is there any moral law concerned there?—I did not give that preference on the basis of a moral law, but on the basis of scientific results.

9027. But must there not always be a slight element of danger in using a perfectly unknown drug on a human being?—There is always an element of danger, but after some practice, of course, experimenters become more or less facile.

9028. They cannot be facile with a new drug?—But they can go to work in such a way as experience has indicated, just like explorers in a new country can form some idea of the probable dangers in front of them.

9029. It is not obvious how a man can estimate the probable safe dose of an entirely new drug?—It is obvious that it is a matter of experience. Supposing that a man has a new drug and dilutes it one million times, the probability derived from previous experiments is that a millionth part will not contain a lethal dose.

9030. It all depends upon what was the strength of the material before he began to dilute it?—Supposing he finds a white powder in a paper submitted to him, as being some material which hitherto has not been investigated, he has not the faintest idea whether it is innocuous or lethal. The first thing that he would do would be to take this crude substance and considerably dilute it, let us say one million times, and the records of previous experience with drugs would lead him to understand that in all probability a millionth part would not be a lethal dose. He might therefore with propriety begin his investigations, and then he can heighten the dose according to his judgment.

9031. But, admitting that the danger of subjecting him to too large a dose is slight, why not begin with a smaller dose on an animal; it is not denied, is it, that it forms some guide as to whether the thing will be dangerous or not?—It is denied that it affords some guide as to what is safe for use among human beings. The whole of the animal experimentations as a matter of fact have to be verified in some form or other and at some time or other on the human organisms. Why not, therefore, begin with the human organisms at once?

9032. I have tried to convey that in my opinion you should begin with an animal because it safeguards the human being, and that you do not lose very much time by it?—You do not lose very much time by it, and in the majority of instances you gain little.

9033. You have frankly admitted that the class of so-called protective sera have been discovered entirely by animal experimentation?—Yes.

9034. But you ask us to believe that it is quite possible that they might have been discovered by other means?—Yes, that is my position.

9035. There does not seem to be much force in the suggestion that they might have been discovered by other means. Everybody, yourself included, has had these other means at disposal, and yet you have to admit that nobody discovered anything in the nature of a protective serum until it was discovered by animal experimentation?—If homeopaths had had at their back all the immense resources, the revenues of a province often, which allow these investigations to be carried out, I do not think that homeopathy would have been behindhand in indicating in all probability another and a better way.

9036. But you do not require a very great laboratory, and you do not require a laboratory to be richly endowed in order to do valuable research. Some of the greatest discoveries have been made by people who have had very limited equipment?—But the work with regard to antitoxic sera has been done by people detached from ordinary daily life and the pursuit of their profession for this specific purpose. That is a little expensive. You have to pick your man also; he must have special faculties and ability for that work. Our body in England is not sufficiently big to detach men from daily life and practice for this specific research work, except occasionally.

9037. But you are aware that there is a great deal of money behind this question. If homeopathy can produce efficacious things like the various protective sera, an immense amount of money can be made out of it. Does the fact that they are not invented not tend to diminish the hope that our knowledge in that direction

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will be extended in any other way than by animal experimentation?—I should be sorry to think that the basis of scientific experiment was the love of gold.

9038. I did not mean to suggest that it was.

9039. (*Sir Mackenzie Chalmers.*) You are not yourself a member of the World League of Opponents of Vivisection?—No, I am a member of no anti-vivisectional body.

9040. How did they come to ask you to give evidence on their behalf?—The application came through a friend of mine, a homeopathic colleague, a member of the League. I asked to take this homeopathic point up with regard to the subject, and, as a matter of fact, he requested me to do it myself, and I had no objection.

9041. Practically your evidence is evidence on behalf really of the mass of homeopathic practitioners?—You may put it so.

9042. I want to ask you this: Speaking on behalf of people whose opinion you represent, what changes would you advise in the existing law. We have to consider the Act of 1876 and whether it requires amendment. Will you kindly tell me whether you suggest any amendment?—Are my suggestions to be put from a practical point of view?

9043. Yes, because we have to advise the Government?—The main suggestion that I would make is that when experiments with regard to the remedial action of drugs—I am not dealing with other elements now—are concerned, they should in the first instance be carried out on healthy human beings. That is my point.

9044. Would you have it enacted by legislation that a licence should not be granted for an experiment upon an animal until a corresponding experiment had been made on a healthy human being?—I would leave that to the discretion of the Inspector; I do not think that a hard and fast rule could be laid down in every instance; but I think that discretionary power should be given to a responsible individual in the case of every specific question of this kind being submitted to him, he being fully and entirely responsible for his decision.

9045. The Inspector merely inspects what is going on. I do not understand your suggestion. How would inspectors deal with it?—May I put it in this way: that the advisers of the Home Office should be held responsible in each instance for the vivisectional investigation on the lower animals of the properties of a new drug, and that they should give their specific reasons in each case why the prior investigation on a healthy human being was not adopted.

9046. You agree that that would limit experiments enormously, because there is not a large supply of healthy human beings willing to be experimented upon with unknown drugs?—I think you would find that in the interests of science a sufficient supply of healthy human beings was forthcoming. In America the supply of medical students in the Homeopathic Colleges is a never failing source of adequate material.

9047. You think it is not only justifiable, but right, that in the first instance a drug which may be remedial or may be toxic should be tried on man?—I believe it is not only justifiable but right, if it is carried out under the supervision of a competent experimenter. Anybody can bungle, of course.

9048. (*Chairman.*) Where do you get the volunteers from; are they from among medical students or from people who are paid to undergo it?—Some of them are medical students; some of them are persons who do not require any financial inducement; and some of them require just sufficient financial assistance to recoup them for detachment from the duties which they ordinarily discharge.

9049. (*Sir Mackenzie Chalmers.*) Just to test those principles a little further, take the case not of a new drug but of a specific poison like snake bite; would you say that it is justifiable to try an experiment on the lower animals with a view to testing a remedy for snake bite?—We do not go to work quite in that way. I am sorry to appear unsatisfactory in my replies, but it is not always possible to answer a question in the way in which it is put. In indicating what was useful for snake bite, homeopaths would run all over the details of the drug experiments, many thousands in number, which they had in their possession, and see which of these paralleled the condition of snake bite, and they would put their finger upon the series of experiments already made which most paralleled

the condition of snake bite as indicating the remedy; it might not even be necessary to carry out any new investigation. If I may give an instance, when cholera in the thirties swept over Europe, Hahnemann was applied to for effective remedial measures to meet the cholera epidemic. He indicated a satisfactory remedial measure from homeopathy, without additional experiments, from what had been already made.

9050. With what result?—He got, if I may put it very much better results than the current therapy of his time, and homeopaths afterwards in London repeated them, with such success that they were made the subject of a special Government Inquiry. All this is ancient history.

9051. And are his methods still applied in India where cholera is always present?—Yes.

9052. By whom?—They are mainly applied by those people in the Army with homeopathic leanings, of whom there are some; and also by such educated professional native gentlemen as engage in homeopathy, of whom there are not many; but there are some.

9053. I suppose you admit the utility of vaccination?—Is it my personal opinion that you want?

9054. I only ask it as the basis of a further question?—In the majority of instances I admit the utility of vaccination.

9055. Vaccination has been known for about 100 years?—Yes.

9056. Have homeopaths by their methods discovered any equivalent substitute for vaccination?—Homeopaths hold vaccination to be as striking an instance of homeopathy perhaps as could well be devised; the majority prefer to carry out vaccination in the ordinary way by subcutaneous injection of vaccine matter. Some of us prepare the vaccine matter according to the method that I have indicated for tuberculin—that is to say, by purely homeopathic process—and, it is said, with equally satisfactory results.

9057. (*Chairman.*) And it is swallowed?—Yes.

9058. (*Sir Mackenzie Chalmers.*) As a prophylactic or as treatment?—Both.

9059. May I ask from what it is prepared? I do not quite understand how the method applies in this particular case. Would you take a case treated in this way and tell us what is done?—Let us say, for instance, that an ordinary small child presents him or herself with three or four healthy vaccination vesicles; the material is taken from all these, dried, diluted, usually titrated in a mortar with sugar and milk, and when it has reached a certain degree of attenuation—how far attenuation is made depends upon the ideas of the individual—when it has reached what he considers a satisfactory attenuation, that is taken as the remedial agent.

9060. How is it introduced?—It is swallowed. When you dilute it two or three million times there is no longer objection to its being introduced into the economy through the mouth.

9061. Have you any figures showing how far this vaccination by homeopathic doses which are swallowed has any effect?—As far as vaccination is concerned, I cannot produce figures on that point (although I know that this thing has been done), because a good many of our homeopaths, as I say, still prefer the ordinary and what they consider to be the least troublesome way of introducing the vaccine matter into the arm; they hold that that is just as homeopathic as if it is administered through the mouth, only that a more considerable proportion of the drug is required.

9062. (*Chairman.*) Is it accepted as a substitute for vaccination?—That is a point that rests with the homeopathic public; some do and some do not.

9063. I meant was it accepted as a substitute by the authorities who require vaccination?—No, they want some local evidence of what has transpired, and we can afford them none in that way.

9064. (*Sir John McFadyean.*) You spoke about the necessity of greatly diluting the material from the child's arm. Have homeopaths or any other people ever produced any evidence whatever to show that material taken from a child's arm and given by the mouth to another will do it any harm?—Evidence of that kind is obtainable just as evidence is obtainable of any other remedial action.

9065. It is not remedial; it is an ineffective method. Can you give the Commission any reference to any experiments on human beings showing observable effects produced on human beings; by giving them an undiluted dose by the mouth of vaccine matter?—Do you mean immediate effect?

9066. Or remote; any effect whatever?—No obvious effect.

9067. Can you refer me to any effect at all?—When it has been tried in that way the homeopathic advisers maintain that the patients are immune.

9068. It is not a question of immunity with small doses. I want to know why there is a necessity for diluting it. Is there any evidence to show that it is harmful if you give it undiluted?—No, none.

9069. (*Sir Mackenzie Chalmers.*) Have any experiments been recorded showing any effect from these minute doses?—Any prophylactic effect, yes; that is to say, any protective effect. Many of them in America; I do not carry the detail with me.

9070. Not in England?—Not in England.

9071. There is only one other point that I just want to get your opinion upon. Homeopaths of course study physiology?—Yes, they have to do so.

9072. Do you think that experiments on animals for the purpose of elucidating physiological problems are justifiable or not?—Am I compelled to answer that question?

9073. I ask your opinion, that is all?—As a matter of fact it is still a moot point with myself; I have not made up my mind.

9074. I take your answer. Would you agree that physiological knowledge has been advanced by experiments on animals?—Nobody can deny that.

9075. Whether those experiments were morally justifiable or not is a moot point in your mind?—That is so.

9076. But nevertheless we are bound to avail ourselves of what has been done in the past?—There is no earthly reason why we should not. We still use rubber, although it comes from the Congo. I trust I have made it perfectly clear that homeopaths at least hold that their method of obtaining information with regard to the remedial virtues of a drug—that is to say, by experiments on healthy human beings—is in their opinion of immeasurably greater value than the alternative method of experimentation on animals.

9077. That I understand to be your opinion. May I take it, then, that your evidence to-day is really confined to that aspect of what we may call vivisection which relates to the action of drugs—pharmacology?—Yes, that is so.

9078. And whatever opinion your Society may hold, you are not here to go into other questions?—I speak for myself.

9079. Among homeopaths you have surgeons as well as physicians, of course?—Yes.

9080. Is it ever justifiable, do you think, before trying a new operation on a human being, to try it on an animal under anaesthetics, or would you try it first on the human being?—I should imagine that most competent surgeons (and I think that no one but a competent surgeon—that is to say, a thoroughly skilled, experienced person—would do it) would answer your question that most decidedly it would be better to try the experiment on a human being and not on an animal. I may say that I have myself some experience in surgery, and that personally would be my decided conviction. A man knows exactly what are the necessary limitations when he operates on the human body. He has some preliminary knowledge to tell him whether his proposed experiment is in flat defiance of what nature can bear or not. An experienced person would know exactly the tax that he proposed to levy on the economy before he started, which only experience could afford; and if he was simply skilled in operating on animals he would not know the exact tax that he proposed to levy on the human economy. He would be more likely to get a satisfactory verifiable result by his human procedure than by his animal procedure.

9081. I was thinking of this sort of thing. You have from time to time new kinds of sutures; you substitute, say, a catgut suture for a metallic suture. Is it justifiable to try that first on a human being, or would you try it first on an animal?—One of the most distinguished surgeons of the day, Professor McEwen, of

Glasgow, had to deal with the same subject and answer that question. He made no animal experiments.

9082. I am asking you, do you think he was right?—Certainly.

9083. It is better to try at first on the human being?—Most decidedly. When you have dealt with animals you have to deal afterwards with human beings, and thus you have to deal with different tissues; and after you have produced animal results you have to produce human results, to see whether those results are verified. It is better to go to the human being at once.

9084. I would like to get your opinion on this point. When you find a new surgical method successful with animals, does it not go a long way towards justifying you in trying it on human beings?—Yes, it would; but I hold that the experience and *naus* possessed by a competent operator would render unnecessary the preliminary procedure on an animal.

9085. (*Dr. Gaskell.*) I was wondering whether, in your method of investigating the action of drugs, you would say that you get accurate knowledge of the action of drugs on different organs or on the patient as a whole?—Both on the patient as a whole, and then in cases of poisoning, acute or chronic, by the *post-mortem* investigations that follow. We accept those *post-mortems*, of course, and what they disclose, as part and parcel of the investigation. Investigation such as this may cover many countries and many years.

9086. Do not you feel sometimes rather doubtful as to whether it is one organ or another that is chiefly affected by the drug?—What you get in the case of poisoning by a drug is exactly parallel to what you get in a condition of disease wherein it is very often impossible until the patient is dead to determine the precise local seat of the mischief.

9087. Is it not better, both in that case and in the case of disease, that subsequent experiments should be made upon animals to see whether that drug does act especially upon one organ rather than another?—That has been done so often, and so little practical information has been obtained—that is to say, the additional power which it has given with regard to the selection of remedies has been so insignificant—that it is no longer held among homeopaths as affording any material additional information of consequence.

9088. What I was mentioning rather was that in physiological experimentation we have the power of analysis which is deficient at the bedside; you can limit your experiments to one particular organ or your drug to one particular organ?—Precisely.

9089. And so you can gain information without complication from other organs; do you not consider that an advantage?—Not much. You often find in cases of disease, particularly a chronic disease, that you do not have to treat one organ solely and wholly. It is the organ in connection with the environment in which it finds itself that the homeopath calls the *tout ensemble*, the totality of symptoms and conditions which has to be treated, and not the simple outburst of one particular organ.

9090. Do not you think that, whereas clinical investigation has been present to the minds of medical men for generations, a great advance has been made of recent years; do not you think that within the last 30 or 40 years knowledge has been very much more quickly obtained than before?—Knowledge has been much more quickly obtained because the armamentarium which advance in science brings with it has been much more copious than before.

9091. Has it not been coincident with the method of experimentation?—It very often grew up side by side with it.

9092. You consider that the experimental investigation has not really largely assisted in elucidating the slow methods of nature's experiments?—Experimental methods on animals, in my judgment, have not added any material information to that afforded by similar experiments carried out on the human subject when they have been taken as the alternative; and when no similar experiments have been carried out on the human subject often nature's experiments were sufficient in number and demonstration.

9093. Would you say that of our knowledge of the tracks and functions of the central nervous system? Have we not gained an enormous amount of knowledge of late years from experiments on animals in that direction?—Physiological and pathological

Mr. G. H.
Burford,
M.B.

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Mr. G. H. knowledge, yes. What is the outcome of it with regard to treatment?

9094. I was asking you about our knowledge; you can get a certain amount of knowledge of disease from the hospital bedside?—Yes.

9095. Have we not been able to understand those diseases much more in consequence of our more accurate knowledge of the tracks and functions of the nervous system, owing to experiments on animals?—Without doubt, as far as academic knowledge is concerned, decidedly; no medical student even could take an opposite view.

9096. Has not that enabled the surgeon to remove tumours when otherwise he would not have known where they were?—He does not always distinguish himself in that direction. The results of brain and spinal surgery, based on vivisectional experiments, are not to be compared with the results of abdominal surgery.

9097. Based also on vivisectional experiments?—No, the most skilful abdominal surgeon of the Victorian era, Lawson Tait, publicly foreswore any alliance or allegiance that he had to knowledge vivisectionally obtained. And the first ovariectomy done in England was not done on an animal, but on a human being.

9098. I understand, then, on the whole, that although you do not think that experiments on animals have advanced our knowledge in a useful direction very largely, still you do think that they have been useful to a certain extent?—They have advanced, without doubt, our diagnostic knowledge.

9099. Therefore, you would not be inclined entirely to abolish such experimentation?—I have already said

that, on the whole, what I am inclined to is a considerable restriction of vivisectional investigations.

9100. (Dr. Wilson.) Reference has been made to experimental work in physiology. Would you be in favour of the illustration of physiological lectures by experimentation on animals?—I am afraid that is a moot point, upon which I have not made up my mind. I have had no actual practical experience of physiological teaching, and I should be sorry to commit myself definitely on that problem.

9101. (Chairman.) There is just one question I should like to ask you. You said that if the homoeopaths had had laboratories and sufficient funds for carrying on investigations, they might have made as many discoveries as have been made in other laboratories where experiments on animals are conducted. What would be the nature of the experiments, on the principles that you have spoken of, in such a laboratory?—Experiments on human beings and the physical examination, so far as practicable, of the various system and organs of the human beings while the experiments are going on.

9102. You would not want laboratories for that, would you?—You want a laboratory for examining the blood, the urine, the perspirations, excreta, and so on.

9103. That is a very simple thing; that would be more in the nature of a consulting room?—No, the blood examinations made nowadays are most complicated and expensive businesses.

9104. I did not understand what you meant by saying that laboratories were required for your homoeopathic investigations; but I see now what you mean?—They would have to be as extensive and as well equipped as those in use in the present day.

TWENTY-FIRST DAY.

Wednesday, 5th June 1907.

PRESENT:

The Right Hon. The Viscount SELBY (Chairman).

Colonel the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.

Sir W. S. CHURCH, Bart., K.C.B., M.D.

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.

Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.

Mr. W. H. GASKELL, M.D., F.R.S.

Mr. J. TOMKINSON, M.P.

Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (Secretary).

Miss L. LIND-AF-HAGEBY, recalled; and further Examined.

Miss L. Lind-af-Hageby. 9105. (Sir Mackenzie Chalmers.) I want to ask you a few questions, please. I think you described yourself, in answer to one question, as a lecturer and writer?—Yes.

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9106. Do you lecture and write on any subject besides vivisection?—I have written and lectured on a great many subjects—social, educational, and literary subjects. My articles have generally appeared in the Swedish press and magazines.

9107. In England are you working chiefly for anti-vivisection?—For the last five years I have exclusively worked for anti-vivisection.

9108. For the last five years you have given yourself up to it?—Yes, for seven years I have been most keenly interested in it, but for the last five years I have given myself exclusively to it.

9109. As I understand, you do that as a labour of love and without remuneration?—Yes.

9110. Who publishes your books, are they done by yourself or a society?—If you mean my books on vivisection, I have only written one book, "The Shambles of Science."

9111. Only that one?—Only that one, together with Miss Schartau; it was first published by Mr. Ernest Bell. After the trial, when Mr. Bell withdrew it, it was brought out by Miss Schartau and myself.

9112. As publishers?—As publishers.

9113. Not by any society?—No.

9114. Have you seen any experiments, except lecture experiments? Have you seen any research experiments?—No, I have not seen any research experiments, except, perhaps, after demonstrations before students. Sometimes it happened that one or two physiologists remained, and that some special inquiry was settled by one or two experiments.

9115. But everything you have seen yourself has been in connection with physiological lectures?—With physiological demonstrations. I have visited some physiological laboratories, and seen the effects of research experiments; if I may put it in that way.

9116. You have seen the animals afterwards?—Yes.

9117. Have you ever found any of those animals suffering?—At the Pasteur Institute I have found animals suffering.

9118. But there they are experimenting on rabies and other matters?—They carry on very many experiments of all kinds.

9119. I was asking about England. Have you visited any physiological laboratories in England, and seen the animals afterwards?—I have visited the Brown Institute, where I saw a number of fox terriers intended

for experiment, and I was not very pleased with the way they were kept. I did not consider it very humane. But I have not seen any animals after they have been used for private research.

9120. May I ask how many actual experiments you have seen in England?—We considered in 1903, when we counted over 100 lectures that we had attended, that we had seen demonstrations at about 50.

9121. Where were those lectures, please?—Some lectures were at the Imperial Institute, among them a course of lectures by Dr. Pembrey on animal heat. At the same Institute another course of lectures on the sources of animal energy by Professor Starling; on the circulation by Dr. Brodie; and on the chemistry of muscle and nerve by Professor Halliburton; on the action of anæsthetics and narcotics by Dr. A. D. Waller. Those at University College were on the mechanism of the secretory process by Dr. Bayliss; those at King's College on pancreatic secretion by Professor Starling; and on co-ordination by Dr. F. W. Mott. Then we attended all Dr. Brodie's lectures at the London School of Medicine for Women.

9122. You have kindly given us the names of the demonstrators?—I have given them now.

9123. The only one, I think, whom we have had before us is Professor Starling. Have you read his evidence?—Yes, I have.

9124. Do you agree with it?—I am afraid I do not.

9125. At the time you attended these lectures, may I ask whether you went simply to see what would happen, or whether you went having already made up your mind that vivisection was wrong and cruel?—I, of course, had made up my mind, as I think I explained to Lord Selby, in answer to one of the first questions put to me; but at the same time, I was honestly open to conviction—both Miss Schartau and I were. We started attending these lectures with a sort of idea that anti-vivisection had exaggerated, and we did not start attending these lectures with any fixed idea to write a book or to come out with a public accusation.

9126. That is what I wanted to know, rather?—No, not at all; and, if we had, perhaps sometimes we should have observed details more than we did. I have noticed in reading through my notes, how clear it is that there was no such intention at all at first. It was only after considerable experience that we felt we would like to write about it.

9127. May I put it in this way. If you had not found what you thought was a certain amount of horrors, would you have been disappointed?—No, we should have been very glad.

9128. In the case of all these experiments the animal according to law, had to be anæsthetised?—Yes.

9129. And it had to be killed before it recovered?—Yes.

9130. In your opinion in how many cases was the anæsthetic insufficient?—That question could best be answered by a reference to the "Shambles of Science." I could not at this moment say how many experiments are described there, but I have already pointed out that there are two which describe the efforts to produce complete anæsthesia, especially "A Grand Demonstration," described at the end of the book.

9131. We will come to that presently; but in two cases specially you thought that the animal was not properly anæsthetised?—I should not say that. In two cases we thought efforts were made to produce complete anæsthesia, and those cases are recorded faithfully in the book. In the majority of cases we did not get that impression, and that is also recorded in the "Shambles of Science."

9132. Have you made any independent study on anæsthetics?—I had previously attended Dr. Waller's course of lectures on the action of anæsthetics and narcotics in order to get efficiency on that subject. I have just heard—I heard it before the Commission the last time—that he is considered a great authority on the subject. I have also attended operations on human beings and seen their administration.

9133. You have seen many operations on human beings?—Yes. I have also had chloroform myself and other anæsthetics.

9134. Perhaps I may ask you when you had chloroform yourself were you conscious?—No, I was not.

9135. You felt no pain?—No, I felt no pain.

9136. Have you seen the evidence of Mr. Morris, the

President of the Royal College of Surgeons?—I have read it through, but rather carelessly.

9137. I merely wanted to ask you generally. What he told us was that when an anæsthetic is given, the power of feeling pain disappears before the power of motion; and he was talking of a long experience as a surgeon on human beings. Do you agree with that?—I do not think that is always so. I think there are cases, and I have here an extract from Professor Flint, who is an authority, to that effect in his "Handbook of Physiology," showing that a patient may actually feel the operation under light anæsthesia, and yet at the time of regaining consciousness perfectly may forget that he has felt the pain. He says, "Voluntary movements and cries are observed in persons subjected to painful surgical operations, when incompletely under the influence of an anæsthetic, concerning the character of which there can be no doubt. The movements are voluntary, and the cries are evidence of the acute perception of pain, but such patients have no recollection of any painful sensation. So far as can be judged from what is known of the encephalic centres, the pain under these conditions is perceived by some nerve-centre, probably in the pons varolii, but the impression is not conveyed to the cerebrum, and is not recorded by the memory" (p. 578). That is his view.

9138. That raises the point whether there is really pain in a sentient animal or only what you may call a local feeling of pain?—This would be a sensation of pain at the time, though the brain is unable to record it afterwards, to remember it. I have felt the same once myself, when I had nitrous oxide. I had had a tooth taken out some years previously by an incompetent dentist, and I had 11 tiny pieces left in my jaw which had to be extracted afterwards. I felt all the pain in a sort of way whilst this was being done, but when I recovered I had a faint recollection only that I had felt pain, and had heard what the dentist and anæsthetist said.

9139. Then your brain was not under complete anæsthesia?—It was partly.

9140. Mr. Morris's evidence was to this effect, that you might have a person really, as he believed, completely anæsthetised, but that in certain operations, especially touching certain nerves, you might have movements and cries and yet no pain?—That is said, I should think, not only by Mr. Morris, but it has been said by a number of surgeons. The testimony is always that the patient does not remember anything afterwards; but I think I have answered that, in giving my point of view, and in quoting Professor Flint's opinion. But I think that all anæsthetists are agreed that in perfect anæsthesia the body should be limp and motionless.

9141. Would you depend upon the nature of the anæsthetic used or not?—No, I do not think so. I presume that we are talking of ether and chloroform now.

9142. Or an overdose of morphia would have the same effect?—I do not think an overdose of morphia can be classed in the same category with an anæsthetic.

9143. Then you rather disagree with some scientific evidence which we have had?—Yes, I think I do.

9144. Then Mr. Morris further went on to say that to some extent consciousness and certainly power of movement came back before sensitiveness to pain. Would you agree with that?—I do not agree that that is always the case. I have a good deal of evidence which could be brought to show that that is not always the case. It may be the case, but not always.

9145. What I was coming to really is this: In certain animals you observed, as you have said, that there were movements which looked like the movements of an animal in suffering, from which you inferred that the animals were suffering?—Yes.

9146. Do not you think it is possible that those were merely reflex movements; that you may have cries and you may have movements without any real suffering?—I do not think so. I think those cases were cases of returning consciousness. At all events, it is not an ideal condition that the animal should move or cry; and as we can only go by signs, nobody can say, except by signs, whether the animal or person is suffering pain.

9147. Do you agree or not with some evidence we have had that in cutting certain nerves you always get a nervous contraction, although the animal is

Miss L. Lind-
af-Hageby.

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Miss L. Lind af-Hageby. absolutely under anæsthesia?—Yes, you may get a nervous contraction; but that is quite different from the purposive movement which I have seen in some animals, notably in the so-called "brown dog," or the movements that I saw in the dog that I described under the title of the "Troublesome Dog." That is not a mere nerve contraction.

9148. Now I want to ask you, generally, have you seen Dr. Thane's observations on your book?—I have now.

9149. Having read them, do you still adhere to your statements, or do you agree to an extent with Dr. Thane?—I still adhere altogether to my statements in "Shambles of Science." I think Dr. Thane's criticism touches minor points, but leaves the book on the whole very much untouched.

9150. In what respect does it leave it untouched—generally, I mean to say. You say that it touches on some minor points?—For instance, he says: "The accounts given of the proceedings themselves are generally fairly correct."

9151. Yes; it is the inferences which you draw that I think he objects to?—Yes, I suppose he does.

9152. Which, he says, are not justified by the facts observed—that is his point?—He says: "The accounts given of the proceedings themselves are generally fairly correct."

9153. But that you misinterpreted what you saw?—He objects to the "tone" in which the work is written, and to the suggestion of "dishonesty and dishonourable conduct."

9154. Do you really say that there was dishonesty and dishonourable conduct in what you saw among the lecturers there?—I would not define it as "dishonest." I do not think that word is used throughout the book—or "dishonourable conduct."

9155. Do not you think that these men who were experimenting every day are capable of administering an anæsthetic properly?—I cannot pronounce an opinion as to whether they were capable or not. I can only speak to what I saw; that I did not think they did produce proper anæsthesia.

9156. Did you ever point out to any demonstrators at the time that the animal was not properly under anæsthesia?—No, I did not.

9157. Do not you think it would have been fairer, if you thought that the animal had been suffering, to call the experimenter's attention to it, and say: "Now, I saw movements which I thought indicated consciousness"?—I had experience of what happened when that was done once. There was a cat struggling very much at one of Dr. Bayliss's lectures, and a girl who was there said: "I am sure they have forgotten to give an anæsthetic." She told a student, a man, to go out and tell the lecturer that he had forgotten to anæsthetise this animal; and I am very pleased to see that Dr. Thane does not deny that this cat moved on the operating board before the lecture, or that Dr. Bayliss was fetched in consequence—it is in "The Struggling Cat."

9158. Where does Dr. Thane say that?—In his observations on "The Struggling Cat," page 6. Dr. Bayliss has evidently acknowledged that the cat began to move when he had gone out of the room to get his notes. Then, of course, his account is different from ours. He says that he came in and turned a tap.

9159. Yes?—Whereas we say that he came in and tightened the strings, and went out again, smoking his pipe, I believe—there is no objection to his smoking his pipe.

9160. Did he not also turn a tap?—No.

9161. Are you sure?—I am positive that he did not turn a tap.

9162. You know how the anæsthetic is administered there—from another room?—Yes?

9163. You know that the bottle is in another room?—Yes?

9164. With a pipe underneath the floor?—Yes. That is not the argument on this occasion. Dr. Bayliss says there was natural respiration. He admits that upon this occasion there was no artificial respiration, and consequently the anæsthetic could not come from another room. He says: "The anæsthetic was given from a Woulff's bottle with natural respiration." That would be quite a different sort of apparatus, and we did not see a Woulff's bottle (I know a Woulff's bottle quite

well) upon that occasion, nor did we see Dr. Bayliss turn any tap. Moreover, the cat was placed in the corner of the room when we came in, on its little operating board, and afterwards it was moved on to the large table or counter before the students, and there was no bottle or anæsthetising apparatus moved with it.

9165. When the cat began to move, do you think there was any consciousness, or was it simply the reflex movements before returning consciousness came on?—I should say that it was entirely conscious. Dr. Bayliss says it moved; he does not deny that it moved.

9166. You think that that cat never had any anæsthetic?—Dr. Bayliss acknowledged later on that he had been experimenting on this cat earlier in the afternoon—that cat had been there a long time—and then he had gone out. And even supposing there had been the apparatus there, the cat was lying there all alone, and nobody attending to it. I think that is a strong point.

9167. It depends upon the nature of the apparatus. In some of these laboratories there is an automatic apparatus?—But Dr. Bayliss says it was not an automatic apparatus on this occasion. He says there was natural respiration.

9168. May I just read to you what Dr. Thane says, and ask you if you agree?—Yes.

9169. "This was an experiment on the production of heat in the sub-maxillary gland. The anæsthetic was being given from a Woulff's bottle with natural respiration. The lecturer left the theatre to get his notes." How long was he away?—I cannot say, because when we came into the room there was no lecturer there. I think we came first of all the students. There was only the struggling cat when we went into the room, and it was struggling very much when—

9170. "The cat began to move"?—Yes, Dr. Bayliss says "the cat began to move," but as Dr. Bayliss was not there when we entered, and as he acknowledges that he was not there when we entered, he cannot possibly know how much or how little that cat moved.

9171. But he can know how long he was away?—Yes, but he cannot know the amount of the struggles.

9172. "A student fetched him, and he came at once, and gave more anæsthetic"?—Yes, he came in, but I do not agree that he gave more anæsthetic.

9173. Have you made any inquiry from him about it?—Naturally not.

9174. "This required only the turning of a tap; possibly the reporter mistook the turning of the tap for tightening the strings"?—It is very strange. I cannot understand how it would be possible with natural respiration to produce more anæsthesia merely by turning a tap. It would be possible with artificial respiration when the air is being pumped continually from a pump by turning a tap. It is altogether beyond me, and I cannot find any explanation of such a thing.

9175. (*Colonel Lockwood.*) He gives the explanation Question 1767, in answer to Mr. Tomkinson?—Yes, I understand that it is possible with artificial respiration, but not with natural respiration.

9176. (*Dr. Wilson.*) Was there a tube in the trachea?—No, there was no tube in the trachea.

9177. (*Sir Mackenzie Chalmers.*) There was no tube in the trachea?—No.

9178. How was the anæsthetic applied in this case?—I saw no anæsthetic applied, so that I could not answer that question. Whether Dr. Bayliss had applied an anæsthetic to that animal before we came into the room I cannot say, but I do speak to this: That when we came in, and it was struggling in a corner of the room, there was no anæsthetic applied.

9179. Was Dr. Bayliss examined at the trial? Was that one of the cases at the trial?—No, that case was never touched upon.

9180. Then Dr. Bayliss says: "I see that the lady student, who is reported as saying, 'I am sure they have forgotten to give the cat an anæsthetic' has been advertised for. I have not heard whether she has been found"?—I do not know who advertised for her. I did not advertise for her.

9181. She was a stranger to you?—Yes.

9182. (*Chairman.*) Did I correctly understand you to say, in answer to a previous question, that you had never spoken to the lecturer, and that that was in

consequence, more or less, of what had happened when a lady student had spoken to him?—Yes, I saw the effect.

9183. What happened to the lady student?

9184. (*Sir Mackenzie Chalmers.*) She seems to have fetched Dr. Bayliss?—She sent for Dr. Bayliss. I meant to infer that it had very little effect that Dr. Bayliss knew of the distress of the young girl, who seemed very sensitive; in fact, she began to cry.

9185. (*Chairman.*) I thought the suggestion was that Dr. Bayliss treated her in some way that was extremely discouraging by his manner?—No, I simply meant that he took very little notice of it.

9186. (*Sir Mackenzie Chalmers.*) Are we to take it from you that your impression is that that cat, instead of having more anæsthetic given to it when it began to move, was simply tied down tighter?—That was my impression.

9187. That we must inquire into. After the experimenter's attention had been called to the fact, after somebody had gone out and told him that the cat was moving, instead of increasing or giving any anæsthetic, he merely tied down the cat more tightly?—He pulled the strings at the four corners and tightened them.

9188. What happened after that?—The cat was lying on its back.

9189. What happened after that to that cat?—After that the lecture began. Dr. Bayliss came in and explained that they were rather short of dogs in the laboratory, and this afternoon he could not have a dog. He also said that he had experimented on this cat earlier in the afternoon. We were sitting behind this girl who had shown so much distress, and we saw that it distressed her still further. She was then spoken to by a lady student at University College, who made light of it. I cannot recollect what actually was said, but she laughed, and probably reassured the girl that the cat was not suffering.

9190. How long did the lecture continue?—The lecture continued, I suppose, an hour and ten minutes—a little more.

9191. Was the cat killed at the end of the lecture?—No.

9192. No?—No, the cat was taken out before the hour was over.

9193. You do not know what happened to it?—No.

9194. Did the cat again make any motion; was there any sign of pain that you saw in the cat after what was done to it?—Yes, the cat struggled.

9195. Struggled still?—Struggled throughout the time we saw it; but, as often happens when you see an animal struggling, it did not show the same forcible movements—movements of the same strength—as it did at the beginning, which accounted for the girl who was so distressed becoming more calm afterwards. But it did move all the time distinctly.

9196. How far were you off?—I was very near; I was always very near. And Miss Schartau and I always walked up to the animal before leaving whenever the animal was not taken out before the lecture was over, and looked at it. Sometimes we spoke to the laboratory attendant.

9197. There we have an issue of facts which we must inquire into.

9198. (*Sir William Collins.*) Was Dr. Thane present at any of these experiments?—No, he was not.

9199. (*Sir Mackenzie Chalmers.*) Anyone can go in, I suppose; anyone about the place?—I think any medical man is allowed to go in.

9200. There are some other points which I dare say other members of the Commission will ask you about the investigation made by Dr. Thane. I want to ask you, generally, do you object to all experiments on animals? How far do you go in that?—Yes, I object to all experiments on animals.

9201. Irrespective of whether pain is caused or not?—I do not believe that vivisection can be made painless.

9202. You do not believe in complete anæsthesia. I want to take you to this case first. Supposing that an animal is properly anæsthetised, and is killed before the experiment concludes, before the anæsthesia passes off, do you object to that?—Supposing an animal could be properly anæsthetised, which I deny for vivisection

operations as a rule, but supposing it could be, and supposing it were killed before it recovers consciousness, I should not object to that as an anti-vivisectionist; but I should object to it as a general humanitarian and for other reasons, but not merely as an anti-vivisectionist.

9203. You do not object that the animals should be killed when they are unnecessary or for various reasons when they are a nuisance?—I object to the sacrifice of life.

9204. May I ask you this, for instance? You know that 20,000 dogs are killed every year in the Dogs' Home in London?—Yes.

9205. What I may call superfluous dogs?—Yes?

9206. Do you object to that?—Under present circumstances, and as the world is, I think it is a regrettable necessity, and as vivisection exists it is far better for the dogs to be protected and killed in that painless way. But I do not agree with what Professor Starling said before this Commission, that this is done to rid the streets of "the danger of stray dogs." I think it is done from a humane point of view.

9207. I want to know whether you object to that or not?—No, not at present.

9208. Then if a dog which had to be destroyed could be properly anæsthetised, do you think there would be any objection to increasing knowledge by means of its death?—I should think there is. Because I know that vivisection anæsthesia is always unreliable. I would trust a veterinary surgeon of skill and capacity to operate on an animal painlessly if he sought to do so; but I would never trust a vivisectioner in that capacity. Moreover, it must be remembered that vivisection is not only a few experiments on so-called "anæsthetised" animals.

9209. I will take you to the others afterwards. I want, first of all, to know whether you think it is wrong and objectionable, where an animal is to be killed, and assuming that you can give it complete anæsthesia, to use it for experimental purposes?—Yes; for the reasons I have stated.

9210. Then what it comes to is this: That you would not trust any lecturer who performs experiments, or any scientific man, to give complete anæsthesia?—No, from my knowledge of what vivisection is I cannot trust vivisectioners with regard to complete anæsthesia.

9211. You do not agree with the experimenters when they say that in the case of an animal they give much heavier doses of anæsthetic than they would in the case of a human being, because it is somewhat important that the human being should recover from the operation?—I brought evidence last time to show that that is not so. I brought a considerable amount of evidence with regard to light anæsthesia to contradict that entirely, and also to show that fatal doses are not generally employed.

9212. Your opinion is that people who are performing what I may call vivisectional experiments prefer light anæsthesia, and do not give complete anæsthesia?—Yes; and in some cases they prefer no anæsthesia at all.

9213. Can you give me any case of that?—I said last time that I could give the Commission a considerable number.

9214. I have it all before me in your evidence. Can you give me any case in England?—There is a statement in a book that I referred to by Sir Lauder Brunton, saying that experiments with drugs cannot be made under anæsthesia.

9215. I have not come to those yet; those are what you may call feeding experiments?—Drug experiments, poison experiments; they cannot be made under anæsthetics, because the anæsthetic would interfere with the result.

9216. I quite agree with that. I want to ask you this. How far does your objection go on moral grounds to vaccination, the using of calves for the preparation of vaccine?—I object to vaccination for the sake both of the infant and of the calf.

9217. Have you ever seen a bad case of smallpox?—No, I have not.

9218. It is a pleasure some of us have had?—I have seen scars.

9219. I may take it therefore that if you object to vaccination you object to the whole principle of what

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9221. Are you not generalising too much from a single case?—I think not. I have plenty of evidence before me of very serious complications following on the use of the antitoxin. I have also a German book, written only for the medical profession, which gives no less than 13 diseases as the result of the use of diphtheria antitoxin. It is not an ideal remedy.

9222. No, I do not suppose that any remedy is ideal. But if there is a great concurrence of medical opinion that life is saved by using antitoxin in many cases in which it would be lost without, do you think it is unjustifiable to use it?—There is a considerable amount of medical opinion against the use of anti-toxin, both in this country and in others; I could quote many names; but even supposing that the majority of the medical profession claim that diphtheria anti-toxin is an invaluable remedy, that does not prove that it really is so. Medical history is full of discarded theories and superstitions that have gone. I have brought quotations from three works on medical history, but I will not trouble the Commission with them, in which it is specially emphasised that medical history is not least interesting because it always teaches you what not to do.

9223. I take it, then, that you object absolutely to the use of antitoxin?—Yes.

9224. If you were convinced that it was useful, would you still think it wrong?—I would, because I know from personal experience, from having had diphtheria, that there are other means of fighting the disease more natural and followed by less objectionable results.

9225. We had some very interesting evidence the other day about snake bite, and I want to ask you this. Do you think it is right to use animals in order to experiment on a new remedy for snake bite?—I should think it would be better to use other means. I do not believe in the scientific value of these experiments.

9226. Did you look at Colonel Rogers's evidence of the other day; have you seen it?—I am afraid I have not read it very carefully.

9227. What he told us was that he is able now to save a good many lives of people or animals who have been bitten by snakes, by scarifying the point and putting in permanganate of potash, and that he discovered that by a series of experiments, on, say, 20 or 30 unfortunate animals, it may have the effect of saving many hundreds of human lives and animal lives. Do you think it is justifiable or not?—There is no harm in using the remedy itself.

9228. Do you think he was justified in using animals to experiment on?—I think it would be better if he had used himself.

9229. The poor man has not got 20 or 30 lives. I am afraid he would have died 20 or 30 times. However, that is your answer. You think he ought not to have used animals?—Yes, I think, as in the case of other people who are devoted to either scientific or social or ideal questions of any kind, we all know that the highest thing is to sacrifice yourself, not others.

9230. Do you suggest that all experiments should be made first on the experimenter?—Certainly. I have no objection to vivisection, provided that the vivisectors experiment on themselves, and nobody could have. They are perfectly entitled to their opinions.

9231. A good many experiments have been tried by doctors on themselves?—A very small percentage of the whole.

9232. Do you draw any distinction yourself between causing pain to an animal in sport, and causing pain to an animal with the object of finding cures?—Certainly. I object to what is called blood sports, most seriously.

9233. All sports?—And I consider the anti-vivisectionist who does not object to them most illogical. I consider the anti-vivisectionist who takes pleasure in blood sports, which cause pain to animals, most illogical. In that view I believe I am not supported by

the total number of anti-vivisectionists; but that is my opinion.

9234. I only wanted to know how far you go. Take, for instance, this case. Have you ever seen a good day's rabbit shooting?—No, but I can imagine what it is. I should think it hateful.

9235. But in what I may call your public agitation you do not include sports?—There must be a division of labour for everybody. I am a thorough humanitarian, so far as I can possibly be so. I have written against sport a good deal in Sweden.

9236. I do not know whether it is fair to ask you, which do you think is the most wrong: in rabbit shooting, to maim an unfortunate rabbit which crawls into a hole to die, or to perform an experiment on a rabbit which you think and hope may result in benefit to human beings?—That all depends on the way in which the two actions are done. My personal opinion would be that the vivisector (though I am a very strong anti-vivisectionist) who honestly thinks he works for the good of humanity, or the good of science, and who honestly tries to render his experiments painless, is on a higher moral scale than the man who seeks his pleasure from maiming and hurting animals.

9237. His pleasure is not in maiming and hurting; but in the pursuit of his pleasure it is incidental?—That alters the case a little; it depends on how far he is conscious of the maiming and hurting of animals, of course.

9238. (*Chairman.*) It is wicked in a bad shot, and not wicked in a good shot, you mean?—I do not mean exactly that. It depends very much upon the man's attitude of mind, does it not?

9239. (*Sir Mackenzie Chalmers.*) You agree that new remedies and new drugs must be tried, do you not?—Yes; but I think that the development of medicine is rather the decline of drugging; I think that is one of the signs of the time.

9240. You think, if I may say so, that it is wasted labour to try new remedies and new drugs?—I think it would be far better if we turned our attention to the preservation of health, and in a minor way to the prevention of disease, by strengthening the general health.

9241. So that you do not think it is justifiable, if a new remedy is brought in, to try it first on an animal, and then on a human being?—No, especially as experiments on animals are so useless. Professor Starling has told this Commission that "the final experiment must always be on man," and we have many statements against the danger of applying the results of experiments on the lower animals directly in the sick room.

9242. I may take it that you object to every experiment of every sort or kind for the purpose of advancing knowledge, believing that it is both wrong and useless?—Yes. There are experiments, of course, which I might quote which are altogether different from the ordinary experiments which are performed now. There is, for instance, one experiment given in Beddard's "Practical Physiology" (p. 151), with the object of studying the intestinal movements in a live cat under the Röntgen Rays. The cat is taken gently, it is not even anaesthetised, there is not even that discomfort to the cat, the movements of the intestines are studied by means of the Röntgen Rays, and it is specially said that the cat must be treated gently, as otherwise abnormal conditions will be set up. Nobody can object to an experiment of that kind, either on a human being or an animal. There is a certain class of physiological apparatus which is being used nowadays for experiments on human beings, by physiologists on themselves, and those experiments are not at all objectionable. I have brought plenty of pictures of that kind of apparatus, if the Commission care to see them. Now you can study the pulse, the blood pressure, and a great many physiological questions by means of this new physiological apparatus for man. And that is developing every day; and I believe that if humanitarian pressure is strong enough, physiologists will more and more have to apply this painless kind of apparatus to experiments on animals as well as on human beings.

9243. I take it that the general object of your evidence is that experiments on animals should be disallowed by law?—Yes.

9244. Without exception?—Yes.

9245. Assuming that that cannot be done, have you any suggestions to make as to how the existing law should be modified in any way?—I have. I would first of all suggest that demonstration experiments before students should be forbidden. Both in the "Shambles of Science," and later, in Sir Thornley Stoker's evidence before this Commission, it has been brought out very strongly that such experiments are demoralising; and, after my recent debate with Professor Halliburton a few days ago, where students came in large numbers, and behaved, as the Press characterised it, in a very brutal way, I feel still more strongly that those experiments ought to be discontinued.

9246. Have you any other suggestions to make?—Yes; that the use of morphia should be made illegal as an anæsthetic.

9247. (*Mr. Tomkinson.*) And curare?—Curare should be forbidden altogether.

9248. (*Sir Mackenzie Chalmers.*) Curare should be forbidden altogether?—Yes.

9249. And morphia should be forbidden as an anæsthetic?—Yes.

9250. You do not object to morphia being given previously to an anæsthetic?—No, not to help the induction of anæsthesia simply. But I believe when morphia is given in a fairly large dose it produces a condition which is deceptive, and that therefore, when vivisectioners give a very small dose of a real anæsthetic afterwards, it may be misleading, but I do not think that that could be legislated against. But nobody can object to the use of morphia merely as an induction to anæsthesia.

9251. Have you any other suggestions to make?—Yes; that experiments on all the higher animals, dogs, cats, monkeys, horses, asses, etc., should be forbidden.

9252. With the exception of man, you think all other animals should be excluded?—I do not approve of experiments on men and women, except voluntary ones, and of those only when undertaken for the real good of the men and women—the patients; of course, all life is an experiment, if you extend the word.

9253. I do not want to play with words. Every dose of medicine is an experiment, I suppose?—Yes.

9254. Have you any other suggestion to make?—My whole object is that the Act should be repealed.

9255. Assuming that that is not possible, have you thought of any other course?—Yes, I have another suggestion to make, which I brought out last time, and which I feel very strongly about; that is, that students should not be allowed to experiment on frogs.

9256. Even pithed frogs?—Frogs are now experimented on by unlicensed people.

9257. After death, so to speak?—The question is, what is death? I have not quite finished my evidence on that yet, if I may be allowed to continue.

9258. You mean, that there are certain modes of pithing which, in your opinion, do not destroy consciousness—the sensation of pain?—I consider that a really pithed frog, in which the brain and spinal cord have been totally destroyed, does not feel pain.

9259. But you think that as long as there is any purposive movement the animal does feel pain?—Yes, as long as the spinal cord is intact.

9260. (*Chairman.*) You wish, I think, to add a note to your evidence with regard to Question 7430?—Yes.

9261. I think you wrote to the Secretary, and he suggested that you had better make it part of your evidence to-day?—Yes.

9262. It relates to the pithing of frogs; and perhaps you will just tell us what it is?—There were two notes, in fact three notes that I sent in, which I was asked to give in evidence instead. I was asked at Question 7315 to give a reference to a passage of Claude Bernard. The words which I quoted from Bernard's "Leçons de Physiologie opératoire," are preceded by the following sentence, relating to the one referred to, which I translate:—"The dose which we generally employ is 5 centigrammes, but one can give the dog, without any danger, a stronger dose, even double, of morphine, provided the salt is pure. The animal still remains sensitive, etc." Then I have a few words, if I may add them, on morphia; in fact, just a few sentences. I wish to add in extension of my evidence on morphia last time that the morphia doses generally recommended in text books are not fatal doses (I have a list of such doses here), and also that the survival of animals experimented upon under morphia is proof positive

that the doses given are not always fatal ones. (I have another list of such instances.) Even Professor Starling does not assert in his evidence that all animals die after the so-called fatal doses of morphia to which he refers, but says that in most cases they do so; that was at Questions 3607-8. I have evidence here from Dr. William Murrell's "Manual of Pharmacology and Therapeutics," that even such a strong dose as 37 grains of morphia has been given to a dog without affecting it; and that doses of 17 grains have been given without producing serious symptoms; and Professor Starling mentioned a dose of 15 grains as being the utmost limit of a fatal dose.

9263. What do you mean by the utmost limit of a fatal dose? If more was given it would be fatal, would it not?—Yes; but Professor Starling mentioned before the Commission that a fatal dose would be within one grain and a half and up to 15 grains. I have several extracts here to show that over 30 grains have been proved not to be fatal doses in dogs.

9264. (*Dr. Gaskell.*) Would you mind giving us the size of the dog?

9265. (*Chairman.*) I did not intend to interrupt the examination on this point; it is another point altogether; you were speaking about pithing frogs?—Yes. I can leave the morphia now until I am asked questions upon it; I have very little more to say about this point.

9266. If you have nearly finished it you might as well continue?—I was asked the size of the dog. This is an extract from a "Manual of Pharmacology and Therapeutics," by William Murrell, Lecturer on Pharmacology and Therapeutics to the Westminster Hospital, Examiner in Materia Medica and Pharmacy to the Conjoint Board of the Royal College of Surgeons and the Royal College of Physicians, London, 1896. Murrell says that "Recent observations show that dogs are but slightly susceptible to the action of morphia. H. C. Wood and David Cerna gave a dog weighing 15½ kilo. a dose of 2·4 gram (37 grains) of morphia without affecting its respiration. Caldwell gave a dog 17 grains without producing serious symptoms; whilst to a number of other dogs he gave 12 grains hypodermically, with no bad effect" (p. 339). Then Claude Bernard refers to a dose of 30 grains, that is, twice the dose to which Professor Starling refers, and this enormous dose produced great excitement and convulsions of great violence. After four hours the dog was frightened, lay stretched out in a corner, but could walk. The dog was not dead the following day. That is from Claude Bernard's "Leçons sur les Anesthésiques et sur l'Asphyxie" (p. 201).

9267. Without any effect on what, does he say?—He gave this enormous dose. It produced great excitement and convulsions of great violence. "Without effect" was Murrell's quotation, which I quoted before.

9268. Without any effect on what?—On respiration.

9269. It was confined to that?—In both quotations it is shown that the dose was not fatal.

9270. (*Sir William Collins.*) Did the dog recover?—The dog recovered—both dogs recovered.

9271. (*Mr. Tomkinson.*) The dog did not lose consciousness?—Not in these two cases. Shall I go on to the next note that I have?

9272. (*Chairman.*) Yes, please; that is about the frog?—Yes. At Question 7430 I was asked to supply a note of the physiologists who attach different meanings to the word "pith." Thorough pithing of a frog is described in the following way by Dr. Augustus D. Waller in his "Exercises in Practical Physiology," part III., page 44: "Insert one blade of a strong pair of scissors into the mouth, as far back as it will go, and cut off the top of the head. . . . Thoroughly destroy the brain, bulb, and cord by means of a stout wire pushed several times down the spinal canal. The frog is now thoroughly pithed, and should remain permanently flaccid and motionless; any movements of the limbs signify that the spinal cord has not been thoroughly destroyed." In "Essentials of Experimental Physiology," Dr. Brodie gives directions for thorough pithing, on page 16, as well as advice for incomplete pithing on pages 49 and 214. Professor Stirling includes mere decerebration as well as destruction of the brain and spinal cord, under the word "pith" (pages 219, 377, "Outlines of Practical Physiology"). In "Practical Physiology," by Beddard, Edkins, Hill, etc., the word "pith" is used in its proper and inclusive sense (page 14). Professor Busch says under the head.

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ing: "Pith a frog. This ordinarily means destroy the brain" (Laboratory Manual of Physiology, page 10). In his evidence before this Commission Professor Starling speaks of a brainless frog as a "pithed frog" (3467), and in the pamphlet, with which I have been favoured, Dr. Thane commits the same error. In the above-mentioned book, Dr. Waller states that "For certain experiments it is necessary only to decerebrate the frog—i.e., to destroy only the hemispheres, or the hemispheres and optic lobes. In this case the scissor cut must not be made quite so far back, and the brain carefully removed." That ends my note on that question. May I ask a question? I have also added a list of those experiments which are recommended in text books to be made on only decerebrated frogs, but if the Commission do not wish to hear it—

9273. I think we had better go on with the examination now; I interrupted you before?—I was asked to give another reference.

9274. If it is only a reference, you might as well give it now?—I have a little comment upon it; so perhaps it had better wait.

9275. (Mr. Ram.) Will you tell me, please, what is your attitude with regard to the Act that we are acting under to-day? Would you entirely repeal it?—I would repeal it and substitute another Act prohibiting vivisection. I would not only repeal it—

9276. You would enact in lieu of it an Act saying that vivisection was to be wholly illegal?—Yes.

9277. How would you define vivisection?—I would define vivisection as experiments calculated to inflict pain on living animals for scientific purposes.

9278. You would limit it to scientific purposes?—I should like to prohibit all painful experiments, scientific and otherwise; but if we are only dealing with scientific vivisection, I dare say it would have to have the word scientific.

9279. Would that not leave a very wide field open by which great pain could be inflicted on a large number of animals, not for scientific purposes, but for purposes which might be considered less worthy than scientific purposes?—Certainly, there is a great deal of other cruelty which should be stopped by other Acts, if possible.

9280. Would you stop all of it?—Personally, I would stop by legal enactment as much as possible, but I have not the slightest hope that in my lifetime all such cruelties will be stopped.

9281. Take the farmyard operations, which have been spoken to by other witnesses; would you stop them?—I think that it should at least be enacted by law that those farmyard operations are not permissible without anaesthetics.

9282. You would give anaesthetics in every case?—Yes.

9283. Every colt, every lamb, and every young animal of every sort?—I think it is most cruel to perform that operation without anaesthetics.

9284. And you would make it illegal to perform any one of those operations on any animal, at any age, without anaesthetics?—Yes, certainly.

9285. Meantime, if the Act is to continue, you make the suggestions that you gave to Sir Mackenzie Chalmers just now?—Yes.

9286. And you do not wish to add to them?—No.

9287. Do you know the practice in America?—I know something of the practice in America.

9288. Do you know that there is no restriction by law there of vivisection?—I know, nor is there in my own country, Sweden, or in Germany or France; and I know something of what is being done in those countries.

9289. Are the practices in those countries worse than they are here to-day?—I consider that they are. I consider that they are much worse in countries where every medical student has the right to vivisection any animal in whatever way he pleases.

9290. Therefore the Act that we are acting under to-day is of advantage in making things better than they are in countries where there is no such Act?—Certainly, I think so.

9291. Do you know anything of the writings of Dr. Leffingwell, an American doctor?—Yes, I know of him.

9292. I have here a book which was sent to me by

Mr. Sydney Trist, of the office of the "Animals' Guardian." Do you know the "Animals' Guardian"?—Yes.

9293. Do you know the people connected with it?—I know some of them.

9294. Are they anti-vivisectionists?—Yes.

9295. They sent me this book, and I find in this book that what this gentleman, Dr. Leffingwell, desires is that there should be in America exactly the same Act as we have in England?—Yes.

9296. You are aware that that is what he desires?—Yes.

9297. Then I need not trouble you, as you know it, with extracts from his book. Do you agree with him that in America, and perhaps in Sweden, to have such an Act as we have would be a very great step in advance?—I quite agree; because here, at least, you have an opportunity of finding out something of what is being done, but you cannot in America or Sweden.

9298. And here, too, if any illegal case is brought to the knowledge of the Home Office proceedings can be taken against a person doing an illegal act?—Yes, it should be so.

9299. And inspectors exist for the purpose of finding out whether there are such illegal practices or not?—Yes, they are supposed to.

9300. In how many of the experiments which you have enumerated in the "Shambles of Science" do you consider the Act had been broken?—I think I have been asked that question before.

9301. If you have answered it before I am sorry I have asked you?—I think I was asked the question this morning.

9302. At any rate, I need not tie you down to ones and twos and threes. You do think that in several of those experiments, if not in all, there have been infringements of the Act?—I think that the Act, as it stands now, admits of a great deal of cruelty. There is no effective control over vivisection as the Act stands to-day.

9303. Whether there is control or not, in your opinion some one or more of these cases were infringements of the Act?—I believe, on the whole, it is possible to do a great many cruel things under the Act.

9304. Did any of these cases that you thought cruel strike you as being infringements of the Act?—That is a question that is rather difficult to settle. If a man performs an experiment under licence or under Certificate C, before students, which licence or certificate lays down that the animal should be anaesthetised, and if he employs an anaesthetic that is no real anaesthetic, or if he gives the anaesthetic in a manner that is not proper and is not conducive to total abolition of consciousness, is that infringing the Act? It comes to that question.

9305. (Colonel Lockwood.) Like "The Struggling Cat"?—Yes.

9306. (Mr. Ram.) You thought, then, that some, at any rate, of these experiments that you saw were conducted in the way you describe?—Yes.

9307. Did you make any representation to any Inspector or to the Home Office as to any one of these alleged breaches of the law?—No.

9308. Over what period do all these experiments extend?—The experiments described in this book extend, I believe, over a period of six months; I am not quite sure.

9309. And at no period during that time did you make any representation or any complaint?—I made representations, but not to the Home Office or to the Inspector.

9310. You made them, I think, to the public through this book?—Yes; I think we did.

9311. Did you ever apply to the experimenter to know whether you had misunderstood what was done or to ask for any explanation?—No, we did not.

9312. I will deal with one or two of those cases in a moment; but before I come to them I want to ask one or two general questions. I refer first to your answer to Question 7243. The question asked you was, "May I point out that the train of your argument really seems to involve a belief on your part that they intended to suggest that the anaesthesia should be so slight that the animals might be conscious; because

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it arose in connection with a question of cruelty?" And your answer is this: "My whole evidence is in connection with cruelty"?—Yes.

9313. I take it that that is your position now?—Yes.

9314. Eliminate cruelty and you would not object to vivisection?—But my point of view is that cruelty cannot be eliminated from vivisection.

9315. But take a hypothetical case. Assuming that cruelty could be eliminated, and that there was and could be no suffering on the part of animals, I take it that you would not object to vivisection?—I have already answered that question, too.

9316. Would you mind answering me?—No, I will answer you in the same way: that supposing such an impossible thing were possible as that vivisection could be made painless—are you quoting drug experiments and inoculation and those things, or are you assuming only a surgical case?

9317. I am assuming for the moment a surgical case?—If it were absolutely painless, if it could be made so, and the animal killed immediately after, so that there was absolutely no return of consciousness, I said that I should not object to it merely as an anti-vivisectionist, but I should object to it from a general humanitarian point of view.

9318. I am afraid I do not quite follow that. You as an anti-vivisectionist would not object because there would be no suffering. Will you clear it up?—It is very difficult to answer a hypothetical question.

9319. I will put a question which is not hypothetical. What is the distinction that you draw between the anti-vivisectionist and the humanitarian?—The anti-vivisectionist may be a person who sees very acutely the cruelty and selfishness in vivisection, but has not yet had his eyes opened to other cruelties in the world which may be just as bad, or nearly as bad.

9320. That is your definition of an anti-vivisectionist?—But the general humanitarian is a person who sees other forms of cruelty and selfishness, and is anxious to remedy them, too.

9321. I do not quite follow the distinction?—As a humanitarian, I should object even to the painless vivisection (supposing vivisection could be made painless) of a dog.

9322. Why?—Because, as I have also said in my previous evidence before this Commission, I am not an anti-vivisectionist solely on the ground of pain; I am an anti-vivisectionist on the ground of objecting to the general exploitation of animals.

9323. That is the reason why I referred you to your evidence in which you said "My whole evidence is in connection with cruelty"?—Yes?

9324. Do you wish to modify that?—That I can easily stand by, because my whole evidence had to do with bringing proof that vivisection cannot be made painless.

9325. Do you wish now to go further, and say that you now object to what you call exploiting animals for any purpose?—I am very anxious to be allowed to stick to my position. My position before this Commission is simply that of an anti-vivisectionist, and I am an anti-vivisectionist because I know that vivisection, including drug experimentation, inoculation experimentation that involves keeping an animal alive for a long time, extirpation of organs, and things of that kind, never can or will be made painless. That is my position here.

9326. I am reminded that you said, in answer to Question 7622, "I should object to an experiment on a living animal, supposing it was unconscious . . . because I take my stand against vivisection, not only and entirely on the ground of pain, but on the broader humanitarian ground of objecting to the general exploitation of animals for our use"?—Yes, and I was asked a good many questions last time as to what I considered to be exploitation of animals.

9327. I do not want in the least to go over ground that you have already covered, or to put anything to you which might be an extension of what you said; but do I take it that in your view any pain inflicted on an animal otherwise than for the animal's good is wrong?—I consider it to be so.

9328. How about breaking a horse to harness?—That may be for the animal's good. I consider that the domestication of animals is distinctly for their good,

because it trains their minds and helps their general mental development.

9329. I understand that it is lawful to break a horse to harness with whip and spur, and so on?—I have read a book by a very famous Norwegian trainer of horses who does not do it with whip and spur, but does it in a most humane way; it is a most interesting book.

9330. I wish it was more universal, but I am confining my question to what takes place to-day?—I think that any cruel training of horses and breaking in of horses is most objectionable, but I do not think that the process need be cruel.

9331. Supposing you take the best process that you can conceive, the horse is ultimately broken in, and you make him drag a carriage or a cab in London, not to an excessive extent, you do not injure the horse by doing it, but you make him drag it about contrary to his wishes. Is that right or wrong?—It depends entirely upon the driver, and on the amount of driving that is done with the horse and the amount of load that the horse has to pull.

9332. If it has a moderate load and is driven for an hour or two and is made to go uphill when it would rather not, and is made to go in a cab when it would rather be in the field, is that right or wrong?—I do not think it is cruel to be made to do what you would rather not do. We are all of us made to do what we would rather not do.

9333. Should you call it exploiting the animal?—No, no more than I consider myself exploited by having to give evidence before this Commission. I do not like doing it.

9334. (Chairman.) I think there is a little difference. I do not think you have been compelled to come here; you have come willingly. The horse is compelled to go in a shaft and to be fastened to the pole of an omnibus?—But I am compelled by my own moral feeling with regard to vivisection.

9335. There is a great deal of difference between being compelled by your own moral feeling and being compelled by somebody else?—But a father and mother have to compel their child to do a thing very often both mentally and physically. You would not call that cruelty.

9336. We do not like it applied to us when we grow up.

9337. (Mr. Ram.) What I really want to know is your definition of exploiting animals. It would be very helpful to us when you are telling us what view you wish us to take?—I could go on for days telling the Commission what I consider to be exploitation of animals.

9338. (Chairman.) Then I am sure Mr. Ram will withdraw the question?—It is too broad, I think.

9339. (Mr. Ram.) In mercy to the rest of the Commissioners I will withdraw it. It is a fact, is it not, that in consequence of the exploitation of animals—that is, use being made of animals for the convenience of mankind—thousands of animals lead a very comfortable life which they otherwise would not lead; do you agree?—Some animals lead a very comfortable life, but the majority do not.

9340. Supposing that we did not eat sheep and oxen after they were dead, do you suppose that we should keep them with the utmost comfort for three or four years, feed them in winter, and house them in bad weather?—No, but the journey to the slaughter-house and the present slaughter-house methods are anything but comfortable.

9341. Do you think it is not worth the animal's while to have three or four years of a very happy life because at the end it will be killed by a blow?—I would certainly prefer to be a wild sheep than a domestic sheep.

9342. Supposing that mankind did not exploit animals for their own good—horses, cows, and so on—for use in life and for food after death, how many of those animals do you think we should have in this country?—The supply is generally found in nature to meet the demand, and those who have entered upon this very wide question (and many competent people have done so) have come to the conclusion that supposing we gave up eating sheep and oxen the supply would naturally regulate itself, because at the present moment there is a great deal of artificial breeding. For instance, we do not eat dogs and cats in this

Miss L. Lind. country, and yet that matter can be managed without our being cemeteries for them.

5 June 1907. 9343. Do you agree with me that if it were not for the use of these animals a great many of them would not exist, or there would be many thousands fewer?—They would not exist in the same quantity.

9344. Why should a man keep a horse at all if he never uses it?—I have not said that a man may not keep a horse provided that he uses it in a humane way.

9344a. Why should a man keep a herd of oxen unless he meant ultimately to eat them or to sell them to others for food?—He might keep oxen as the peasant in Sweden keeps them, to plough his soil and draw his carts and to work.

9345. Do you think that oxen kept to draw carts and till the soil in this country would not have a worse time of it than if they are kept for four years and then knocked on the head and eaten?—Very probably they would at the present moment. It depends whether we are arguing from the point of view of greater humanity or lesser humanity than exists at the present moment.

9346. I will pass from that. Now I want to come to your answer to Question 7645. I will read the question if you desire it, but it is rather long, and the answer is what I want to deal with?—I know the point.

9347. You say: "I should not dream of denying that that method"—that is, the method of immunising animals for cattle plague—"has been discovered by experimentation on animals; in fact, that is something that we all know; but the point is whether some other method might not have been employed, and could not have been employed with equal advantage. I believe, on the whole, in natural immunity versus artificial immunity, and I am not at all inclined to admit that the principle is a true or right one." Could you tell me what other method you had in your mind then?—As relating to that special case, I believe I had in my mind the method which has been referred to before the Commission of isolation and slaughter; that, I think, in the first instance. And, secondly, I had in my mind an objection to the whole system of immunising by means of sera, the various substances drawn from an artificially diseased animal, which I do not believe in.

9348. Then what you would do would be to isolate and not to attempt to immunise at all?—I do not believe in those methods of immunisation. I believe that they are unhygienic—dangerous—blood poisoning.

9349. And you would substitute for them simple isolation?—I would substitute for them sanitation first, prevention of disease and isolation. I believe immensely in the natural powers of the blood of slaying bacteria, in the so-called bacterioidal powers of the blood.

9350. You are opposed, then, to the use of any sera at all?—I am, from the hygienic point of view as well as the moral.

9351. Some witnesses have told us, and I want to know whether you agree with them, that they object to the use of sera because it is the importing of a substance from the body of an animal into the body of a human creature; is that your objection?—That would not exactly be my objection. If I am to speak on the subject from the scientific point of view, I object to it because I know that the serum of one animal has the power of destroying the red corpuscles of another animal if the serum be injected into that animal—what is referred to as the globulicidal action of the blood; and that wonderful capacity in one's own blood is a proof that we are not meant to lend and borrow blood from one another, and especially not from different species. I also object to it because all artificial immunity which can possibly be induced by these sera is of very little value compared with that of strengthening these normal and natural powers of the blood which, when we have them, are an absolute protection against bacteria and disease.

9352. Then you do not think that there is some artificial immunity which can be obtained?—In some cases no doubt there is a certain amount of truth underlying the whole system of serum therapy, as, for instance, we know that in the case of measles and certain other diseases if you have an attack you are

liable to be protected for a certain time. But I believe that the principle is wrong and its application is wrong.

9353. May we not hope that further experiments, the principle being right, might show a way to applying it more accurately and more beneficially than at present?—I do not think so. There is a paper by Dr. Brodie on "The Immediate Action of an Intravenous Injection of Blood Serum,"* of which I have brought an extract, but I will not trouble the Commission with it, showing from his experiments with serum that serum is by no means an innocuous substance, but is always, more or less injurious.

9354. Now one or two questions, please, with regard to some of the other cases alluded to in the "Shambles of Science." At page 37 you deal with the incident of the marmot. You speak of a marmot the spinal cord of which had previously been divided?—Yes, I have a short statement on that marmot, which I brought with me last time, in answer to Mr. Thane's statement on the marmot, and perhaps I am allowed to read it now.

9355. (Chairman.) That is just what Mr. Ram is going to elicit from you if you will answer his questions instead of reading your statement.

9356. (Mr. Ram.) It may be that my questions will save your reading?—Yes.

9357. You observe that the statement which you make is, "We once saw a marmot the spinal cord of which had previously been divided"?—Yes.

9358. On that Mr. Thane says: "I knew this marmot well. It belonged to Dr. Pembrey, of Guy's Hospital Medical School. I have often seen it. It was never experimented upon, save for having its temperature taken. It is the only marmot that has been shown to physiologists in this country. To the general regret it died a short time since. I have since examined the body. The spinal cord has not been divided." Do you accept that statement of Mr. Thane's?—I have prepared my answer here.

9359. I will not preclude it if it is necessary, but will you just tell me first do you accept that statement of Mr. Thane's?—If I may give it in my own words—

9360. (Chairman.) The only difficulty is that we have a little essay read in answer to a question?—It is not an essay.

9361. It looks rather formidable?—No, it is not really.

9362. It means that a great deal is introduced which really is not evidence at all very often; it is information furnished to you by others who are not here?—This is a direct answer to the statement which Mr. Thane made in his evidence on the marmot.

9363. But you never saw the marmot except on this one occasion?—I never saw that marmot, except on this occasion.

9364. I think you had better follow the questions.

9365. (Mr. Ram.) If you will just answer my question first, you may find that your statement is not necessary. Do you accept that statement of Mr. Thane's as to this marmot, that it is the only marmot that has been shown to physiologists in this country; that it died, and that he examined the body and found that the spinal cord had not been divided?—Very well. I will answer your question so far as I can. The statement that that marmot had never been experimented upon save for having its temperature taken is altogether incorrect. According to a paper by Dr. M. S. Pembrey —

9366. (Chairman.) Now this is the very thing —? —I cannot answer the question unless —

9367. Will you allow me just to say this. We are not here for you to disprove what Dr. Thane may say about this marmot by saying that Dr. Pembrey has said something else. If you think it necessary, Dr. Pembrey could come here. We cannot try the issue as to whether that marmot was experimented upon before or had had its spinal cord severed?—But, my Lord, it is an answer to a personal attack on me, and I understood from what you said last time that I was to be allowed to answer anything which had been brought into the evidence and which was in the nature of a personal attack.

9368. I do not call this a personal attack. Here it is

* See "Collected Papers for the Research Laboratories of the Royal College of Physicians, London, and the Royal College of Surgeons, England." Edited by T. G. Brodie (1903). L.L.-af-H.

said that the statement which you make in that book about this marmot is not accurate?—I bring evidence to show that Mr. Thane's criticism upon our book is incorrect.

9369. (*Mr. Ram.*) Will you forgive me for saying that I am not going into his criticism, and I certainly am not making any attack upon you. Is there any evidence which enables you either to disprove or accept Mr. Thane's statement that he examined the body of that marmot and found that the spinal cord had not been divided?—I can prove first that experiments had been performed on this marmot before.

9370. We will leave that for the moment, because I am dealing with the dead marmot?—In the second place we never laid claim to having seen the actual division of the spinal cord of this marmot. It is very difficult to answer such a complicated question if I am not allowed to give my answer fully.

9371. (*Chairman.*) It is a very simple question?—The marmot, according to Mr. Thane's criticism, was in a state which resembled that in which the spinal cord has been divided. That has been acknowledged by Mr. Thane. He said that it was in a state which resembled paraplegia. We drew that inference from seeing the marmot and from my experience of other marmots, because I have lived a great deal in the Swiss Alps, and as a humble student of natural history I have observed marmots, and I may say that if the spinal cord had not been divided that marmot was very different from other marmots. I have not, of course, made a post-mortem on this marmot.

9372. (*Mr. Ram.*) But you see Mr. Thane has?—Mr. Thane says he has.

9373. I want to get it quite concisely. You state here as a fact that you once saw a marmot the spinal cord of which had been previously divided. Mr. Thane says that he examined the body of that marmot after death and found that its spinal cord had not been divided. Do you admit that you made a false deduction from what you saw?—I do not admit that we made a false deduction from what we saw, because I have no evidence sufficiently strong to convince me that we did. I have not seen that marmot myself after its death.

9374. Will you not accept the evidence of Mr. Thane?—I should hesitate to accept the evidence of Mr. Thane because he had just made a most inaccurate statement by saying that that marmot had never been experimented on before save for having its temperature taken.

9375. Then I am to take it that this distinct statement of fact by Mr. Thane is one that you decline to accept?—I decline to accept it on the ground that he had just made an inaccurate statement.

9376. Will you admit that the state of the marmot was one which might lead you to make a wrong deduction?—I need not admit that, because the marmot looked as if it had had its spinal cord divided.

9377. And therefore you inferred that it had?—Yes.

9378. And you still stick to that inference?—I see no reason not to stick to it.

9379. Then I leave that.

9380. (*Chairman.*) Do you think that because in your opinion Dr. Thane has made a misstatement about the number of experiments on that marmot it justifies you in dismissing the whole of his evidence?—I think that when Mr. Thane says that that marmot was never experimented upon save for having its temperature taken, and it had actually been subjected to fasting experiments—starvation experiments—I am justified in not accepting the second part of his statement.

9381. But supposing we thought that you were mistaken in your view about this marmot, you would think it very unfair that we should not accept any of the statements you have made in the "Shambles of Science"?—I am sure that the Commission will carefully scrutinise anything that I have said.

9382. We certainly shall, and, I hope, fairly.

9383. (*Colonel Lockwood.*) You want to bring Dr. Pembrey in evidence?—I have got the actual paper by Dr. Pembrey,* who owned this marmot, showing that starvation experiments had been performed on this marmot in the autumn time lasting 52 hours, 50 hours, and 42 hours at a time, when the marmot is in the greatest need of feeding, being an hibernating animal.

9384. (*Mr. Ram.*) What you saw was capable of one of two explanations, either that the animal was hibernating or that its spinal cord was divided, is that so?—No, it was not capable of that first interpretation which you refer to, because if it had been hibernating it would not have bitten the demonstrator, and Mr. Thane does not deny that it bit the vivisector, which really is the chief point in our bringing in that marmot.

9385. Then you ask us on this evidence, which we have to balance, to say that this statement of Mr. Thane's is and must be to his knowledge untrue?—I do not ask the Commission to accept that. I only beg the Commission to excuse me from accepting Mr. Thane's statement.

9386. What alternative can you suggest to us that we can find?—I can only leave the question with the Commission to draw their own conclusion between the two statements.

9387. In that same chapter which begins about the marmot you give an account of an experiment on what you call "The Quiet Cat"; that is at pages 37 and onwards?—Yes.

9388. Do you suggest, or do you wish the reader to believe, from this account, that that cat was under anæsthesia the whole time of the operation or not?—May I just look at the chapter.

9389. If you please; it is only three pages?—That cat was quiet all the time, and artificial respiration was, according to our observations, maintained. As no operation had been performed which necessitated artificial respiration (because sometimes it is necessary or the animal will die) we drew the conclusion that the animal was under curare, because artificial respiration would then be necessary, and, of course, if the animal was under curare it would be absolutely quiet.

9390. Is the conclusion which you desire the reader to draw that the animal was under curare and not under an anæsthetic?—The conclusion might be drawn that the animal had had both curare and an anæsthetic, and when curare has been given an anæsthetic is highly unreliable, and the animal would lie quiet without further doses.

9391. Will you show me anything in this chapter on "The Quiet Cat," which gives any indication to the reader that there was any anæsthetic employed at all?—No, I do not think anything is said about an anæsthetic.

9392. You have just told me that the animal, in your opinion, might have had both an anæsthetic and curare administered to it?—We do not say anything about an anæsthetic for the reason that nothing was said to the students.

9393. That is what I complain of. You have just said that, in your opinion, the appearance of the animal was such that both an anæsthetic and curare might have been administered?—I said that they might have been; but, if I might continue, our inference was that no anæsthetic had been given because we did not see any administered; there was no smell of ether or chloroform, and nothing was said by the lecturer about an anæsthetic having been given.

9394. (*Colonel Lockwood.*) Who did this experiment?—Dr. Bayliss. All the experiments at University College described in "The Shambles of Science" were by Dr. Bayliss.

9395. (*Mr. Ram.*) Then I understand that you draw the conclusion yourself, and that you wished to communicate it to the reader that this cat was not under any anæsthetic at all?—We drew the conclusion that it was curarised only.

9396. (*Chairman.*) Did I catch rightly what you said just now that all the operations described in the "Shambles of Science" were by Dr. Bayliss?—All the operations referring to University College were by Dr. Bayliss. May I add to my answer in conclusion that we do not state definitely that the animal was not anæsthetised.

9397. (*Mr. Ram.*) So I gathered. I asked you whether there was anything to indicate that it was?—It was not sufficiently indicated to us to enable us to state such a thing positively.

9398. In the case of the "Dog that Escaped" which begins on page 67, was that dog, in your opinion,

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* "The Respiratory Exchange during the Deposition of Fat," by M. S. Pembrey, *Journal of Physiology*, Dec., 1901. L.L.-af-H.

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under anaesthesia the whole time that it was under the experiment?—That dog we describe as struggling until curare was injected, and I note that Mr. Thane does not deny that curare was administered.

9399. He says that it was?—Yes, and so do we in the “Shambles of Science,” and after the injection of curare the animal became absolutely quiet.

9400. Do you know that Dr. Thane also says “anaesthesia was kept up the whole time by pumping air charged with a known amount of anaesthetic through the lungs”?—Yes, that is a distinct contradiction of what we say in the “Shambles of Science.”

9401. It is?—We say that no arrangements for artificial respiration had been made. And there again I must stick to what we have said for this reason, that after that particular demonstration we went up to the animal and stood near it for some five minutes after the other students had gone, talking to the laboratory boy, and I had ample opportunity of observing at that time whether artificial respiration had been maintained or not.

9402. Do I rightly understand that it was your belief then, and is now, that that animal was under curare only and had never had any anaesthetic at all?—That is my belief.

9403. Did you ever make any complaint to the Home Office about it?—We never made any complaint.

9404. There was, as you believed, a very gross violation of the law and horrible cruelty?—Yes.

9405. And you made no representation about it?—No, and our book was not written for the purpose of showing how the Home Office administers the law in England.

9406. (Chairman.) I do not think Mr. Ram's question referred to the book, but to what you saw. Whether you had written the book or not you made no complaint?—I see. I beg your pardon.

9407. (Mr. Ram.) You made no complaint at all?—No.

9408. At page 147, at the end of what you describe as “A Grand Demonstration,” you say: “We meet the boy carrying the miserable white rabbit into the lecture room again”—did you at the time when you wrote that believe that the animal, the miserable white rabbit, was alive or dead?—When the animal was brought in again we had no opportunity of observing whether it was alive or dead; we simply put in the book that it lay absolutely still. It had been taken away from the room because it struggled. It might have been alive or it might have been dead. We do not insist on its being alive; the description is merely general, and gives no details to that effect.

9409. What was the inference which you intended the reader to draw from that sentence?—Either that the animal at that time was properly anaesthetised or it may have been that it was dead. I say that it was impossible for us to draw any inference; you cannot if you merely see an animal carried in when you are passing out of a door and you see it is lying absolutely still.

9410. You notice that in this case again Mr. Thane says: “An anaesthetic (ether) was used the whole time; it was given just in the same way, and under the same conditions, as in an operation on man”?—Yes; and the whole chapter “A Grand Demonstration,” in our book describes how carefully ether was being given. We begin by saying “There is a strong smell of ether meeting us as we walk up the stairs.”

9411. Do you think that the animals referred to in this “Grand Demonstration” suffered anything, or that they suffered nothing?—I believe that the animals, as we have endeavoured to show, continually came to, and I see that Mr. Thane does not deny that. He says, “There was probably some movement now and then as the effect of the anaesthetic began to fall off. More ether was at once given.” That is exactly the same as we describe.

9412. He also says: “The so-called ‘violent struggling movements’ of the rabbit are greatly exaggerated. As explained by the lecturer (see p. 143), they are the reflex working of the muscles, excited by the injection of any depressor substance; they occur, as happened here, in deep anaesthesia”?—Yes, that would imply the purely reflex effect of such injection, but that is not what we referred to.

9413. Did you see anything there which enables you

to say that it was not purely reflex action?—The suggestion throughout this chapter—

9414. Please answer the question?—May I say that throughout this chapter, so far as I can remember at the present moment, we do not suggest that everything was not done to anaesthetise the animals.

9415. Do you suggest that the animals suffered any pain?—We suggest that the animals kept coming to, and that everything was done to put them under the influence of the anaesthetic again.

9416. Is it your suggestion that in the course of the experiment they suffered, or that they did not suffer?—There may have been some suffering on return to consciousness.

9417. (Chairman.) Do you make any suggestion that in that experiment the requirements of the Act were not complied with?—No, that suggestion is not made in that chapter. The suggestion is rather made of the difficulty of anaesthetising these three animals, and the nature of a vivisectional experiment versus a surgical one.

9418. (Mr. Ram.) I want to read to you one expression in your book, and to ask you to what it applies. To whom do you intend this sentence on page 68 to apply: “The tormented long for their freedom, but the tormentors fight the merciful death. They are artists in the black art of producing the utmost agony, while the gateways through which death could enter are carefully watched; but sometimes they are not clever enough”; that is in “The Dog that Escaped”?—That is not meant to apply to any special vivisector; it applies to the whole system.

9419. And to everybody who practises vivisection?—The style of this book is not like a law report or anything official, like an inspector's report; we say things which have a general application, and not only things which have a special application.

9420. Then you deny that it has a particular application to anybody whom you could name?—I think it is quite clear to anybody who reads it that it refers to vivisection generally, and to vivisected animals generally.

9421. Have you ever in England seen anybody, any operator, who, in your opinion, endeavoured to produce the utmost agony while watching the gateways through which death could mercifully enter?—I have read of many, as, for instance, the experiments of Mantegazza in Italy.

9422. May I take it that these words are not, in your opinion, applicable to anything you have ever seen in England?—They are applicable to the whole system of vivisection.

9423. In England as practised to-day?—In England, too.

9424. As practised to-day?—Of course, if you read it very literally word for word, and take it to pieces as if it were, as I say, the report of an inspector, perhaps, I cannot say that I have ever seen any vivisector who is an artist in the black art of producing the utmost agony; but from reading the whole literature of vivisection I know that there have been such.

9425. I am asking you whether here in England, under the law as it exists to-day, you can give us any instance of any person whom you have either seen in England or read of in England, who has produced the utmost agony, and carefully seen that death did not occur?—I am sure that I can give an instance of that.

9426. Will you give us one, please?—Firstly, all vivisectioners generally—

9427. Please leave the vivisectioners generally. I ask. Can you give us any instance in which in England since this Act came into operation those words could be justly applied?—Then I must say that the method of vivisection in itself is an attempt to stop death because death would interfere with the experiment.

9428. (Chairman.) Surely you can answer the question a little more directly?—Then I can say that there are vivisectioners at present licensed and holding certificates in England who, I think, would be quite capable of exercising the “black art of producing the utmost agony, while the gateways through which death could enter are carefully watched.”

9429. (Mr. Ram.) Will you give me the name of one person?—I can give you the names of several. I give you the name of Dr. E. Klein, who in 1906 held three

certificates exempting him from the obligation to use anaesthetics.

9430. Any other?—I can give the name of Dr. Schäfer, who in his last evidence before the Royal Commission exhibited the greatest callousness with regard to frogs.

9431. Any other?—I can give you the name—I hope I do not give offence to the Commission—of Sir Lauder Brunton. I quoted some experiments of his in my former evidence which I considered callous and brutal.

9432. Any other?—Am I supposed to go on?

9433. I am asking you for any that you desire to give. You assert that that is, in your opinion, the state of things to-day under our present law here in England?—If the tendency of vivisectioners be to do cruel things, I do not think there is any power of stopping them in England, neither in the law as it stands nor in its administration as it is now administered; there is any amount of liberty under the administration of the law in England.

9434. You believe that these people whom you have named are all capable of doing this act—of producing the utmost agony while the gateways through which death could enter are carefully watched?—I trust I have not mentioned any name without giving a reason for so doing.

9435. Now, can you give me, please, any instance in which any one of these persons—take Dr. Schäfer, for instance—has been guilty of anything like this?—Certainly, if I am allowed to read his evidence before the last Royal Commission.

9436. What is the date of that?—It is the last Royal Commission.

9437. That would be before the present Act was passed?—Yes, the last Royal Commission on Vivisection.

9438. After which the Act was passed?—Yes.

9439. You recollect that my question was under the Act to-day; still, I do not want to exclude anything?—I did not quite catch that question before.

9440. What I asked you was: Can you give me instances either of people or of operations since the present Act was passed which justify this definition of yours?—I think I should be able to do it if you will allow me a few minutes to think.

9441. Perhaps you would like to do it after the adjournment.

9442. (Chairman.) Will you let me ask you, before we pass from these words in this chapter, "The Dog that Escaped," do you consider that in the case of the dark-coloured dog, which you call "The Dog that Escaped," the operator and demonstrators were "artists in the black art of producing the utmost agony while the gateways through which death could enter are carefully watched, but sometimes they are not clever enough"; and then (after three dots) you go on, "There is a dark-coloured dog somewhat resembling the Dalmatian," and you describe the experiment. Did you consider that those who were operating upon this dog were such artists as you describe there and guilty of what you describe there?—I think that in writing that passage —

9443. Would you kindly answer the question?—Yes, I am answering it, and you must allow me to say that I feel the responsibility of what I am saying, and therefore I cannot answer very quickly; I must think before I answer. In writing that passage we had in our minds a number of vivisectioners generally, and also—allow me to finish—the two operators upon that occasion.

9444. But do you consider that what happened to the dark-coloured dog described in this chapter is an illustration of that "black art of producing the utmost agony while the gateways through which death could enter are carefully watched, but sometimes they are not clever enough"?—Yes, it is an illustration, because they sought to prevent death and failed.

9445. You tell us this about the dog as an example of the present treatment of animals?—It can be taken as an example.

9446. Do you mean it as an example?—Yes.

9447. You do?—Yes.

9448. I thought you told us before that you did not think that in this case the Act was not complied with?

—No, not in this case; that was in relation to "A Grand Demonstration."

9449. Then you do mean to say that this was a case in which—Dr. Bayliss, was it?—Dr. Bayliss and Professor Starling.

9450. In which they were acting the part, in treating this dog as they did, of "artists in the black art of producing the utmost agony, while the gateways through which death could enter are carefully watched, but sometimes they are not clever enough"?—Yes, it may be taken like that.

9451. (Mr. Ram.) There is one other quotation that I want to ask you about. On page 152, under the heading of "Vivisection Bankrupt," you say this: "In the pain-tortured bodies, trembling under the sharp steel, bathing in their own blood, and in vain trying to tear the straps that fasten them to their crosses of agony, twitching under the piercing currents of artificial electricity, there is no trace left of the normal life in the nerves, in the blood, in the wonderful co-ordinated mechanism of the internal organs." Does that relate to animals which are not under any anaesthetics?—That chapter relates to vivisection generally.

9452. Do you intend in that paragraph to allude to animals under anaesthetics or to animals not under anaesthetics?—That paragraph should be read, as I pointed out in my former evidence, in connection with the paragraph that follows. The two paragraphs were written to show that vivisection is always performed under abnormal conditions—(1) pain and (2) anaesthesia.

9453. But do you believe that these "pain-tortured bodies," of which you speak in that paragraph, were under anaesthesia or not?—We do not speak of any special experiments. This is a general chapter on vivisection in England, vivisection throughout Europe, vivisection all over the world.

9454. That is what I am trying to get at. Do you present this as a fair and true representation of vivisection as practised in England to-day, that animals have their "pain-tortured bodies, trembling under the sharp steel, bathing in their own blood, and in vain trying to tear the straps that fasten them to their crosses of agony"? Is that a true representation of vivisection as practised in England to-day?—Yes, it is a true representation according to other descriptions of vivisectional experiments given in other chapters.

9455. Will you tell me of any experiments that you have seen in which any one or all of these different ingredients of pain were present?—I can refer you to "The Dog that Escaped," which we have just been discussing.

9456. Is this passage, then, intended to represent and sum up your opinion of what you yourself saw?—Not only of what we saw; that added to our general experience through reading of what vivisection is and the experience of others.

9457. And you believe that that goes on to-day in England?—Yes, I believe that that goes on to-day in England.

9458. And you believe that what you have described in this paragraph is a truthful representation of certain experiments which you saw?—Yes; and, added to that, my further experience and knowledge of vivisection.

9459. And you made no complaint to the Inspector or to the Home Office?—No, I was not there to make complaints to the Inspector; I wished to see how vivisection is practised in England under the Act of 1876. It is supposed to be more humane here than in other countries.

9460. And you represent that as a fair description of it?—Yes.

9461. (Dr. Gaskell.) I want to ask you a question with respect to what you have just told Mr. Ram about "The Dog that Escaped." Am I to understand that curare only was given and no anaesthetic?—According to our impression curare only was given.

9462. And there was no artificial respiration?—There was no artificial respiration according to our observation.

9463. The object of the demonstrator was to kill that dog, I presume?—No, because a slight dose of curare does not kill a dog. Dr. Bayliss said, "The animal is now slightly curarised."

9464. Have you ever seen or heard of anyone who was going to slightly curarise a dog who would not put in

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a tube for artificial respiration first?—I have frequently read that a slight dose of curare does not stop respiration.

9465. But would he not put in a tube first, on the possibility of respiration being stopped?—I think it is quite possible that that would not be done in the case of a very slight dose. Our contention here is that Dr. Bayliss meant to give a very slight dose and gave a stronger dose than he intended, and therefore respiration failed.

9466. You did not see that tube which was in the trachea?—No, we saw that there was no tube in the trachea.

9467. Then Dr. Thane's statement there is absolutely untrue, is it? He says, "Anæsthesia was kept up the whole time by pumping air, charged with a known amount of anæsthetic, through the lungs—i.e., artificial respiration was carried on. Curare was administered, and the artificial respiration, the air still charged with anæsthetic, was kept on as before"?—Yes, that is a distinct contradiction.

9468. Contradiction of what?—Of our description in the "Shambles of Science."

9469. But is it a contradiction of fact?—Yes, it is a contradiction of fact.

9470. Then you say that that is a deliberate statement on the part of Mr. Thane and whoever it was he consulted—Mr. Bayliss—to deceive this Commission?—Mr. Bayliss may not have remembered, because no notes are kept of demonstration experiments, according to the evidence given at the trial.

9471. Mr. Thane goes on further to say: "The apparatus for artificial respiration broke down. The professor went to put it right. The experiment was continued, but the apparatus gave way again. Respiration was stopped, and the animal died of asphyxia." It is a very circumstantial account—invented, I presume, according to you, by Mr. Thane and Mr. Bayliss?—I can pass no opinion as to whether it is invented. I simply say that I do not agree with it.

9472. I will leave it at that. You have quoted Professor Claude Bernard with respect to the action of morphia, and I presume that you have read various papers of his, have you not?—I have read a good deal of his work.

9473. He says, in talking of the action of morphia, "However, the sensibility persists, for if we pinch the animal it moves and cries"?—Yes.

9474. That is what you saw?—Yes?

9475. And from that you conclude that Claude Bernard considered that the animal felt pain?—Yes.

9476. But he goes on to say: "But these are only movements due to a very obscure kind of perception. In reality he does not dream of getting into a rage, or of defending himself; his intellectual faculties are completely in abeyance"?—Yes, that is no contradiction.

9477. That does not look, does it, as though he considered that the animals suffered pain?—As a supplement to that I put my quotation from Claude Bernard on the effect of morphia, where he says that the animal "feels the pain but has lost the idea of self-defence."

9478. In this case he is defining what he means by feeling pain—that is, movements are due to a very obscure kind of perception?—But he says distinctly that it "feels the pain." He cannot say anything clearer.

9479. This is a further statement of Claude Bernard in his "Lessons on Anæsthetics and on Asphyxia"?—That is the book I have brought with me to-day, and that book has a great many statements which I can show you proving that Claude Bernard did not consider morphia an anæsthetic for dogs in any sense. I have carefully read through this book before coming to-day.

9480. On page 219 he says distinctly: "Morphin sends to sleep pain which has its seat in the sensorium"?—He says, on page 219, that he injected a young dog with 5 c.c.'s of a solution of morphia hydrochlorate, and that resulted in—yes, you are right—that is an exception.

9481. An exception?—Yes, that is an exceptional case, and entirely different to that given on p. 222, and on other pages; but even then he shows that "Si l'on reveille le chien ou seulement si on le pince" physiological functions at once become more normal.

9482. I did not understand it to be an exception. Again, as illustrating the action of morphia, you will find on page 235: "After the injection of 1-2 centigrammes of hydro-chlorate of morphia into a man, the first effect of inhalation of chloroform given in the usual manner is to produce a state of analgesia, with retention of the intelligence, the sense and the power of voluntary movement"?—Yes, but I do not propose to deal at all with the effect of morphia on man. My evidence last time was rather to show that dogs are extraordinarily insusceptible to morphia, and I have here another quotation from Claude Bernard.

9483. I wanted to ask you whether you did not realise that Claude Bernard says there that the action of morphia with a very slight dose of chloroform is to remove all pain although consciousness persists and the sense and the power of voluntary movement?—Neither am I dealing at the present moment with morphia and chloroform; that makes all the difference in the world.

9484. But the chloroform is so slight, according to Claude Bernard, that consciousness still persists with retention of the intelligence, the sense and the power of voluntary movement, but that the action of morphia, plus a slight amount of chloroform is to remove all pain?—Then my answer is, that if that be the case in man, man is far more susceptible to morphia than a dog would be.

9485. But I was simply stating that Claude Bernard's argument is that morphia removes pain?—I cannot accept that statement of what morphia does with regard to man as being equivalent to what it does with regard to a dog.

9486. Do you not believe—it is not a question of belief; do you not know that in these cases of anæsthetics being given, the sensation of pain is one of the first things to go?—That is not always the case. The sensation of pain may persist. I have quoted Professor Flint to show his opinion upon that point, and I believe that is very often the experience of people who have been under anæsthetics.

9487. Is it not a very common experience to find that pain does go even although apparently consciousness still exists?—It is a common experience; but, on the other hand, it is a common experience that pain does not go.

9488. Have you ever had laughing gas?—I have; I was just describing that when you came in this morning. I had laughing gas once for the purpose of extracting a number of pieces of a tooth which were left in my jaw, and although I was unconscious to a certain extent, I felt the pain of the operation all the time; though when I came to perfectly I very soon forgot all about it.

9489. And then when you were asked the question whether you felt any pain you said "No"?—I did not. I said that I had felt the pain.

9490. Is it not often the case that patients under a slight anæsthetic like laughing gas, not only cry out but make violent movements when the tooth is drawn, and afterwards assert that they felt no pain at all during the operation?—That is often the case; but it is equally often the case that people do feel pain and yet are in a kind of subdued state; as, for instance, when slight doses of chloroform are given to women in cases of labour. I know of one instance of a woman who felt pain all the time. She lay in a state of subdued hushed consciousness, but was afterwards able to state definitely that she felt all the pain, though she was unable to give any expression to it. That is a personal acquaintance of mine. There are such cases.

9491. That meant that a very little more chloroform might have been given with advantage, did it not?—She could not ask for more, because the people round her thought that she was unconscious and talked of her as being unconscious, and she heard them talking of her as being unconscious.

9492. (Dr. Wilson.) She was in a cataleptic condition?—She was in a cataleptic state.

9493. (Dr. Gaskell.) Do you not notice that Claude Bernard speaks of the persistence of sensibility in the case of morphia; it is the persistence of sensibility that he speaks of, and then later on he defines what he means by sensibility?—Yes.

9494. And he goes on to point out the difference in the use of the term sensibility by philosophers and physiologists?—Yes.

9495. He says that "according to the former sensi-

bility is the sum total of the psychical reactions provoked by external stimuli. For the latter, for us, it is the sum total of the physiological reactions of every kind provoked by external stimuli"—Yes.

9496. Do you not understand by that that he does not consider that the retention of sensibility after morphia means necessarily the retention of consciousness?—I think, from reading Claude Bernard's work on this subject carefully, one comes to the conclusion that he does mean by his *sensibilité* what we mean by sensitiveness. I remember I was once attacked in a book by a Swedish vivisectioner on this very subject, and in my answer I pointed out the various meanings attached in French to the expressions *sensibilité*, *irritabilité sensitive*, or *excitabilité sensitive*; and there is no doubt that by the first Claude Bernard means the persistence of conscious sensation. For instance, here on page 222 he says: "Though in a state of torpor the animal is excitable and sensitive. But if the nerves have not lost their *sensibilité* they are at least dulled." And he adds that the animal is sensitive to pain if it be pinched or subjected to stimulation. Besides, there is the distinct phrase "signs of pain"—*les signes de la douleur*. What could be clearer?

9497. But in this other case he says "*sensibilité*." "However the sensibility persists, for if we pinch the animal it moves and cries." And later he states that "for the physiologist in addition to the conscious sensibility there exists also an unconscious sensibility"?—But if he has mentioned *les signes de la douleur*, there can be no doubt about it.

9498. Afterwards he distinctly asserts that morphia sends to sleep pain," as I showed you?—I do not think it has that meaning, for he has previously said (p. 211): "L'action de la morphine diffère tout à fait de l'anesthésie."

9499. Now, I would like to go from Claude Bernard to your own evidence?—My printed evidence, or the book?

9500. The evidence in the book. Will you turn to your chapter headed "An experiment that 'is not supposed to be useful' "?—Yes.

9501. You tell us, "The animal is now deeply under morphia"—and then throughout the whole of the experiment you further speak of it as "absolutely still." Then you say, "We can be almost certain that the dog has been also curarised, because it is absolutely still with the stillness that is characteristic of the 'hellish orali' "?—Yes.

9502. And therefore all we know from this statement of yours is that the dog was under morphia, and in your opinion had curare as well?—Yes.

9503. But you will see from Dr. Thane's evidence that there was no curare?—Yes, I have seen that he says so.

9504. Then does not your evidence show that morphia alone kept that animal still?—Yes. All the evidence that I have given on morphia has gone to show that morphia is a drug which has the power of making an animal lie very still—not a cat, but a dog—and that the animal may be sensitive all the time, and that it would respond to painful stimuli.

9505. But these were painful stimuli and it lay still—it did not respond?—Yes.

9506. Therefore, according to your own evidence, morphia was very distinctly an anæsthetic in that case?—According to my evidence, if we were certain that the dog had not also been curarised it would show that it had lost its power of self defence.

9507. But you find now that that is not so?—I hear that Mr. Thane mentions that it is not so; I do not find that it is not so.

9508. Then, may I ask, do you still stick to it that the animal was also curarised?—I stick to all the things that we have written.

9509. It was under curare?—Certainly.

9510. And this is not evidence of the anæsthetic properties of morphia?—No.

9511. This dog had curare?—Yes, because it had every sign of having curare.

9512. Although we are told definitely and absolutely that no curare was given?—Yes.

9513. Take another case, "Scarcely any Anæsthetic"?—May I add that our reason, and a very strong reason, for concluding this, is Dr. Brodie's own statement in his book, where he distinctly states that

he prefers curare because it is essential to the only satisfactory method. *Miss L. Lind-af-Hageby.*

9514. Which are you referring to now?—"The only completely satisfactory method." 5 June 1907.

4514A. I was speaking of "Scarcely any anæsthetic"?—But the physiologist was Dr. Brodie on both occasions, and he has written in his book for students very clearly on his preference for curare.

9515. "Not supposed to be useful" was the one. Was that Dr. Brodie?—Yes.

9516. In this case "Scarcely any Anæsthetic," at page 49 in the old edition, you say: "The animal is so profoundly under morphia we have scarcely had to give it any anæsthetic at all," says the lecturer?—Yes, those were the words of Dr. Brodie.

9517. I understand that to imply that the main narcotic given there was morphia?—Yes, according to Dr. Brodie's own words; he has not denied it.

9518. No, I do not want to deny it?—I believe he has not denied it through Mr. Thane.

9519. I only want to point out to you that you say on the previous page: "The strings which hold the dog's legs are loosened. The assistant demonstrator lifts up first the hind legs and then the body several times, whilst the lecturer holds down the animal's head." Would not the dog have made any movement, if it had not been anæsthetised, under those circumstances? The limbs are free, the dog is capable of moving them as much as it likes; it is pulled up and down in this way; if morphia was not an anæsthetic would not the dog have moved?—I have never denied that morphia has the power of keeping a dog quiet. Morphia can keep a dog quiet.

9520. What, when it is in pain?—If it were in a state of narcosis, it is quite possible that it would be quiet for that moment when the legs were loosened from the board.

9521. But it was in pain all the time, according to you. Do you mean to say that it would not demonstrate its pain if it was conscious and was capable of doing so, by the legs being moved?—How could it demonstrate its pain by moving the legs when the assistant demonstrator held both hind legs? How could it struggle with its hind legs when he held them both for the moment, if it had wanted to do so? If I take up a dog which is fastened to an operating table and lift up its hind legs and hold them together with my hands, that dog cannot struggle.

9522. Let us take "Long Experiments." This again is a case of morphia and a little chloroform?—Yes, according to Professor Halliburton.

9523. And you say throughout that the animal was quiet. It is not an instance that you give of any evidence of pain?—No, we do not give that as an instance of evidence of pain; we could but see that the animal was quiet, and we honestly put it down and recorded it.

9524. And yet what was mainly given was morphia?—I do not say that the animal did not suffer pain. I do not know. I could only record that it was quiet.

9525. Then I understand that in these three cases of morphia which I have quoted to you, although the animal was in pain, it remained remarkably quiet as though it was properly anæsthetised, and did not show any sign of that pain by struggling and so on?—I do not know whether that is so in the three cases you have quoted.

9526. I understand from your own statement that in these three cases the animals were very quiet?—Then it must be so. I cannot at this moment recollect which three they were. If that be so, it is just because morphia does make a dog quiet. In two of the experiments I believe chloroform was given as well.

9527. I have a question to ask you on "The Only Completely Satisfactory Method." Here again your remark (on page 134) is: "The director of the laboratories is anxious to make the most of the dog that is so nice and quiet to experiment upon"?—Yes, the director was Dr. Waller. He is the director of the laboratory.

9528. Do you mean to say that that quietness was not due to properly given anæsthetics?—We inferred that it was due to curare, because Dr. Brodie was the lecturer, and he has expressed his preference for curare. He said nothing about having given an anæsthetic, and there was no smell.

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9529. Here again I suppose you would not agree with Dr. Thane's statement: "The assertion that curare has been used is false. The animal was anæsthetised with chloroform throughout, under the superintendence of the director of the laboratory"—The director of the laboratory did not superintend any giving of chloroform in our presence during the hour and a half, and if you give chloroform you have to give every five minutes, or more frequently. I do not think Dr. Waller would say that he did do that, because that I should think would be impossible.

9530. Dr. Thane goes on to say: "Artificial respiration is being kept up"; quite true; but the writer omits to state that the air pumped in was passed through a bottle containing, and so became charged with, the anæsthetic"—That is his statement.

9531. Is not that a possible method of giving chloroform?—Yes, and we stated that artificial respiration was kept up.

9532. That you omitted to see, apparently?—But no chloroform was given in our opinion.

9533. Because you could not smell it?—We could not see any bottle.

9534. No, because the bottle was in another room?—Nor could we smell it.

9535. It is the same as in the case on which the action was. There, again, because you did not see the chloroform you stated again and again that there was no anæsthetic given. In each of those cases anæsthetics are given in the same way, with chloroform, the air rising through it?—But on other occasions, in the University of London, anæsthetics were given in the usual way through dropping the anæsthetic on to a cloth, and then everybody could smell it. On this occasion there was no such indication.

9536. And you made a mistake?—I do not acknowledge that I made a mistake. I saw the pump; we describe how we saw the pump and heard the hissing sound, but no bottle.

9537. If you do not acknowledge that you made a mistake, I say no more?—Certainly, I do not.

9538. There is one more question that I should like to put to you on this book. You speak in one sentence, I think it is in that same "A Grand Demonstration," on page 144, of the rabbit moving its paws: "When the table with the operation board is now lifted up, and the rabbit carried to the place in the middle where the dog had been, the little animal again stretches up its fore-paws with claws thrust out"?—Yes.

9539. Are you aware that in physiological laboratories that movement of extension of the paws is a sign of asphyxia?—There was no asphyxia then. The animal was, according to Mr. Thane, asphyxiated later on in another room.

9540. Will not asphyxia take place if chloroform is pushed too much?—It was not pushed too much, because no chloroform was given. It was ether.

9541. Ether is still more restricted?—According to Dr. Thane, and, I think, it is admitted, the animal had to be taken out to be killed because it came out of the anæsthesia. There was no question of asphyxia at all, neither according to our account nor Dr. Thane's.

9542. Do not you know that when a rabbit is under an anæsthetic, either chloroform or ether or A.C.E. mixture, as soon as the experimenter sees that movement of the paws he gives less chloroform, because he knows that he is overdoing it?—That is quite possible; but that was not the case here.

9543. It is the beginning of asphyxia in consequence of an overdose of chloroform?—It would entirely contradict Mr. Thane if what you say is true; because he acknowledges, I believe, in his pamphlet that the rabbit was taken out to be given more anæsthetic.

9544. But that is afterwards?—He does not say that there were not violent struggles and movements of the rabbit. He says that they were "greatly exaggerated" by us.

9545. I was only referring to that movement of the paws. I wanted to know whether you recognised that movement of the paws does not necessarily mean returning consciousness?—Yes; but I also contend that upon this occasion it could not mean asphyxia or any danger from too much anæsthetic, because, according to Mr. Thane, the animal was sent out to be killed by an overdose of ether. If it had already had an overdose of ether, it would not have been sent out to be killed.

9546. (Dr. Gaskell.) You spoke of physiologists using light and deep anæsthesia?—Yes.

9547. Are you acquainted with Hooper's experiments on the recurrent laryngeal?—I am not.

9548. Do you not know that with ordinary anæsthesia, when that nerve is stimulated you get closing of the glottis, and that when anæsthesia is pushed deeply you get opening of the glottis. You have not heard of that?—I am not acquainted with those special experiments. I do not know to which you refer.

9549. I only wanted to know whether you were aware that the peripheral nerve alters its effect according to the condition of anæsthesia. That the recurrent laryngeal opens the glottis when the anæsthesia is deep?—Of that, I think, I am generally aware—not of those special experiments—because that is one of the reasons why I have stated that physiologists prefer light anæsthesia, as deep anæsthesia sometimes interferes.

9550. This is the peripheral nerve?—Yes.

9551. That has nothing to do with the central nervous system?—Do you mean when the nerve is cut off?

9552. I mean the nerve cut off and the muscles stimulated by excitation of the peripheral end?—Yes.

9553. You do not also know of Bowditch's experiments upon the action of anæsthesia upon the sciatic of the frog?—No; of course I am not a vivisector, and even a vivisector cannot be acquainted with all experiments.

9554. I thought you had studied a certain amount of physiology?—I have studied a certain amount, but not in the way that a vivisector would.

9555. There again the evidence is that stimulation of the nerve will cause extension when the anæsthesia is slight, and the opposite effect when the anæsthesia is deeper. And do you know the difference between pressor and depressor effects upon the stimulation of an afferent nerve, according to whether the anæsthesia is deep or not?—Yes, I have some knowledge of that.

9556. And also the difference in the effect upon the respiratory centres?—Yes.

9557. How inspiration takes place in one case and expiration in the other; you know that?—I have some knowledge of that, too.

9558. Do you not see then that the reason why physiologists make a distinction between light and deep anæsthesia is on account of the reversal of effects which take place in nervous actions?—Certainly, I have seen that all the time, though, of course, not only from the vivisector's point of view.

9559. It has nothing to do with the question of pain?—It has to do with the question of pain. Thus, for instance, Professor Starling in one place refers in his book "Elements of Human Physiology," to incomplete anæsthesia, and speaks of "Pain, as evinced by the movements of an animal not fully under the influence of an anæsthetic." That would prove, in my opinion, that incomplete anæsthesia has to do with pain.

9560. But I was using your own terms, light anæsthesia and deep anæsthesia—not incomplete anæsthesia?—That is the same thing, light or incomplete, so far as I understand.

9561. That is exactly what I want to get you to see. It is not the same thing?—I have never heard that before.

9562. Do not you know that the reason why a distinction is drawn between deep anæsthesia and light anæsthesia is to emphasise the deep anæsthesia because of its reversal of the effect?—I quite see that.

9563. That is all I want to bring out with reference to that. Now, might I ask you with regard to your theory that the spinal cord of the frog is capable of feeling pain. I think I am right in saying that you rather hold that view?—Yes.

9564. I suppose I shall not be wrong in understanding that you hold that view on account of the apparently purposive activities which take place on stimulation of a nerve when the spinal cord is isolated?—In holding that view I am in very distinguished company.

9565. I only wanted to know whether that is your reason for holding it?—I have many reasons, but I cannot answer you with a single sentence. I could give a considerable amount of evidence on that point if I were allowed

9566. I thought you said that it was merely because of the purposive activities?—That is one of the reasons. I have more; I have a great deal of knowledge of these experiments.

9567. I was only asking you that because I wanted to know whether you would also hold the same view with regard to the spinal cord of a dog?—I do.

9568. That there purposive reactions take place showing that there is pain?—Yes, and I based my statement last time—

9569. Do you also hold the same view with regard to a monkey?—Certainly.

9570. And do you hold the same with respect to man?—No.

9571. You draw a distinction between a monkey and man?—Yes, I do. I could not offer an opinion about the anthropoid ape, but I do think that a monkey is sufficiently low for the spinal cord to be exceedingly highly developed in comparison with the cerebral hemispheres.

9572. Then according to you man's spinal cord is not capable of feeling pain, but you think that a monkey's is; there is that difference in the purposive activities of the spinal cord of a man and of a monkey?—Yes, I think there is a difference. I cannot be absolutely dogmatic on that point.

9573. If consciousness of pain is established because of the purposive activities upon stimulation, may I ask why you confine yourself to the spinal cord; is there not another system in the body, the sympathetic system?—Yes, but I do not think that the sympathetic system plays the same part as the spinal cord.

9574. Is there not any evidence of purposive reflexes from the sympathetic system?—Yes, there is. I do not think that any physiologist has ever put forward that the sympathetic system would be capable of any kind of consciousness; but a great number of physiologists have put forward that the spinal cord would be capable of a kind of hushed consciousness. Thus Huxley himself in 1870 read a paper entitled "Has the frog a soul, and, if so, of what nature is that soul," based entirely on these strange phenomena exhibited by the spinal cord of the frog.

9575. Are you not aware that the sympathetic system used to be looked upon as entirely independent of the central nervous system, and that they went so far as to speak of the coeliac ganglion as the brain and the soul of that system. You did not know that?—I think there has been a great deal of theorizing with regard to the solar plexus generally, and is still; I am quite aware of that.

9576. I thought you said that no physiologist had ever put forward the view that the sympathetic system was independent or capable of reacting like the central nervous system. That was the view that was always held?—I am not aware of any physiologist who would be acknowledged as a physiologist at the present day, who has put forward that theory.

9577. One great physiologist I know of was a man named Bichat?—Yes?

9578. Did he not put forward that view?—Not that I am aware of.

9579. Do you know the experiments that have lately been made on Auerbach's plexus in the intestines?—I am acquainted with them.

9580. There is evidence in those experiments that the ordinary peristaltic action of the intestine is due to nervous stimulation, owing to Auerbach's plexus?—Yes.

9581. Would you not call that of the same character as the purposive activities that you can get from the spinal cord of a frog?—No, I would not, because they would be of an entirely different nature. The purposive activities which you get from the spinal cord of a frog, and of animals higher in the scale, present certain, what I might call mental or psychic phenomena, which I am sure would not be presented by any plexus of any kind.

9582. The one is the ordinary movement of the intestines and the other is the ordinary movement in the striated muscle; I do not see quite where the distinction lies?—The distinction lies in this, that the theories I hold with regard to the spinal cord, spinal automatism, and its power of independent action, are based not on mere movement, but on the deliberate choice in action, and on signs of consciousness which have been again and again manifested by such animals.

I need only refer to Steiner's well-known experiments on frogs *contra* Goltz and to Pflüger's experiments.

9583. But upon this question of peristalsis in the intestine, there are two effects occurring: one is the contraction of one part of the intestine, and the other is inhibition of another part?—Yes.

9584. That is as purposive as the purposive movements of the muscles. It is not a simple contraction of the muscles but a purposive movement?—You are now using the word purposive in the wider sense in which it can be applied to every function of the body; there is nothing in the body which does not have a purpose; but I am using the word purposive in the sense of conscious purpose in a living creature. It is quite a different thing.

9585. I do not understand where the word conscious comes in.

9586. (Mr. Tomkinson.) May I take it that your general position towards the question is this: that while you are strenuously opposed to every kind of cruelty, and would do your best to minimise it, the special branch that you have taken upon yourself is the branch of this anti-vivisection?—That is so, because I consider it the worst, perhaps, of cruelties, though there are others that can be compared with it.

9587. And in regard to its practice you are, in the first place, sceptical about the possibility of total anaesthesia?—Entirely sceptical.

9588. And even if you were not so, you would think it inevitable that, either from carelessness, or omission, or indifference, at times great cruelty would be inflicted by operations?—Yes, I think so.

9589. Those are your general grounds of opposition to the system altogether?—Yes.

9590. You are not altogether opposed, if you could not obtain a complete prohibition of it, to taking instalments?—No, I am not opposed to it, but I should continue to demand abolition.

9591. In the direction of its limitation to certain animals?—No. If I had no choice I should, of course, gratefully accept the limitation of the practice.

9592. And you would also support further restrictions in regard to the prohibition of morphia and curare as anaesthetics?—I should, as preliminary measures.

9593. Did I rightly understand that you have witnessed yourself personally many operations?—Yes, I have witnessed a certain number of operations on living animals.

9594. And did you say that in two cases only you thought that complete anaesthesia was established and maintained throughout the operations; there is some confusion about the "Two cases only"?—Yes.

9595. (Chairman.) Does that mean two of those in the book or two of the 50 you spoke of?—All the experiments that we saw are not described, but the severer ones are described in this book.

9596. But I only meant that Mr. Tomkinson's question seemed to assume that you were speaking of only two out of the 50 operations which you had seen properly anaesthetised?—It would be an exaggeration to say two out of the 50, because during many of those 50 there was nothing which might be called real vivisection. But among those described in this book there are only two. That is the answer.

9597. I understood that was what you did say. I only wanted to make it clear.

9598. (Mr. Tomkinson.) You have no suggestions to make as to increase of inspection?—No, I do not believe in increase of inspection.

9599. You think that inspection is a mere formality?—As inspection is practised now it has been of no avail at all, I think, either in checking the increase of vivisection or in enforcing compliance with the Act.

9600. On the ground of its infrequency?—Both on the ground of its infrequency and other matters associated with it. I do not think the Inspectors (but perhaps that is outside the inquiry) are men we have confidence in as Inspectors.

9601. Did you notice what Professor Starling said with regard to experiments in the way of starvation, of depriving animals of food?—Yes.

9602. Did you notice that he said that he thought the mere fact of going without food was not a painful experience?—Yes, I noticed that with some surprise.

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9603. And instanced the Italian faster Succi?—Yes.

9604. Who was supposed to have fasted for 40 days?—Yes.

9605. But who was afterwards found to have received something?—Was he? I did not know that.

9606. In the way of a phial, I think he did. What have you to say with regard to that? It is rather a novel idea that starvation could take place without cruelty and pain?—I should certainly consider starvation generally, forced starvation, painful. Of course a professional faster might by power of will reduce that pain to a minimum; but I saw the Italian faster Sacco come out of his fast at Hengler's Circus at the very moment he broke it, and I think he was in a very weak and bad state.

9607. Undoubtedly he was?—But I do not think that case can be at all applied to the artificial starvation of an animal that is craving for food and has no will power to put against the pain. Professor Starling gives several examples in his book of animals starved for experiments, and especially one case where an animal is starved and then made to do hard work afterwards, drawing a milk cart, which certainly would be cruel. I do not know whether he would declare that to be a painless procedure. It is at page 491 of his book. He says: "Much more efficacious than starvation in causing disappearance of the hepatic glycogen is muscular work. If a dog be starved for a day and be then made to drag a heavy milk cart about all the next day" (that is rather hard lines, I think), "there will be found only the merest traces of glycogen in its liver." A dog is to be starved one day, and then made to drag about a heavy milk cart all the next day.

9608. You described how you witnessed some operations which you thought extremely cruel on account of the imperfect anaesthesia, and you were asked whether it had occurred to you to make any representation, and you said it had not. You did not think of laying an information?—We were not there for that purpose.

9609. Were you there under any obligation of loyalty or secrecy not to do that?—No, we were there entirely as independent students for the purpose of seeing what actually took place, and we were there for the purpose of seeing how experiments are performed under the present Act. It was not for us to remonstrate with the vivisectioner or to make any kind of official declaration, because the result would probably have been that we should have been turned out, and the experiments would have gone on just the same. We were there to see how they were carried out without any interference from us, and it cost us very much not to interfere many times.

9610. Do you think it would be a good thing if there was a freer right of entry, by people of accredited position, to go in and pay surprise visits at times and witness operations?—I should think so, provided that those people were chosen from among those who have humane views and not among those who are in favour of vivisection, or who only advocate its scientific utility and care nothing for its moral value.

9611. I am speaking of ordinary laymen without marked opinions upon either side?—Yes, I should think that would be useful.

9612. You are aware, of course, that in foreign countries the whole practice is infinitely less humane than in this country?—I am aware of that.

9613. That is to say, there are unspeakable cruelties and horrors in France and the United States and different places?—Yes.

9614. Therefore I suppose you admit the possibility, if the practice was totally prohibited here, of driving it more abroad where they are more inhuman, and probably the sum of animal suffering might not be diminished?—That is a very difficult question to answer. You mean that English physiologists would emigrate wholesale to foreign countries?

9615. No, I do not say that; I only meant that there might be more experimentation abroad by people there already?—I do not think that. The only danger would be of a wholesale emigration of all English vivisectioners to foreign countries; otherwise I think it would diminish the total sum of animal suffering.

9616. But the example of this country?—Yes.

9617. You would like this country to set an example?—Yes, I think the example would do a great deal of good.

9618. I think you said you were a vegetarian?—Yes, I am.

9619. You no doubt then appreciate very deeply a good deal of the cruelty in our slaughter-houses?—Yes, and I am very anxious that such avoidable cruelty should be prohibited by law.

9620. And also in many other ways; for instance, the trapping of wild animals?—Yes.

9621. Some people hold it to be an absolute necessity to prevent a plague of rabbits; thousands of rabbits are trapped in a very cruel manner?—I think it might be possible to destroy rabbits in a humane way, at any rate in a much more humane way than is done at present.

9622. (Dr. Wilson.) I think you said at a previous sitting that you had not witnessed any experiments at all on the Continent before you came to this country—in Sweden?—No, I had not witnessed any experiments in Sweden. I said I had seen the results of many kinds of experiments at the Pasteur Institute, but that I had not witnessed any actual experiments.

9623. But you held strong views on this question before you came to this country?—Yes, we did.

9624. Was that from your reading as to how experiments were carried on in Sweden and other Continental countries?—Partly from seeing the Pasteur Institute and partly from reading.

9625. And you had preconceived views as to how vivisection was carried out in this country, I take it?—I do not think we had very preconceived views as to how it was carried out in this country in particular; in fact, we expected it to be much more humane than it was, because of the Act of 1876—because of England being one of the few countries where there is any restriction. There is some restriction in Denmark and some in Norway.

9626. And when you entered at the London School of Medicine for Women, were there any experiments carried on in the physiological class there?—No, I believe not. The place is not registered for experimentation.

9627. They are not allowed there?—No. I believe Professor Cushny made a mistake in his evidence, no doubt unintentionally, in saying that the place was registered.

9628. Then did you join the advanced course of lectures on physiology at your own request, or were you invited to attend?—No, we joined them at our own wish. We sought permission in the usual way. Any medical student, any student at the London School of Medicine for Women, has the right to apply for permission to attend these courses, and we did that in the usual way.

9629. You had no intention then of writing about what you had witnessed?—No, we had no intention at the time of writing.

9630. Now, just a few questions with regard to this book, the "Shambles of Science." I take it, from all you have said, that you honestly and truthfully maintain that what you state in your book represents your impressions of what you saw, and of what you listened to?—Yes.

9631. And even as regards that experiment in that chapter, which was eliminated from the first edition, you still adhere to your views there?—I still adhere to our views there.

9632. Perhaps this is not a question which I should put, but with regard to what I may call the somewhat cynical headings of the various chapters, and somewhat scornful and sarcastic remarks that are made throughout the text, you used those, I take it, to emphasise in the strongest fashion your honest convictions as to what you saw and listened to?—The book was written under a great deal of indignation, and the scorn and sarcasm probably came out of that.

9633. But when you used the title "Fun," you had quite another feeling, which you wished to express. You did not mean that to apply to the students, did you?—Yes, in a certain way. The title was suggested by the, in our opinion, unnecessary hilarity and joking which went on whilst the dog was being experimented on.

9634. Some questions have been asked about the "Struggling Cat"; you said there was no tube in the trachea?—Yes.

9635. Dr. Thane, I think, maintains that there was

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artificial respiration carried out?—No, Dr. Thane acknowledges that there was no tube in the trachea on that occasion.

9636. But does he state that there was artificial respiration?—No, he states that it is as we say, that there was natural respiration in the case of the struggling cat.

9637. And, of course, there was no tube inserted into the mouth?—No.

9638. So that anæsthesia could only have been administered in the ordinary way, by a mask?—Or by dropping it on a cloth.

9639. And you did not see anything of that?—Nothing of the kind.

9640. And the cat was muzzled?—The cat was muzzled in the usual way, and tied down on its back.

9641. And it struggled?—And it struggled, and the movements are not denied.

9642. I think all these points were brought out, but I wished to make this clear about the respiration?—In that case it struck us that as there was no artificial respiration, the ordinary apparatus for anæsthetising ought to have been at work. It is not suggested that there was artificial respiration, with a pump blowing in the anæsthetic, and therefore I may well ask who is supposed to have administered the anæsthetic on that occasion.

9643. You did not see anyone?—No.

9644. You did not see any anæsthetic administered during the whole period of the demonstration?—No.

9645. (*Dr. Gaskell.*) May I ask, did you notice a bottle on the table?—No, we did not. And may I add that even if we had noticed a bottle upon the table, the bottle could not administer the anæsthetic; you would have to have somebody to deal with the bottle.

9646. Then you deny the entire statement that "The anæsthetic was being given from a Woulff's bottle, with natural respiration. The lecturer left the theatre to get his notes, and the cat began to move; a student fetched him. He came at once, and gave more anæsthetic; this required only the turning of a tap"?—We cannot deny that the anæsthetic may have been given through a Woulff's bottle before we were there, but we deny that it was being done in our presence.

9647. You deny the turning of a tap?—Yes.

9648. "Possibly the reporter mistook the turning of the tap for tightening the strings"; that again is not true?—No.

9649. You assert that Mr. Thane and Mr. Bayliss have made misstatements there?—Yes.

9650. Then that is another untrue statement.

9651. (*Dr. Wilson.*) Now with regard to the marmot, your impression was that the spinal cord had been divided?—Yes.

9652. But, of course, we are all liable to make mistakes. Can you not imagine that that marmot was suffering at the time from paraplegia, from some cause or other; animals often do, as you know; is not that possible?—I think, as Mr. Thane acknowledges, that the animal was paralysed in its hinder parts and looked like one that had had its spinal cord divided; I cannot go further than to say that that was our impression. I have said that before, and that we have not seen it after death, and had no opportunity of verifying whether it was so or not. I can only say that if it had not had its spinal cord divided, and if I had been allowed to read my statement, which I was not, I should have been able to put in some evidence to show why it was such an abnormal marmot compared with other marmots; because it must either have been in a state of hibernation, and then it could not have bitten the vivisector; or it was in the awakening stage, when it is well known that a marmot begins to shiver, and the temperature rises exceedingly rapidly, as much as 12 degrees or more in one hour, and that when that shivering has once begun nothing can stop the marmot from becoming absolutely awake, and then from running about. This marmot lay still, and was carried about, shown to the class in this peculiar state of what looked to us as paralysis, and was neither in a state of hibernation nor in a state of awakening, nor in a state of being really awake. According to the usual knowledge that one has of marmots, neither of those three states was represented.

9653. But do you not think it is quite possible that as this marmot was retained to demonstrate this extraordinary rise of temperature under certain conditions, there would not have been any object in dividing the spinal cord?—The spinal cord of marmots is very often divided. There is a great work written on the marmot alone by a well-known French physiologist, Professor Dubois, of Lyons, which is the best known monograph on the marmot, and a considerable number of his experiments are concerned with dividing the spinal cord of the marmot in various places. Under the circumstances, therefore, seeing the state of the marmot and having this knowledge of experiments on marmots, it was the most natural and in fact the only conclusion we could draw.

9654. I will not say the only conclusion, but I admit that it might have been the natural conclusion under the circumstances. But you see Dr. Thane has made a post-mortem of the marmot, and do you not think that, after his positive statement that he found no evidence whatever of the division of the spinal cord, you must really admit that your impression must have been wrong?—I gave as my reason for not admitting that, that Professor Thane made a seriously inaccurate statement almost in the same breath as he corrects us, and I cannot feel justified in accepting his statement two lines lower down when he has just made himself guilty of such a great inaccuracy as he has done.

9655. Can you not admit this much, then—that Professor Thane may have been wrong in his inference after making the post-mortem; but surely you cannot say that he did not honestly believe, when he made that post-mortem examination of the marmot, that the cord had not been divided?—But I do not think I have said that.

9656. Yes?—I do not think I have.

9657. That was my inference—that you doubted his truthfulness, not the accuracy of his statement?—I do not think I have said that he did not honestly believe that the spinal cord had not been divided when he made the post-mortem. But I simply begged to be excused from accepting this one statement as accurate when he has just made an inaccurate one.

9658. I wanted to make that a little more clear if possible. Now about anæsthesia, you contend absolutely that as a rule light anæsthesia must be more or less incomplete anæsthesia?—I do.

9659. And in anæsthetising animals so as to ensure complete anæsthesia you would insist upon the same agents being used and the same care being taken as in anæsthetising human beings for operations?—Certainly, if the anæsthesia is to be complete or reliable.

9660. And from all you have read and seen of experiments, you believe that morphia and urethane and these other narcotics are unreliable as so-called anæsthetics?—I do, and I believe I am supported in that by veterinary surgeons of much experience.

9661. And as to the struggle of animals under operations, you have seen operations, as you have stated, on human beings under anæsthetics. Whenever a patient begins to struggle under an anæsthetic, does or does not the surgeon take that as an index or indication that the patient is coming out of the anæsthetic?—He does, and measures are taken at once to ensure deep anæsthesia by adding to the anæsthetic given.

9662. So that any movements of the limbs on the operating table, whether they may be more or less purposive or not, or reflex, are indications that the anæsthesia is incomplete, and they at once take steps to give more?—Certainly; that is always so.

9663. You have seen animals operated on?—I have seen animals operated on by veterinary surgeons, and that has been the case.

9664. With animals the same care is taken?—Yes; I was referring to an operation by Professor Hobday under those conditions.

9665. A question was asked you about permission being granted to outsiders, as it were, to witness some of these experiments. Do you think that if medical men belonging to one or other of the Anti-Vivisection Societies were allowed to be present, that would be a satisfaction to the Societies? Supposing these men were accredited by the Secretary of State, after being nominated by their Societies, to be present at any operations on animals, do you think that would give more satisfaction to the Societies?—I certainly think

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that would give a good deal of satisfaction and more guarantee that those Societies have at present.

9666. If they were medical men then there could be no objection to their being present?—No, I do not think that physiologists would have any objection; at any rate they would object less than to lay people.

9667. An ordinary layman, of course, could not judge so fairly as one who had some knowledge of medicine?—No. May I add that an Association of Medical Anti-Vivisectionists is being formed called the International Medical Anti-Vivisection Association; there are already some 200 members.

9668. They are not all anti-vivisectionists, are they?—Yes, they are all anti-vivisectionists.

9669. (*Colonel Lockwood.*) In London, is this?—In England and in France. There are about 100 members in England and 100 in France; it is only at its beginning.

9670. (*Dr. Gaskell.*) With respect to that statement of Dr. Thane's, which you characterise as inaccurate, on the question of the marmot, if Dr. Thane intended to imply that no cutting experiment had been made upon the marmot before, would that have been inaccurate?—He did not say that.

9671. If he had intended to imply that, would it have been inaccurate?—It would not have been inaccurate, but he said that no experiment had been made save the taking of the temperature.

9672. My question was as to a cutting experiment, cutting the spinal cord?—Yes, but he did not say that.

9673. But that might have been in his mind?—Then he did not express himself correctly.

9674. But his statement in that case would not have been inaccurate?—No, then it would not have been inaccurate.

9675. (*Sir William Church.*) I gathered from an answer that you gave to Sir Mackenzie Chalmers that you do not think that antitoxin is of value in the treatment of diphtheria?—Yes.

9676. And then you gave him a considerable number of names of, as you said, persons who had written or spoken against its use as being dangerous?—Yes.

9677. Would you kindly give the Commission the name of any person of repute, who is held in good repute by his professional brethren and the public at large, either in this country, or in Germany, or in France, or in any other country, who, having had large experience of diphtheria, both before and after the introduction of the antitoxin, is not convinced of its usefulness?—I should be most obliged if I could be allowed to send that as a note. I will prepare a list of names of people. It is rather too wide a question to answer in a minute, because there are a great many. I have the names of a great many opponents of vivisection in general, who are also opposed to serum therapy.

9678. They must be people of repute, both in their own profession and in the eyes of the public?—If you mean by that, repute among pro-vivisectionists and vivisectionists, I do not think I can supply it, but if you mean men of repute among humanity at large I am sure I can supply it.

9679. (*Chairman.*) You cannot supply any name at present?—Yes, I can.

9680. You might do that now?—I can supply the names of the men at the discussion that took place at the Chelsea Clinical Society last year, I believe in March. Professor Wright, I believe, spoke against serum therapy to some extent.

9681. (*Sir William Church.*) Who is he?—Sir Almroth Wright.

9682. Where did he speak?—At a meeting of the Chelsea Clinical Society, and also Professor Hewlett. But, if I am asked to do it now I cannot do it with the same accuracy and care that I would if I supply a note of it. Then I believe Dr. Rosenbach, a German physician, has written against it, and Dr. Boucher, and Dr. Deswatines in France.

9683. I do not think it is any use mentioning the names here now, because what I have asked you to refer the Commission to is the names of persons of high professional and general repute who hold the view that you hold of the uselessness of antitoxin in diphtheria.

9684. (*Chairman.*) I should have thought it better, if you can mention them now, to do so, because if a note is sent in a list might be sent which we should have

no opportunity of questioning as regards the eminence of the persons, and so forth. If you can give us any information now it would probably be more useful to us?—I could mention the name of one man of eminence who has previously expressed those views, but I believe I am precluded from mentioning his name at the present moment.

9685. (*Colonel Lockwood.*) Why?—I think it is one of the Commissioners whom I should mention.

9686. (*Chairman.*) You might as well let us have the name?—If I may mention Dr. George Wilson, I believe he has expressed such views; and Dr. Boucher, in France, the secretary of the French branch of the International Medical Anti-Vivisection Association, Dr. Lutaud, Pasteur's eminent opponent. From Germany I could supply names by the hundred.

9687. (*Sir William Church.*) We only want one, the most distinguished?—Dr. Lahmann, in Germany; and I do not know whether homeopaths in England would be considered to be men of repute or not, but I believe the homeopaths stand as one man against serum therapy. I can mention the hospital staff of the Homeopathic Hospital.

9688. If you had noted my question, I asked you for the names of those who have had large personal experience of the treatment of diphtheria, both before and after the introduction of antitoxin. People who have not had large experience of diphtheria both before and after the introduction of antitoxin I do not want you to refer us to, but I want you to refer us to those who have had large practical experience of the treatment of diphtheria both before and after the introduction of antitoxin?—That condition is scarcely a fair one to impose upon me, because the men who would oppose the use of diphtheria antitoxin would be men who oppose it on principle, and on various principles, and therefore would not try it; they would rather take the experience of other men who have failed than try it themselves.

9689. That is just what I want to avoid. Now one or two other questions, if I might be allowed to ask you. At Question 7423 I asked you a question about pithing: "You understand what pithing is? (A.) Yes, of course," and you have been kind enough to-day to give us a list of names of physiologists who use the term in various ways, you say; but do not they all use it in this sense, either that they mean total separation of the spinal cord from the brain or destruction of the cerebral hemispheres or of the whole brain?—I do not think they all use it in those senses. I have already given evidence to show the different senses in which it is being used.

9690. Yes, you have given us a number of names, and I ask you whether all those authorities to whom you have referred do not use the term either as expressing the severance of the spinal cord from the brain or the destruction of some part of the brain itself, either the cerebral hemispheres or the cerebral hemispheres and something more?—Then I must answer "No," because some people use it in the proper sense, the inclusive sense, the sense to which I would agree where the word pithing would be applicable—namely, the destruction of the brain and spinal cord.

9691. That is what I say?—No, I beg your pardon, I do not think you said that; you did not include the destruction of the spinal cord in your question.

9692. No, of course I exclude the spinal cord, that is going still further; but I say: Do any of the physiologists to whom you have referred us to-day make use of the term pithing in any other sense than either the separation of the spinal cord from the brain or the destruction of either the cerebral hemispheres or the cerebral hemispheres and other portions of the brain?—I am afraid I must give again the same answer, unless I misunderstand you entirely, that some use it in the real sense of destroying the spinal cord as well.

9693. I am perfectly well aware of that; that is a different question. I will not trouble you any more on that. I understood you to say, in answer to Dr. Gaskell, that you are of opinion that the spinal cord is sensitive to pain. I do not know whether the head also feels pain when it has been decapitated?—I think Sir Lauder Brunton gave some evidence to that effect about Ludwig holding that view. (Question 7106.)

9694. (*Sir William Collins.*) Do I rightly understand that the word "pithing" is ever used to denote the condition of mere separation of the brain from the

spinal cord?—A mere cutting—a mere cut, yes, it is used in that sense.

9695. That is one sense?—Yes, but it is very rare.

9696. Is it used in the sense of mere destruction of the contents of the cranial cavity?—Very often.

9697. That is another sense?—Yes.

9698. Is it used in the sense of destruction both of the contents of the cranial cavity and of the spinal canal?—It is used in that sense, too, and my contention is that that is the proper sense.

9699. Is it your opinion that only in the third and last sense the term pithing ought to be employed?—Yes, that is so.

9700. After I concluded my questions on the 1st of May you answered Question 7627 in these words: "From my own point of view I am prepared to say that nothing which I should call useful has been obtained by experiments on the lower animals?"—Yes.

9701. If that be so, should I correctly understand you to contest the assertion of those many witnesses who have appeared before us, who have stated that various drugs and other modes of treatment as well as other useful discoveries have been made by experiments upon the lower animals?—I answered partly in that answer that it was a question of relative terms, and that my personal opinion was that nothing indispensable or in the higher sense useful had been discovered by such means.

9702. Then in that sense anything useful could not have been gained by experimentation on the lower animals?—I look upon it from the moral point of view, and I do not believe that the lesser and immediate utility which might be claimed as the result of doing something against a moral law is of real advantage. I believe that it is a greater advantage on all planes, moral and physical, to do the thing which is morally right, and that it will ultimately also be found to have been intellectually right.

9703. To take the case of Dr. Cushny's evidence, would you contest the assertion that various soporifics, such as chloral and sulphonal, have been discovered by experimentation on the lower animals?—I should not contest that.

9704. Then do you contest that they are useful?—I contest that they could not have been discovered by experiments on human beings who offered themselves voluntarily. I contest that the method of experiments upon the lower animals is indispensable. From the experimental point of view they might just as well have been discovered by experiments on voluntary human beings who are anxious to find out the truth about a drug which they believe will be of value.

9705. When you say that they might have been so discovered, it is easy of retort that they might not; and it is alleged that as a matter of fact they were discovered by experiments upon animals?—As a matter of opinion I do not think that that answer could be given, because nobody could deny that they could have been discovered by a few experiments on voluntary human subjects, provided that the experimenters had been a little more unselfish.

9706. But you do not allege that they were not so discovered?—I do not contradict it.

9707. Then as regards local anæsthetics—cocaine, for example—do you say that that is not useful or that it was not discovered by experiments upon the lower animals?—I should say that cocaine is exceedingly useful.

9708. Do you say that it was not discovered by experiments upon animals?—I do not think I should say that; but I have the same answer to give in relation to that, that it might perfectly well and better have been demonstrated on the human being.

9709. Does the same answer apply to the case of nitrite of amyl, which has been frequently quoted in evidence?—I think it applies to all cases. If I were keenly interested in the testing of drugs I would certainly offer myself for testing them. I would not do it on dogs, cats, and rabbits.

9710. Whereas they might have been discovered by testing them on human beings, you do not deny that they were discovered by testing them on animals?—I do not deny that you can discover things by experiments on animals; I think I would be very foolish to attempt to deny it, you are bound to discover some-

thing. The question is, first, whether the discovery is really valuable; and, secondly, whether the discovery could not have been made through other means.

9711. But when you make the statement that nothing which is useful has been obtained by experiments upon the lower animals, it seems to me that it is incumbent upon you to show, when it is alleged that anything useful has been so discovered, either that the thing is not useful or that it has not been so discovered?—Then I ought to have added, nothing useful which could not have been discovered by other means; because if an experiment be considered absolutely necessary, my contention is that it should not be a vivisection experiment on an unwilling victim.

9712. You also stated that you had been at the Pasteur Institute. The treatment of rabies or hydrophobia has been often alleged as an example of a useful discovery as the outcome of experiments on animals; have you anything to say in regard to that?—I have to say that I have seen long and sad lists published, amounting, I believe, to thousands of people, who have died in spite of the Pasteur treatment for hydrophobia. They are published in the "Annales de l'Institut Pasteur" and other journals.

9713. In that case do you allege that what has been discovered is not useful?—I am most strongly against the Pasteur treatment, certainly.

9714. (Chairman.) That is not quite an answer to say that you are against it; do you allege that it is not useful?—I do not consider it useful.

9715. (Sir William Collins.) In the case of drugs I understand you to say that nothing useful has been discovered by experiments on animals, although something might have been discovered by experiments on man?—Yes, the decisive experiment must be made on man.

9716. With regard to the treatment of hydrophobia, I understand you to say that it is not useful in itself?—Certainly.

9717. And with regard to the treatment of typhoid fever, plague, and cholera, do you say that useful things have not been discovered, or that they have not been discovered by experiments on animals?—In that case I should say that useful treatments have not been discovered by experiments on animals wherever the treatment takes the form of a serum.

9718. And with regard to snake poison, what answer do you make there?—I am not aware of any successful treatment that has been brought out for snake bites which necessitated experiments upon animals.

9719. With regard to the treatment of myxedema by thyroid extract, do you allege that the discovery is not useful or that it has not been made by experiments on animals?—I think that it has partly been made by experiments on animals, but the principle might just as well have been discovered by other experiments—in fact, an equivalent of thyroid extract is being made very largely, I understand, in chemical laboratories and prepared synthetically.

9720. As regards adrenalin as a mode of arresting hæmorrhage and for other purposes, do you allege that that is not a useful discovery or that it was not obtained by experiments upon animals?—I do not think that the use of adrenalin is absolutely necessary or that I should include it among that which is useful. That again is a matter of opinion whether it is useful or not. I do not consider it useful, if I answer the question plainly.

9721. In therapeutics adrenalin serves no useful purpose?—No, I consider that it is not useful.

9722. Do you think that it was discovered by experiments upon animals?—I am against all the organo therapeutic system of such drugs and the applying of drugs which is now in vogue.

9723. Do you contest the allegation that it was discovered by experiments upon animals?—I think it was discovered by experiments upon animals. A great number of experiments have been performed indeed in my presence with adrenalin.

9724. One more thing—the treatment of anthrax and the prevention of anthrax; do you think that something useful has been discovered there?—No, certainly not. I think there has been a great deal of failure. Only yesterday the papers contained an account of a new process which has been brought out by a technical chemist at Dewsbury, a purely chemical one, which promises much for the future.

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9725. (*Chairman.*) You prefer your judgment to that of Sir John Macfadyean, with his large experience upon a question of that kind, as to whether this treatment has been found a useful remedy for anthrax?—I have not heard his opinion.

9726. (*Colonel Lockwood.*) Would you not say that the discovery of the X Rays has done more for science than any discovery which has been made by the use of living animals?—I think there are infinite possibilities in the discovery of the X Rays, both for physiological investigation and the healing of disease; and in the light treatment and in all the new treatments, more than in any vivisectional treatment.

9727. And will be of more value than any results of vivisection?—Yes.

9728. (*Chairman.*) I think that concludes your evidence?—My Lord, I am not going to take up too much time of the Commission, but there are two things which I have not been allowed to answer; I will do it as shortly as I can—in five minutes.

9729. If you have not been allowed to answer I presume it was for some reason?—The first matter is with regard to a note that I was asked to supply on the paper by Dr. Brodie and Miss Cullis, and then I have an answer to Mr. Thane's statement about the dog that he referred to in his evidence on the fourth day, which is described in the "Shambles of Science"; and, thirdly, a few words about "the brown dog." There are two statements that I am very anxious to bring out on "the brown dog" in answer to Professor Starling and Mr. Thane.

9730. I hope you will give it very shortly, because these are matters which we have discussed at considerable length?—I will do it as shortly as I possibly can; I will only take a few minutes, and I will not read what I have got. The first note that I was asked to give is this: A further reference to the paper by Dr. Brodie and Miss Cullis to which I referred in my evidence shows that the animals were by no means pithed in the proper sense of the word, the spinal cord and the medulla oblongata having been left intact. Nor was it an immediately fatal operation, for such animals can breathe spontaneously and "survive for some four to eight hours." Artificial respiration was carried out during the actual experiments, not for the purpose of maintaining life, but in order to get a more uniform aeration of the blood. Then I have some comment upon that, which I do not think I will trouble the Commission with, but perhaps I may just mention what it is, as it is rather a question that touches the legal aspect of such a case. Either these animals are dead or are supposed to be dead, and then anybody can experiment upon dogs deprived of their brains—any man in the street; or they are alive, and then they ought to come under the Act. Miss Cullis, I see, had no licence when she performed these experiments, and I presume that she considers the animals to have been dead, and therefore that these were not illegal experiments. The question is of the utmost importance at the present moment, as the Act relates to experiments upon animals which are alive, and it seems that we must reconsider what it means to be alive. Then the point in Mr. Thane's evidence was in what he said at Question 1270. He said that the look of utmost agony in the eye of the fox-terrier was the result of our imagination, as "emotions are not expressed in the eye itself, but in the surrounding parts." In answer to this, I must point out that the "surrounding parts" of the eye were quite visible to us, and that it is immaterial to this particular case whether we prefer to localise the expressions of different emotions to the eye itself or its "surrounding parts." That is all I will trouble the Commission with on that point. I have now the last, and that is the so-called "brown dog." That was brought before the Commission several times, especially by Mr. Thane, at Questions 1551 to 1568, and by Professor Starling at Questions 3696 to 3705. I do not know that I shall be able to make myself intelligible now, as I leave out the greater part of my statement on this point; but there are principally two points of view that I wish to bring before the Commission; first, that Professor Starling has said before this Commission that this dog was "quite well" and "quite healthy," and that during the trial—in fact, the whole trial turned on that issue, which was presented by him and Mr. Bayliss—he said that it suffered from a disease, and its state was referred to as "morbid," and even the failure of the experiment was attributed by Mr. Bayliss to this "morbid" state. That is a contradiction to which I feel I ought to draw the

attention of the Commission. Now comes the last, and then I have done, and that is that in operating upon this dog, in subjecting it to a third vivisection, a contravention of the Act was made, for the Act does not say that you are allowed to perform more vivisection experiments on an animal under Certificate B, provided that animal is still in a state of anaesthesia; the Act says nothing of the kind, it says that when the operator has finished, when his object has been attained, he is under an obligation to kill that animal. Now Professor Starling has acknowledged that he had attained his object with that dog, and yet he handed it over to Mr. Bayliss, and it is a significant fact that Mr. Thane, the Inspector under the Act, has told this Commission that he is not quite clear whether this is legal or not (Question 1558)—it is rather a "legal point," and, further, that he does not like it quite (now I may quote a word wrongly, because I do not read it), and that is not a thing one would desire to be done (Question 1561); and, further, that if there had not been this trial he would not have known of this procedure (Questions 1563 and 1564); and that, as far as he knows, there may be cases of a similar kind which would not appear in his report (Question 1563). Those things have been told to the Commission by Mr. Thane, and I have brought it in from the point of view of what has been said on the "brown dog" before the Commission.

9731. (*Sir William Church.*) With regard to the discrepancy between the statement that Professor Starling made here, that the dog was quite well and healthy, and the statement at the time of the trial that the dog was said to be in a diseased state, what was said to be the matter with the dog at the trial?—It was said to be suffering from chorea.

9732. Canine chorea?—Yes.

9733. What is that?—It is a disease.

9734. How does it manifest itself?—It manifests itself in twitching.

9735. And it was suggested at the trial, I think, that some of the movements which might be thought to be due to pain, and which moved the board, were due to this twitching of the muscles?—Quite so, that was the defence of Mr. Bayliss and Mr. Starling.

9736. What is the condition of a dog which has canine chorea?—I have seen a dog with it—Mr. Sewell showed it to me.

9737. What is its condition?—It is a dog that is in a state of disease. You cannot refer to it as being quite healthy.

9738. For instance, if it is a sporting dog is it able to be used for sport just the same as any other dog?—I should think not.

9739. You only think that?—I have seen a dog suffering from chorea, and I am sure that such a dog could not be used. That was not the "brown dog," for I do not admit that it had chorea—it was a bulldog.

9740. Speaking of dogs which have canine chorea, are they what you would call unhealthy; they have this twitching, but are they unhealthy?—The dog that I saw was diseased and in a very diseased condition.

9741. Was that the brown dog?—No.

9742. I am not saying that a dog might not be unhealthy?—I have seen a dog suffering from chorea at Mr. Sewell's establishment.

9743. Then you think I should not be speaking the truth if I told you that I had frequently shot over a dog which had twitchings?—That dog may have suffered from a very slight form of chorea, but, as I understand, it was inferred that the form of chorea that this brown dog was said to have suffered from, according to Mr. Bayliss and Professor Starling, was a severe form; but the dog I saw certainly could not have been used for sport; and what Professor Starling implied here before the Commission was that the dog had recovered from the operation, and that it was in the same condition as it was before the operation. He did not say that it had not canine chorea, but he said during the trial that it developed canine chorea, I believe after the first operation, between the first and the second.

9744. The dog had canine chorea before it was ever operated upon at all?—No, I do not think that was stated.

9745. I beg your pardon, it was?—I beg your pardon. I think Professor Starling stated distinctly

during the trial, and I think I know that trial, that he observed the chorea in January, and he had operated upon the dog in December—he performed the first operation in December.

9746. You assert that it was the result of the first operation?—I cannot assert that. I can only assert that it was declared to have come after the first operation.

9747. Of course we can ask Professor Starling himself, but that was not the case; it was not the result of the first operation. The dog had this twitching before ever it was operated upon at all?—That is not so according to Professor Starling's evidence at the trial.

9748. That I do not know anything about.

9749. (*Chairman.*) It does not seem material to the question that we have to decide whether this operation upon the brown dog was an illegal operation or not?—I only brought it out as a contradiction on the part of Professor Starling.

9750. It is a contradiction that does not bear much on our particular inquiry; it may have done so on the trial; but you cannot ask us to disbelieve all Professor Starling's evidence, because you allege that there is a discrepancy between his saying that the dog was well and that the dog had chorea?—I think it is rather of some importance.

9751. I do not attach much importance to it myself.

*Miss L. Lind-
af-Hageby.*
5 June 1907.

TWENTY-SECOND DAY.

Wednesday, 12th June 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.

Sir W. S. CHURCH, Bart., K.C.B., M.D.

Sir J. MCFADYEAN, M.B.

Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. W. H. GASKELL, M.D., F.R.S.

Mr. J. TOMKINSON, M.P.

Mr. G. WILSON, LL.D., M.B.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. (now Sir) HENRY R. SWANZY, P.R.C.S. (Ireland), called in; and Examined.

9752. (*Chairman.*) You are a Doctor of Medicine?—Yes, I am a Doctor of Medicine of the University of Dublin.

9753. And you are President, I think I am right in saying, of the Royal College of Surgeons in Ireland?—I am President of the Royal College of Surgeons, and I am sent as representing the College.

9754. Will you tell us, before you give us their message, a little about the Royal College of Surgeons in Ireland? It is a large body?—The Royal College of Surgeons in Ireland consists of a Body of Fellows; we have 466 Fellows, and we have also a large number of Licentiates—2,908 Licentiates. The Fellows elect the Council of the College, which consists of 19, and they also elect the President and Vice-President annually.

9755. Is it one of the qualifications that they should be practising in Ireland?—No, not at all. A great many of our Fellows practise in England and in the Colonies, and elsewhere—large numbers of them.

9756. But their headquarters are in Dublin?—Their headquarters are in Dublin.

9757. And you yourself, besides being President of the Royal College, are Ex-President of the Ophthalmological Society of the United Kingdom?—Yes.

9758. And Surgeon to the Royal Victoria Eye and Ear Hospital in Dublin?—Yes.

9759. And I presume from that that you are to a certain extent a specialist in regard to the eye?—I am.

9760. You are aware that we invited the Royal College of Surgeons in Ireland, to give evidence?—Yes.

9761. And it is in accordance with that request that you come here?—It is in accordance with that request.

9762. Would you just tell us what their views are generally on the question of experimentation on animals?—It is considered that it is necessary for the advancement of medical and surgical science, in that it has introduced great improvements and advance on our knowledge and our methods in the past, and that we may look forward to its doing similar work for the good of medicine and surgery and of humanity in general in the future. That is in a general way the position which the College takes up.

9763. Those are the views of the Council?—Those are the views of the Council.

9764. Which is the ordinary mouthpiece, I suppose, of the Body?—Yes.

9765. Is that the unanimous view of the Council?—Yes, that is the unanimous view of the Council.

9766. And your own view?—And my own view, most distinctly; I should not have come unless I was prepared to back it up.

9767. As regards the use of anæsthetics and the avoidance of pain, have your Council expressed an opinion upon that subject?—Certainly. So far as it is possible, and so far as it is compatible with the success of the operations, it is necessary and desirable that every means should be adopted to prevent animals having pain, or inconvenience even, so far as it can be managed.

9768. You say so far as it is compatible with the success of the operation?—Yes.

9769. There are certain operations, as we know, which are provided for by the Act, in which the direction to kill the animal before it comes out of the anæsthesia is suspended?—Yes.

9770. What do you say to that provision; do you think that it is necessary?—Yes, certainly I do.

9771. And when you say, so far as is necessary for the success of the experiment, I suppose you are alluding to experiments of that nature?—Yes, the experiments are with the object of obtaining knowledge, and the animal should be protected from pain and inconvenience in so far as that is compatible with the obtaining of the knowledge which is proposed to be derived.

9772. Have your Council formed any opinion as to whether any animals which are now subjected occasionally, or frequently, to experiments should be excepted from the list?—It is held that there should be no exceptions from that list.

9773. That is to say, that the law should remain as it is at present?—Yes, there are no animals excepted now.

9774. There are exceptions. There is certainly exceptional treatment of horses, for example—of horses, dogs, and cats?—Yes; they require a special certificate; and there can be no objection to that; but some people propose that certain animals should be excepted, and that is what we do not agree with.

9775. That is what I understood, but I wished to have it quite clear whether you raised any objection to the present limitations as regards horses, dogs, and cats?—No, I do not think so.

9776. But you would not, as I understand, extend those limitations?—No, we would not extend those limitations.

9777. Then what do you say as to experiments upon

*Mr. (now Sir)
H. R. Swanzy,
P.R.C.S.
(Ireland).*
12 June 1907.

Mr. (now Sir) animals for the purpose of enabling surgeons, or future
H. R. Swanzy, surgeons, to acquire skill?—That is a point upon which
P.R.C.S. I have heard opinions expressed; I have made it my
(Ireland). business to inquire among general surgeons upon the
12 June 1907. subject, and perhaps when some general surgeons come
before you you may get from them more direct information, because I am not a general surgeon, as you understand; but all those with whom I have spoken, including some of the most distinguished men with us, are clearly of opinion that it would be greatly to the advantage of surgeons, and of those learning to be surgeons, if operations upon animals for the purpose of acquiring skill were permitted.

9778. Would you apply that to students, or only to those who have taken degrees or already acquired and given proof of having acquired a certain amount of skill?—I would extend it to students towards the end of their course, when they were almost surgeons, and when they were about to enter upon practice, where they would have to perform operations very soon upon the human animal.

9779. Would you have surgeons who were permitted to experiment for those purposes do so under a special licence for the purpose, or would you let it be one of the incidents of their being qualified surgeons?—I presume that it would require a special licence in this way: That the animals ought to be protected from pain and inconvenience as much in operations for skill as in operations for the ascertaining simply of knowledge; and therefore I presume it would be necessary to have a special certificate. But that is a point I have not considered; I am merely speaking in a general way.

9780. You mean that you would apply all the protections which are applied to animals as to freedom from pain to the cases of those who were experimented upon for the purpose of acquiring skill?—Yes, I would protect animals to the utmost in that case, as I would also in the case of experiments for obtaining knowledge.

9781. But I was rather asking you, would you require a person who was to get leave to experiment for purposes of that sort to have a special licence or certificate for that purpose from some authority?—I suppose it would be necessary.

9782. (Sir Mackenzie Chalmers.) It would not be allowed under the existing law at all; therefore it would be a change in the law?—I do not know what measures would be necessary, but in a general way it is held that it would be a most desirable alteration in the law if it were permitted.

9783. (Chairman.) You have not considered the question whether the authority to grant such a licence should be the Home Office or a medical authority, or both?—No, I have not at all.

9784. But you think that the law should permit experiments to persons having certain qualifications already for it?—Yes, I do. I agree with a witness who was here—I forget now who it was—who made a very striking observation, which was that he regarded the present law as immoral, because it did not permit these operations upon animals for the acquirement of skill.

9785. (Colonel Lockwood.) Was he a medical witness?—He was a physiologist.

9786. (Dr. Wilson.) It was Professor Starling?—Yes, Professor Starling. He said that he regarded the present law as immoral, because it did not permit operations for the acquirement of skill.

9787. (Chairman.) Because it sent out persons as being qualified for surgery who had not had an opportunity of acquiring that skill?—Yes, because it excluded animals from being used for the purpose of obtaining skill.

9788. Then another point upon which we have had a great deal of difference of opinion from witnesses before us is as to whether experiments should be permitted for the purpose of teaching students by demonstration. What is the view of your Council and yourself with regard to that?—The view of my Council is that it is desirable that students should be taught when necessary by means of experiments upon animals. There was one member of our Council—I wish to be perfectly candid—who seemed to be a little doubtful on this subject; but he seems to have altered his mind about it.

9789. I asked you whether that was the view of your Council and yourself. Did you agree with the majority of the Council, or did you agree with the doubts of the

other gentlemen?—I agree with the majority of the Council.

9790. Have you considered at all the question of what limits, if any, you would put upon demonstrations to students, or would you leave the law as it is?—I would not limit it any more than it is limited.

9791. So much as to the general views. You have told us that your practice and experience lie principally in matters of eye disease?—Yes.

9792. And I think you wish to illustrate your views about the value of experiments upon animals from your experience in eye treatment?—I should like to endeavour to do that if you will allow me.

9793. If you please?—Of course, the question of inflammation, and what inflammation is, has been one of the most important questions, upon which a great deal of light has been thrown within the last 50 years or so, and some of the pioneer knowledge in this matter was gained by experiments upon the eyes of animals. The cornea of the eye is peculiarly suited for observing the processes of inflammation going on, as shown by the experiments of Cohnheim and Leber. Cohnheim was one of the first who showed what pus was, and a great deal of his experiments were made upon the cornea of animals.

9794. Of living animals?—Of living animals. Experiments on the origin of pus corpuscles.

9795. Was that before the days of anæsthetics?—These experiments were done in Germany, and they were not probably quite so particular there. I do not advocate some of the methods which have been used there, but these experiments would be done under anæsthetics, the animal would be anæsthetised, of course, during the whole time, and then not killed afterwards, because you could not see what went on afterwards unless you allowed the animal to live.

9796. These are German experiments that you are speaking of?—They are.

9797. Merely for the purpose of giving us the history?—I am only giving you the history. I want to point out in what way, by experiments upon animals, we have gained knowledge concerning inflammation which is of the utmost importance to medicine and surgery in general. Other cases which I have to bring forward are more specially connected with disease of the eye; but that is a general question, and a most important one, a great deal of knowledge of which has been obtained by experiments on animals' eyes.

9798. That is to say, the discovery of what pus is and how it is formed?—Yes.

9799. What would be about the date of those experiments?—Cohnheim was in 1873, perhaps rather earlier. His first work was published in 1867, but he wrote a monograph in 1873 on the subject. I was in Berlin myself at the time, and I saw his experiments, not upon animals' eyes, as it happened, because his very earliest experiments were done upon the mesentery of frogs and the tongues of frogs, and I saw those. Then as regards the function of the ciliary body, it is very important to know what the function of the ciliary body is.

9800. Will you explain to us what is the ciliary body?—I will try to do so. The iris is the part of the eye which gives colour, when a person has a blue eye or a brown eye, and that is the only part of a certain coat of the eye that one can see. There are two other parts, the ciliary body and the choroid.

9801. Are those two films, as it were, under the colouring one?—Yes, they are behind the white of the eye. The iris you can see in front through the cornea. It was not known what part of this uveal tract, whether the whole of it or what parts of it, secreted the fluids of the interior of the eye; there are two fluids, the vitreous humour and the aqueous humour. We knew pretty well, or suspected, at any rate, that these fluids come from the uveal tract, or from some part of it, but it was not until the year 1880 that it was definitely ascertained that the ciliary body was the part of the eye which secreted these fluids, and then we immediately began to understand certain diseases of these two fluids.

9802. Were those fluids discovered by experiments?—No, they were always known, I suppose, from any remote period. When we found that it was the ciliary body which secreted these fluids—and that discovery was made by experiments upon animals—we then began to understand certain diseases of the eye which we had not understood the nature of at all before. There was

one disease of the eye which was called keratitis punctata, which we thought was a disease of the cornea; but from the time when we ascertained the meaning of the ciliary body we learned that this keratitis punctata was merely a symptom of inflammation of the ciliary body, and consequently we then went about treating the keratitis punctata in quite a different light. Then, also, there are opacities in the vitreous humour, which sometimes appear, and one wondered what they meant; and there were various ideas as to what they meant. We now know that for the most part these opacities, too, are due to inflammatory processes in the ciliary body, and consequently we now proceed to deal with them in a different way.

9803. About when would that discovery be made?—That discovery was made definitely by Deutschmann in 1880.

9804. You say it was discovered by experiments upon animals; was it discovered wholly by that, or was it assisted by clinical observation or by observations on the human eye in cases of disease?—It was discovered entirely by experiments on animals. There was no other way by which it could be done.

9805. You have something to tell us, I think, about the corneal endothelium?—The cornea is the clear part of the eye which covers the iris, between you and the iris; if you look at somebody else you see yourself in that person's cornea—it is the glittering, bright part of the eye. On the back of the cornea there is a very delicate membrane called the endothelium, and between the cornea and the iris there is a little water called the aqueous humour, of which I have already spoken. We did not know what use the endothelium was, but it was discovered by Leber in the year 1873 that the use of the endothelium is to protect the cornea from the aqueous humour—to prevent the aqueous humour filtering into the cornea. If the endothelium is damaged or injured the aqueous humour percolates into the cornea and makes the cornea opaque; and, of course, opacity of the cornea interferes with vision. That has been a most important discovery, because now when we are operating we are most careful to avoid injuring the endothelium, for if we injure it—suppose that in extracting a cataract by any chance we injure the endothelium either with our instruments or by using some antiseptic, a weak sublimate lotion getting into the anterior chamber and injuring the endothelium—the aqueous humour percolates into the cornea and makes it opaque.

9806. That might produce total blindness, I suppose?—At any rate, it would make the eye a very imperfect eye, a useless eye, and damage the sight. That is knowledge which we could not possibly have gained in any way except by experiments on animals. Previously we used sometimes after operations to have opacity of the cornea; and we did not know why it happened. Now we not only know why it happens, but we know what we should do to prevent its occurring. I have seen this thing happen myself and I did not know why it happened. It does not occur now with me, because I carefully avoid those things which would produce it.

9807. By experienced oculists I presume that is perfectly understood, and those effects are avoided now?—Yes, certainly.

9808. Could you explain any further why experiments on animals were essential to this discovery?—I am at a loss to understand how it could be ascertained in any other way.

9809. You say that it was ascertained, in fact, by that means?—Yes.

9810. You gave us the name of the experimenter?—Leber.

9811. In what country was it?—In Germany.

9812. Has he published accounts of his experiments?—Yes.

9813. I am not asking you to give them, but I suppose you would give the same answer here as you gave as regards the function of the ciliary body, that mere clinical observation could not have discovered it?—It could not have discovered it. One might have guessed, but guessing is not any use; we must have definite absolute, downright knowledge.

9814. What would be the nature of the experiment for making this discovery as regards the corneal endothelium?—There were a series of experiments, of course, but the one which was conclusive, or, at least, one of those which was fairly conclusive, was that an

incision was made in the cornea, a sharp instrument was passed into the anterior chamber of the eye, and the back of the cornea was deliberately injured—in fact, it was injured in the form of a cross; then the little wound in the cornea healed, the aqueous humour re-formed after a few hours, and soon one saw in the cornea a little grey cross corresponding with the injury which was done on the back of the cornea; so that obviously the condition of opacity in the cornea must have arisen from the wound or injury on the back of the cornea.

9815. As you say, that is only an illustration of the kind of experiments. I suppose there were a series of them?—Yes, no doubt.

9816. Then you wish to deal with transplantation of the cornea?—As I have just been saying, the healthy cornea is clear, and when opaque it interferes with vision. A great many diseases cause opacity of the cornea, whilst the rest of the eye is perfectly good for vision, so that if there were only a clear cornea the patient would be able to see very nicely. For probably fully a century or more there have been attempts made to transplant a clear cornea from the animal's eye to the human eye in such cases, with the object of providing the patient with a clear cornea and good vision. Those experiments began so long ago as the year 1831; at any rate, Reisinger in the year 1831 operated and used a rabbit's cornea for the purpose. He wanted to transplant the whole cornea from a rabbit; he took away the opaque human cornea and took the rabbit's clear cornea, and put it on instead, and healed it on, but it soon became opaque. Stilling, in 1833, tried the same thing, and there were a great many other experiments of that sort, modified by different people as they tried to think out what could be the reason of the transplanted cornea becoming opaque; but always, although it healed on, it became opaque, so that the operation was no use. Although it healed on there it became opaque. I have not thought it necessary to bring every name here; but, at any rate, it was early in the last century that those experiments began. The question seems to have been taken up again in 1872 by Mr. Henry Power, who still lives, although he does not practice in London any more, who read a paper at the Ophthalmological Congress in London that year. He had been making experiments upon dogs and cats and rabbits, with the object of discovering a good method, and he, too, found that although he could do the operation successfully, yet, when the cornea had healed on, it always became opaque. That was the great trouble, that although you could do a successful operation as such, yet the new cornea always became opaque.

9817. You mean by successful operation, one in which the human being adopted the transplanted cornea?—Yes, a surgical success, but not a success in giving the patient any advantage. Therefore the great puzzle was to do this operation in such a way that the cornea would not become opaque; and the knowledge that was gained by Leber's experiments, which showed that it was the endothelium on the back of the cornea which protected the cornea from the aqueous humour, and that when the aqueous humour percolated into the cornea the cornea became opaque; it was that knowledge which helped us to transplant the cornea from a rabbit or from a freshly enucleated human eye on to another human eye with retention of its transparencies. What was done was then the opacity was trephined off down to a certain depth and the posterior layer of the cornea and the endothelium were retained, and then a piece of the same size was taken off the other cornea with the same trephine, and put into the little hole in the cornea, where it healed on and it remained clear. This was first done by Von Hippel in the year 1877.

9818. What does trephining there mean?—The little trephine is a little circular knife—a little punch, you might say almost. A piece was punched out of the opaque corner and a piece of the same size punched out of the clear cornea, and the clear cornea put on to the opaque cornea, and then, in consequence of the endothelium on the back of the cornea not having been interfered with, as it was in previous attempts, the new clear flap that was put on remained clear. I do not say that that is an operation which is in every-day use; it does not happen in many cases that you can do it, because, unfortunately, there are not a very large number of cases where the cornea is not opaque right through. If it be opaque right through there is no use in taking away a piece of its surface, and the operation is only good

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for cases where the opacity does not go right through the whole thickness of the cornea; there is that limitation to it. But I give it as an example of the valuable progress made in surgery for the good of people who suffer from blindness of a certain kind, and as the result of knowledge gained by an experiment upon an animal. It was the same experiment, in fact, which I referred to before as so useful in letting us know that we must not injure the back of the cornea in operations.

9819. Has serum therapy been utilised for diseases of the eye?—Yes, we are beginning now to use it a good deal. I have put down in my *précis*, as my fifth item, serum therapy; but I would ask you to take that number along with No. 7 and No. 14, because they really are all more or less serum therapy. There is an ulcer of the cornea which is called *ulcus serpens*. It is an exceedingly severe form of ulceration, and eyes are very frequently lost by it; at any rate, if they are not lost as eyes, they are lost as seeing organs, because it causes opacity of the cornea; it is one of the diseases of the eye which causes severe opacity of the cornea. Our methods hitherto have been unsatisfactory; except in the mildest cases we are seldom enabled properly to save the sight of an eye in this disease. The organism which causes this ulceration is the pneumococcus, and recently a pneumococcus serum has been prepared by Römer, which has been used for treating this disease; at any rate, it ameliorates its severity to such an extent very often that it enables the other measures which we have been hitherto using to be used with greater benefit. The experiments were first made upon rabbits by Römer. That is a recent thing—1905 is the date of it. Then with regard to tubercle of the eye, which is a very prevalent and very devastating disease of the eye, we find that treatment with tuberculin is exceedingly valuable. It is useful both in treatment and in the diagnosis, because, although it is a common disease and in many instances you can diagnose it perfectly well, and anyone seeing it is able to make a diagnosis at once, yet there are cases where one is in very considerable doubt, and you have to resort to other measures than your knowledge of appearances; and by using tuberculin in a rather high dose you get certain reactions which help you to make the diagnosis. There is also a method of diagnosis which is not possible in this country at present in the ordinary way in hospitals, but which can be done, I suppose, if we get some friendly physiologist to help us, namely, if we take a little of the water out of the aqueous humour in a case where tubercular disease is suspected and introduce it into an animal's eye, say, a rabbit's eye, it produces tuberculosis of the eye if the disease which the patient is suffering from is tubercular.

9820. That would be for the purpose of discovering whether the tubercle was present in the eye of the human being?—Yes. The method of treatment of diseases of the eye with tuberculin was introduced by Von Hippel in 1904. There is another instance, No. 14 in my *précis*, which refers to a very valuable remedy called jequirity. Jequirity is the active principle of the paternoster bean, a Brazilian seed. We have in these countries, especially in Ireland, a great deal of a disease called trachoma, which is a most destructive disease to the eye. In some cases it quite blinds people, and in order to cure it we have many remedies; but one of the most valuable is this jequirity. It is in my experience a most valuable remedy, but there is one drawback to it, and that is that sometimes the reaction which it causes gives rise to a danger in the eye; it acts in some people with such violence that it endangers the cornea; and therefore we would like very much to have some means of controlling its action. It was found by Ehrlich that it was possible to immunise animals from jequirity. He devised or discovered, I do not know which word to apply, an antitoxic serum which, when introduced into the animal, absolutely prevents your being able to produce a jequirity reaction in its eye at all. Then further experiments by Römer elaborated this point, and Römer found that in human beings if this serum was introduced into the blood the individual became immunised, and that you could not produce jequirity reaction in the eye; and, further, that even if the serum was dropped into the eye without being introduced into the general system, it would prevent the patient getting a jequirity reaction; and, again, if the individual has a very violent reaction from the jequirity and you want to moderate

it, you can do so by dropping the serum into the eye. It is therefore really a very valuable remedy in that way.

9821. What is jequirity; is it a drug?—Yes, it is a drug; it is a little black bean with red spots; it is called the paternoster bean; it comes from Brazil.

9822. I think we have had some evidence that it was used simply in the ground bean?—That is true. What is done is, the skin is peeled off, and then the bean is pounded up, and you make an infusion of it, and it is this infusion which is applied to the conjunctiva, the lining of the eyelids.

9823. I think we had one case given to us where the dry jequirity itself was said to have been rubbed into part of the eye?—I suppose that probably the dry preparation would do, but clinically I always use the infusion.

9824. It was not an experiment in England; it was an experiment, if I recollect rightly, in Canada; in some place, at any rate, where our present law as regards vivisection does not apply?—No doubt.

9825. (Sir Mackenzie Chalmers.) Does the injection cause great pain?—In the human eye, yes, it is very painful; the temperature goes up, and there is considerable feverish reaction in the individual.

9826. When it is used as a remedy?—When it is used as a remedy.

9827. Still, the result is worth it?—Oh, yes, it is well worth it.

9828. (Dr. Wilson.) It is to allay that pain that the serum is given?—Of course, the serum would allay the pain, no doubt, but that is not the great object. Unfortunately, jequirity, when it gives rise to a violent reaction, may produce ulceration of the cornea. That is the danger of it.

9829. Is this serum used in Ireland?—No, we have not got it yet; it is not very easy to get.

9830. You have no personal experience of it?—I do not think you could get it in this country at all.

9831. (Chairman.) There are some other experiences of yours that you wish to give us. Sympathetic ophthalmitis is one, I think?—Yes, sympathetic ophthalmitis is a very important disease. As you know, when one eye is injured the other eye, in certain kinds of injuries, is liable to become very seriously inflamed, and both eyes may be lost. It has been the object of oculists and pathologists for many long years to ascertain what this sympathetic ophthalmitis is and how it comes about. It is, I think, an example, perhaps, of experiments which, while their results were negative, yet had their use; because I think it is a greatly mistaken idea in some people's minds, that when experiments are made and fail to prove what is wanted to be proved, the experiments have been thrown away. But really that is a wrong point of view. If an experiment fails, or a series of experiments, then it is shown that that, at any rate, is not the meaning of the thing, and that we must try in some other direction. Sympathetic ophthalmitis, it was thought at one time, was caused by the irritation of the nerves in one eye giving rise to irritation of the nerves in the other eye, and that then, as the result of irritation merely, inflammation resulted, just the same as when you get a foreign body into one eye the other eye waters very much. But this was found not to be so. Experiments upon animals were done with the object of deciding this point, too; I cannot really give the details, because it is a long time ago now. Then there was an idea that the inflammation, the bacteria or the toxins, travelled round from one eye through the optic nerve of that eye, across the structure we call the optic commissure, down the optic nerve of the other eye and into that eye, and then set up the inflammation in the latter. Many experiments were made in this direction with the object of finding that out; and, of course, the reason why so many experiments were made was because one man made an experiment and he used a certain kind of infection, and that infection did not answer, and then somebody else used another kind of infection, and so on, and the experiments were not all carried out exactly in the same way, or there would not have been any use in repeating them; they were all done in different ways by different men in the course of years, and they all failed to show that the inflammatory process extended round from one eye to the other through the optic nerve and commissure. Now we have begun again in recent years, largely by reason of the generally held

view of the infection of the blood in disease which we have nowadays; and the idea now is that sympathetic ophthalmitis comes about really as a metastatic disease; that is to say, when one eye is injured micro-organisms set up their residence in it, and in the course of time—a few weeks—they extend into the general system; but they do no harm to any organ in the general system except the second eye if they happen to go into it; and they appear to have some special attraction for the other eye, which is a very remarkable thing. If we knew for certain that this view were correct, and if we knew, which we do not yet know, what the specific micro-organism is, we could then perhaps get a serum which, when an eye was injured, we could introduce into the patient's system and immunise him and prevent him from getting sympathetic ophthalmitis. And many a person who is going about blind now, if we had such knowledge, would now be seeing.

9832. It is a very common disease, is it not?—It is a very common disease; it occurs in shooting accidents and all kinds of perforating wounds; we see it every day. The best thing we can do for people nowadays when they come to us with an injured eye, is to advise them to have it removed. But very frequently they say that they would rather not; and they go off and come back again in a few weeks with serious inflammation in the other eye; and they go about with the help of a dog and a stick for the rest of their lives.

9833. Does taking the other eye out prevent it?—Yes, it does, because it takes away the primary focus where the poisonous toxins and the poisonous bacteria develop.

9834. That is a case in which many experiments have been made with a view to discovering a remedy, but they have none of them been successful?—That is an example of a disease in which for years and years experiments were made in one direction, because it seemed so probable that the disease extended round from one eye to the other; and those experiments all resulted in a negative way. Now the new light which has been thrown upon general pathology, and so on, has led us to direct a great deal more attention to this other way of infection of the second eye; and it is by experiments upon animals that this other theory must be followed out and shown whether it is right or wrong.

9835. But all that has been done at the present time has been to narrow the field of research to show that certain fields of research are useless?—Yes; efforts are being made to prove or disprove this theory.

9836. Is glaucoma another case in which experiments on animals have been of use?—Yes, glaucoma is a very prevalent disease and a very serious one. The nature of glaucoma is that the eyeball gets too full. When there are just enough of the fluids within the eye and not too much, they keep the eyeball at a certain tension, and if you put your finger upon your eye you can feel that it is slightly soft. In glaucoma the eyeball gets so full that if you put your finger on such an eyeball it feels as hard as a marble; in fact, the intra-ocular pressure is greatly increased. The idea at first was that there was too much fluid secreted, that glaucoma was a disease of hyper-secretion; or, perhaps, if there was a low form of inflammation, it was the fluids given out by inflammation which caused the increased tension. Von Graefe, about the year 1867, discovered a cure for glaucoma, and he discovered this cure, not by experiments upon animals, but by clinical observation, because, after all, every advance that we make is not made by experiments on animals. Although experiments on animals teach us a vast deal, yet other methods are also not forgotten; and it was by means of clinical observation that Von Graefe, about the year 1867, discovered that iridectomy cured glaucoma (iridectomy is taking away a little piece of the iris), and the way in which it acted, he thought, was because it took away a little piece of the secreting surface of the eye; because at that time we did not know, as we know now, that it is the ciliary processes which secrete the intra-ocular fluids, and it was thought that it was by reducing the amount of the secreting surface within the eye that the tension of the eyeball was reduced. Deutschmann's experiments showed, of course, that the iris secreted very little, but the ciliary process secreted the main portion of it. But it was in the year 1873 (and to this I particularly wish to draw attention) that Leber discovered, by

experiments on animals, that the way of exit of the intra-ocular fluids was at the angle of the anterior chamber, the part that we call the ligamentum pectinatum; and he discovered that this is where the intra-ocular fluids make their escape; because in our eyes, normally, these intra-ocular fluids are always being secreted, and they must be also getting away, or the eyeball would become too full. It is obvious that they must be getting away at the same rate as they are being secreted; and fresh fluids being secreted, when they become used, are discharged from the eye, and the place where they are discharged is the angle of the anterior chamber, as Leber pointed out in the year 1873. A few years after that it was discovered that in the glaucomatous eyes of individuals, which had been removed for one reason or other, the angle of the anterior chamber was blocked, and then it became at once plain that it was the blocking of the way of exit of the intra-ocular fluids which caused glaucoma, and that therefore it was not a disease of secretion, as was formerly thought, but a disease of retention.

9837. Do these fluids escape, then, at the surface of the eye?—They do practically escape at the surface of the eye.

9838. (*Colonel Lockwood.*) Can you cure glaucoma now?—Yes. I was just going to talk about that. The cure of glaucoma previously had been, and continues still, for the matter of that, to be mainly iridectomy, but, as I said before, it was thought that the way iridectomy acted was in consequence of taking away the little piece of the iris. Now that we know that this is not the way the operation acts, and that it is by freeing the angle of the anterior chamber, we make an incision into that very angle, and we know that we must make this incision in a certain way; for it is not the taking away of a piece of the iris that is the important part of the operation, but it is the incision, and the way in which the incision was made. This discovery of the blocking of the anterior chamber as being the cause of glaucoma, which followed on the knowledge that that was the way of the fluids escaping has led also to the use of a very important remedy, or, at any rate, a palliative, in these cases—that is eserine, the active principle of the Calabar bean. Eserine contracts the pupil. It was immediately thought that if the angle of the anterior chamber is blocked, then if you put eserine into the eye, and contract the pupil, that will drag away the root of the iris from the angle of the anterior chamber, and in that way free the angle of the anterior chamber, and then the fluids will be able to get out again instead of being pent up in the eye. And this is a remedy which we use very much. It can hardly be called a remedy, but, at any rate, it postpones the evil day, and it is a very great aid in many cases.

9839. (*Chairman.*) Is it used successfully?—Yes, successfully. The use of eserine in contracting the pupil was discovered by Fraser, of Edinburgh. I do not know how long ago, but, at any rate, it, too, was discovered by experiments upon animals. So that here you have a number of experiments upon animals all bearing towards the cure or alleviation and understanding of one very important disease of the eye. I might say perhaps that in this country very important work has been done by Parsons, Henderson, Starling, and Paterson regarding the normal secretion and excretion of the eye; that is the branch of knowledge improvements in which led us to understand so much about glaucoma.

9840. Were those experiments of pure research, or were they addressed particularly to the cure of glaucoma?—For the most part they were experiments of pure research. Those I spoke of, by which Leber discovered the way of exit at the angle of the anterior chamber, were pure research experiments.

9841. Then I see that you propose to make some observations on two diseases, namely, cataract and detachment of the retina?—Of course, it would be very desirable to know the cause of cataract—senile cataract; and investigations by experiments upon animals are now being made on this subject. Römer has investigated the question of lentotoxins, or cystotoxins, and the protection of the lens against them in normal conditions. Hess's experiments lead him to suspect that the cause of cataract is some auto-intoxication. These are quite new; they are going on now, in fact. Römer's experiments were published in the 60th volume of von Graefe's Archives, and that is quite a

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 recent volume; but I mention these experiments because they are quite new; and they have not resulted yet in anything, but there is every reason to think that we will gain knowledge, and the experiments have to be continued. Detachment of the retina is also a very serious disease of the eye. The retina becomes separated off from the choroid coat of the eye by the intervention of fluid between the two. It was discovered by Leber in 1882 that the primary cause of detachment of the retina is shrinking of the vitreous humour. The disease is an exceedingly difficult one to treat; it is one of the most hopeless that we have, and attempts are being made to replenish the vitreous humour, by introducing the vitreous humour from animals, and some of those cases have resulted with advantage.

9842. As regards the next three matters—cocaine, adrenalin, and argyrol—I think we have had a great deal of evidence upon that class of subject, and unless you think there is anything very special regarding them, we need not perhaps go into them in detail?—I do not think that I need trouble you in that case.

9843. (*Sir Mackenzie Chalmers.*) We have not heard, I think, before of argyrol?—Argyrol is a preparation of silver, and it is used very largely for external diseases of the eye—diseases of the conjunctiva.

9844. Has animal experimentation had anything to do with it?—Yes.

9845. (*Chairman.*) You might tell us about it?—Experiments upon rabbits showed that when argyrol was introduced into the anterior chamber of a rabbit's eye it did no harm; it did not set up any inflammation or anything of that sort; and as it was known that it was good for certain bacterial diseases of the conjunctiva, a friend of mine, Dr. Mooney—one of the junior surgeons in the hospital that I belong to in Dublin—thought he would try what good it would do in a very severe case of inflammation extending into the interior of the eye in a case of injury, and he introduced in this almost hopeless case some drops of argyrol into the anterior chamber, with the result that in the course of twenty-four hours the eye was a great deal better, and in the course of twenty-four hours more was quite well. I regard that as a very brilliant result from the use of a remedy which had never been used in that way before, and which he would not have used if he had not learnt that it, when introduced into a rabbit's eye, did no harm there.

9846. It was a painless experiment on the rabbit, I presume?—I suppose so. I cannot give details. Dr. Mooney had read of it somewhere, and I asked him before I came away where he had read it, and he said he would look it up and tell me, but he had not done so before I came away.

9847. It does not bear so much upon the question of painful experiments on animals?—No.

9848. (*Sir Mackenzie Chalmers.*) But he dared not have used it if it had not been for experiments on animals?—No.

9849. (*Chairman.*) But it does not seem to involve anything in the nature of an experiment. What have you to say with regard to the Finsen light?—The Finsen light, as you know, is a very valuable remedy in certain serious superficial cancers, lupus, and so on; it is a bactericide in those cases, and it seemed that it might be useful in certain bacterial diseases of the eye. But the Finsen light, as used in the usual way for lupus, could not possibly be used for the human eye, because it would do it a great deal of harm. The eye has to be carefully protected in using the Finsen light, I believe, for lupus of the face. Yet it seemed so likely that it might be of use if its intensity could be subdued, that experiments were made in the first place to see if it would kill any of the bacteria that infect the human eye (there would not be any use in using it if it did not), such as the streptococcus, the pneumococcus, and so on. Hertel has tried upon animals' eyes whether it would kill these bacteria if used in a modified form.

9850. Without destroying the eye?—Without destroying the eye; and he has found that that can be done; consequently, perhaps, we will come to use it in this modified way for the human eye.

9851. That, I suppose, is a thing which would involve, in perfecting experience, painful experiments on a certain number of animals?—Certainly, because you must infect the animals' eyes with these bacteria in order to perform the experiments.

9852. That is a case of painful experiments which have had some useful result?—Yes, which promises to have some useful result.

9853. (*Colonel Lockwood.*) You hold, of course, your own opinion on this subject of painful experiments on living animals, apart from that held by your Council, naturally?—Yes.

9854. Do you trace your own success as an eye specialist entirely, or the greater part of it, to experiments made on living animals?—A great deal of it.

9855. Did you witness many experiments?—No, I did not.

9856. Or did you conduct yourself any experiments?—No; I am a practitioner; I merely utilise in practice the knowledge which is gained by these experiments.

9857. It was more from what you read of experiments than from anything you witnessed yourself?—Certainly. It is not necessary to witness them in order to know how to utilise the results obtained from them.

9858. You think that reading about an experiment is quite as useful to the profession as witnessing experiments?—I do not know quite what sort of experiments you mean.

9859. Experiments upon living animals?—The experiments I have spoken of here have been for the most part pathological. There were some, as the meaning of the endothelium on the back of the cornea, and the use of the ciliary body, which were physiological. But I think, of course, that it is not necessary that in every discovery in physiology which is made by means of experiments upon animals those experiments should be always seen by every person who is a doctor; that would be quite impossible.

9860. I understood you to say that you were in favour of demonstrating to students by means of experiments on living animals?—Yes, certainly.

9861. And you would be sorry to see them put a stop to?—I would, indeed.

9862. Do not you think that that knowledge could be arrived at, as it was in your own case, as much by reading as by witnessing experiments?—The experiments which are shown to students relate to the great functions of the body, some of the greater functions of the body, such as the action of the heart, peristalsis of the bowels, and such things as that. They certainly are things which ought to be taught to students in the most demonstrative manner possible.

9863. But I may take it, indeed, it is well known, that you yourself have arrived at considerable eminence in your profession, and yet you say you have witnessed no experiments?—It would have been a very good thing for me if I had.

9864. You think you would have been a still greater man?—I am sorry I did not see still more of them.

9865. Did you see any in Germany?—I saw the beginning of Cohnheim's experiments, and they certainly were something which impressed the nature of inflammation upon my mind more than anything I ever saw in my life. I only wish I had seen more. When I was a student these things were not to be seen, and therefore, of course, I was at a disadvantage.

9866. You said, of course, that you were, and I take it naturally that you are, anxious that the animals during these operations should be insensible to pain?—Yes.

9867. But you rather qualified that statement, because you said it must be consistent with the success of the operation?—Yes, that is, within the four corners of the present Act of Parliament.

9868. Therefore I take it—I do not want to put an unfair question—that your view is that the success of the experiment on the animal is of more value than that the animal should not suffer pain?—Yes, I do most distinctly think so.

9869. And you also think that these operations which you have described to us as being very successful, and which have been derived from experiments on living animals, could not have been arrived at by any clinical examination?—No, I cannot imagine them to have been arrived at by any clinical examination.

9870. Or by any operation on a dead body?—No, oh, no, certainly not.

9871. Only and solely by experiments on living animals?—Certainly.

9872. You did, I know, and very rightly, draw our attention to the fact that the use of iridectomy in glaucoma was by Von Graefe stated to have been not discovered by experiments on animals?—Yes.

9873. Which do you consider, if it is a right question to ask you, are the most skilful operators on the eye, the Germans or ourselves. I do not wish to press it. Would you say that on the whole we are on a par with the Germans. You understand why I ask the question, of course?—I cannot imagine why you ask it.

9874. I will tell you why. Because what I am going to suggest to you is that the Germans are under no restrictions, and we ourselves are under very strong restrictions; and if we are equally as good as the Germans, these restrictions on experiments on animals have not been hurtful?—But are you asking me this with regard to what I suggested, that surgeons should be permitted to operate on animals for the purpose of obtaining skill?

9875. Partly that; but partly I want to know this: Do you think that the regulations laid down by the Home Office to prevent suffering in animals have hampered our science in England?—Well, I really do not think I can say that they have. I think that they are sufficient, except in regard to that one restriction, that our surgeons should be allowed to operate for the purpose of obtaining skill.

9876. But under licence always?—Yes, I do not mind about that.

9877. But otherwise you would say that we were equally in line with the Germans as regards operations?—I do not think we can say that our restrictions have interfered, but if they were made any more strict than they are I have no doubt that they would.

9878. You think that would be a distinct disadvantage to us?—A distinct disadvantage.

9879. And you could not for one moment entertain the idea of leaving out of experiments which are allowed to be made, animals such as dogs, cats, or horses?—Or monkeys, certainly not.

9880. You think they are absolutely necessary?—I do.

9881. (*Sir Mackenzie Chalmers.*) I have not many questions to ask you. Your evidence here to-day represents, I understand, the practically unanimous opinion of your Council?—Yes.

9882. And going outside your Council, among the Fellows is there any minority, or a considerable minority, opposed to experiments on animals?—I am not acquainted amongst our Fellows with any views contrary to experiments on animals, except those of Sir Thornley Stoker upon one particular point.

9883. Those are demonstration experiments?—Yes.

9884. But we want to get evidence on both sides, of course, and I was wondering whether you could suggest to us any eminent Fellow who would give evidence on the other side?—I do not know of any of our Fellows who could give you any evidence of that sort.

9885. Practically, then, your evidence is the unanimous opinion of the Council and the unanimous opinion of the Fellows of the Royal College?—So far as I know it, and I think I would have heard if there had been any other view.

9886. Sir Thornley Stoker confines his objection to doubting the necessity of experiments before students as part of their general training?—Yes.

9887. Was he the dissident that you referred to?—Not if you mean the gentleman who I said I thought had changed his mind. I said that there was one member of the Council (*Sir Thornley Stoker* is not now a member of the Council) who rather inclined to Sir Thornley Stoker's view at one time, but that I believed he now holds that opinion no longer.

9888. Now, then, as regards the point where you suggest an amendment of the law, you suggest that students who are about to become practising surgeons should be allowed to acquire manual skill by experiments on animals?—Yes.

9889. Would it satisfy your requirements if the animal was anaesthetised and then killed before it recovered from the anaesthesia, or would you require the animal to be allowed to recover?—It would not be desirable to let the animal recover in every case. For

instance, if a surgeon wished to try some new method of suturing when a portion of the intestine had been excised, and he wished to see whether it was a good form of suture, and in many other cases. I simply give that as an example. There must be plenty of other operations where it is necessary that the animal should be allowed to live and should not be killed until an interval had taken place, in order to see what the actual result had been in the method of healing.

9890. That, if I may say so, would be a scientific experiment and not an experiment for the purpose of acquiring manual skill. As regards a young surgeon acquiring manual skill, would it satisfy your requirements if the animal were anaesthetised and killed before it recovered consciousness?—I presume that it would; but, really, that is a question which would, perhaps, be better addressed to a general surgeon.

9891. However, so far as you know, it would be sufficient?—Yes, so far as I know.

9892. I suppose eye surgery requires very delicate manual operation?—Yes.

9893. May I ask this question: Does skill become progressive?—Yes.

9894. One of the witnesses told us—I will not ask you if you agree with him—that before an ophthalmic surgeon became a really good surgeon he had to destroy a hatful of human eyes?—That is not an original saying of his; I fancy it was Dr. Mackenzie, of Glasgow, who said that long ago. But it is not true, of course.

9895. But there is still this element of truth in it, that a man's skill is progressive?—Yes.

9896. Do you think that a student, by operating on an anaesthetised animal which was killed before it recovered from the anaesthesia, would acquire skill in operating on the eye?—No, I do not. I do not think, with regard to the eye, there would be anything gained. I am not aware of any operation upon the eye, except the transplantation of the cornea, that I can think of where it would be of any advantage to the student or of an ophthalmic surgeon to operate on a living animal's eye.

9897. You think that he had better begin straight away on the human subject?—No, not straight away, but he can operate upon dead eyes. But I do not think that anything would be gained by operating on a living animal's eye. I think the operations I spoke of are those of general surgery, and more particularly those upon the intestines.

9898. How far are diseases of the eye common to human beings and to animals?—I am told by Professor Mettam, of Dublin, who is head of the Veterinary College there, that all the diseases of the human eye, except certain infectious diseases peculiar to man, also affect the eyes of animals.

9899. You have a common run of disease between animals and human beings?—Very much, it seems.

9900. I ask the question, because it has been suggested to us by certain witnesses that experiments upon animals are useless, inasmuch as an animal is so different from a human being that you can draw no useful inference. You do not agree with that statement?—I do not. One of the diseases that I have spoken of at considerable length here—glaucoma—is not at all uncommon in animals—especially horses are very liable to it; I have seen a horse myself with glaucoma, and I have used eserine for it, because it was in great pain, and relieved its pain in a few hours.

9901. Has iridectomy ever been performed upon a horse?—I do not know. I think if a horse is blind of an eye it is hardly worth doing any operation upon it except removing it. That is mostly what the veterinary surgeons do. A blind eye in a horse, or a nearly blind eye, is of no use.

9902. Horses suffer from cataract, of course?—Yes.

9903. Have we obtained any information, from observations on horses or experiments on horses, with regard to cataract?—I do not think we have.

9904. Is transplantation of the cornea a recognised operation now?—It is, but it is not very often done, because the cases where it suits are not very frequent. It is still in the developmental stage. We hope after a time to be able to use it more extensively.

9905. That, of course, does not come under the Vivisection Act; it is not done for the purpose of knowledge. It is a common operation on human

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Mr. (now Sir) H. R. Swanzy, P.R.C.S. (Ireland). beings?—Yes, but then the preliminary knowledge of it had to be gained. We could not do it on the human being until we had tried it on an animal.

12 June 1907. 9906. As regards what you were asked about yourself, I take it that your knowledge of the functions of the eye has been largely obtained by experiments on animals, and you apply that knowledge in practical surgery to the cure of human disease?—Yes.

9907. May I ask you, is trachoma a common disease?—Yes.

9908. What is the nature of trachoma? It is highly infectious, is it not?—It is very infectious; I would not like to say highly. It is a disease of the conjunctiva, the lining of the lids, and it is little bodies which form in the conjunctiva, called trachoma bodies, which destroy the particular spot in which they sit, and then the whole conjunctiva becomes destroyed and the cornea becomes opaque, too—through a growth on it called pannus—and it is for clearing the cornea that jequirity is used.

9909. Do animals suffer from trachoma?—I believe pigs do very much; I do not know about other animals; I understand that pigs do.

9910. Is it known what the source of the infection is in trachoma?—No, it is not; the micro-organism has not been found.

9911. Are there any experiments on animals at present being carried out with reference to the treatment and the source of trachoma?—I am not aware of them.

9912. Still, if it is a disease that animals can suffer from, information might be obtained, of course, from experiments on animals?—Yes.

9913. Do you operate chiefly under cocaine or under general anæsthetics?—Mostly with cocaine. I put drops of cocaine into the eye, and then the eye becomes anæsthetised, and you can operate without giving the person any pain.

9914. (*Mr. Tomkinson.*) That is a purely local anæsthetic?—Yes.

9915. That is quite effective, is it?—Yes, in cataract operations and iridectomy and such operations.

9916. (*Sir Mackenzie Chalmers.*) I only ask you for the purpose of a further question. In experiments on the eyes of animals it has been said by previous witnesses that the experiments are of a very painful character. But can the experiments which have been of use to you be carried out under cocaine; can the animal be anæsthetised?—The animal would require general anæsthesia; because it is not merely to do away with the pain, but it is also that it does not like to be restrained in any way. It is like a child. In many things that one has to do for a child one has to give general anæsthesia; whereas if you do the same thing for an adult, if you simply use local anæsthesia, the intelligent sense of a grown person will make him not resist. But a child will not be restrained, even to hold it without doing anything to it; it does not like it.

9917. Do you think that in experiments on animals cocaine ought to be allowed as an anæsthetic; or ought animals always to be put under general anæsthesia?—I would think that they ought to be put under general anæsthesia.

9918. Chloroform or ether?—Yes.

9919. (*Dr. Gaskell.*) I presume that in your student days, when you learnt physiology, there were practically no demonstrations, no experiments on animals at all?—I never saw them and never heard of them.

9920. So that naturally you would not have witnessed any in your student days?—I would not.

9921. There is one question I was wondering whether you could give us any information about—that was, with respect to certain experiments performed by Dr. Cecil Shaw for sympathetic ophthalmia. Do you know those experiments?—I know that he did perform some experiments some years ago, and they were published in the "British Medical Journal"; I think they were in connection with sympathetic ophthalmitis.

9922. They were?—I think he used jequirity in some way, jequirity beans.

9923. Were those done in Ireland?—They were done in Belfast.

9924. You had no personal knowledge of them?—I had not any except what anybody can read in the "British Medical Journal."

9925. I presume that experiments of that kind must necessarily be somewhat painful to the animal?—I certainly would suppose that they would be painful to the animal.

9926. In setting up inflammation in the eye and keeping the animal alive to see whether sympathetic ophthalmitis resulted?—Yes, it would be painful.

9927. Do you know whether the results were valuable?—No, I cannot say that I remember them well enough to say anything about that.

9928. Still, there can be no doubt that, even at the expense of some pain to the animal, it is well worth while, most important in fact, to find out the meaning of sympathetic ophthalmitis?—Most important indeed. Dr. Shaw desired by this means to find out whether or not the inflammatory process extended round by the optic nerve and commissure; but I forget now really what the result was.

9929. (*Mr. Tomkinson.*) I have hardly anything to ask you. In addition to what Colonel Lockwood asked you with regard to your qualification, as to the extent to which you subordinate the pain of the animal to the results derived from it, would you push that theory to an extreme in your opinion? For instance, supposing it was impossible to give an animal anæsthesia at all, would you place any limit upon the suffering which you thought it justifiable or right to inflict in order to promote research?—I think that the ascertaining of knowledge is to be put before the suffering of the animal; that is my own individual opinion. But I would always endeavour to the utmost to safeguard the animal from any pain, or even inconvenience, which could be avoided.

9930. So far as you know, that is practically the custom that is carried out so far as possible?—Yes, it is. In fact, so far as I know, animals do not suffer any pain during operations; and after operations which are done aseptically, as they all are, the animals also do not as a matter of practice and as a matter of fact suffer pain or inconvenience, I believe.

9931. I think that is very much the experience of human beings after a mere incision; that under aseptic and healthy conditions the healing up is accompanied by hardly any pain?—Yes.

9932. Have you any doubt in your own mind as to the possibility of keeping animals absolutely in a condition of insensibility for a prolonged period by anæsthetics?—I have no doubt in my own mind, because I know it on good authority; but I have no experience of my own to tell you.

9933. I think you have witnessed very few experiments on animals?—Yes.

9934. Is that from choice?—Not at all.

9935. Would you have any shrinking from it?—Not the slightest—no more shrinking would I have from seeing an animal operated on or experimented upon than I would have in seeing an operation in the theatre of a hospital.

9936. Then you do not look upon it at all in the light of being a cold-blooded and cruel practice?—Not at all—by no means; certainly not, because it is necessary. It would be very different if I saw an animal being stoned by little boys out in the street; I should think that a very beastly thing to witness; but I see nothing at all to shrink from in scientific experiments, carefully carried out in a physiological laboratory, with every precaution for the animal and for the success of the experiment.

9937. (*Dr. Wilson.*) You said that experiments on animals had assisted largely in the cure, or treatment, I should say rather, of glaucoma?—Yes.

9938. Is that disease ever cured?—Yes, it is.

9939. Even if it is far advanced, can it be cured?—No; when it is far advanced, if you allow a certain time to go past, you cannot do anything for it.

9940. It is only in the early stages that it can be cured?—Only in certain rather early stages.

9941. In diagnosing tubercle of the eye, I think you said that it was of great value to be able to inject the aqueous humour from a suspected eye into the eye of an animal?—Yes, for the purpose of diagnosis.

9942. But it is believed, is it not, that the tubercle bacillus is the only infecting agency, that the tubercle bacillus must be in the aqueous humour which you inject into the eye?—Yes.

9943. Could it not be developed, as it is ordinarily, and diagnosis made without having recourse to that experiment?—I do not quite understand.

9944. I mean that the tubercle bacillus, for example, in the sputum, is developed in artificial media?—Yes.

9945. And the bacteriologists never resort, so far as I know, to inject it into the eye?—Yes.

9946. Why could not the same test be made with regard to the aqueous humour which you suspect?—Because experience shows that it is not always successful. The tubercle bacillus in tubercle of the eye is very often in a very attenuated form, and you cannot cultivate it in the same way as you do the more active bacillus which occurs in pulmonary tuberculosis.

9947. It cannot be diagnosed in the ordinary way?—Very often not. I do not mean to say always, but it is often so with what we call attenuated tuberculosis of the eye. Those are the cases where you are likely to have doubts sometimes.

9948. Now with regard to the transplantation of the cornea, is it not the case that only one single instance of cure has been recorded by Zirm in 1905?—That is a very recent case. Zirm has published that case very recently, but Von Hippel published two cases on the human eye.

9949. But these are the only recorded instances of success?—Yes, but they are quite sufficient to show that the thing can be done.

9950. But have not oculists tried the experiment very often on human eyes?—I do not think so, because the cases in which it is applicable at present are not many. That is probably the reason why there are so few cases. We hope to develop it as time goes on. We have made a considerable advance in it now.

9951. The eye, of course, is a very sensitive organ to pain?—Yes.

9952. Would not the insertion of foreign substances into the eye for experimental purposes be likely to produce a great deal of pain?—Yes, they would, as a rule.

9953. And, of course, in experimenting on animals, although for cutting operations cocaine or anæsthetics may be used in the first instance, they could not be used continuously while the animal was under observation?—No.

9954. So that the animal must be more or less in a suffering condition while under observation?—No doubt there would be suffering in many instances.

9955. Then with regard to antiseptics, I suppose you have to be very careful about the use of them in operations on the eye?—Yes.

9956. So that what was called Listerism, or antiseptic surgery, never applied really to those operations. You could not operate under the spray on the eye?—No, you could not.

9957. Did you take any part in that controversy about Listerism some 25 years ago?—I did not take any active part.

9958. But operations on the eye must all along have been aseptic operations to be successful, clean operations?—Yes. We used also at the beginning to use antiseptics at one time, but they were not satisfactory; and then when the aseptic system came more to the front we were delighted to be able to abandon the other method, because the lotions which were used were often deleterious to the eye in themselves.

9959. So that what is called aseptic surgery really was not the direct outcome of antiseptic surgery as introduced by Lord Lister?—I am not so sure about that.

9960. Did he not himself own that he was rather ashamed and, with rare candour, regret that he ever used the spray?—I am afraid those are questions it is better not to ask me so much about, for I am not quite so familiar with them as perhaps a general surgeon would be.

9961. I only want to show, if possible, that eye surgery all along had to be clean surgery to be success-

ful, even before what is called the aseptic treatment came into vogue?—Yes.

9962. Of course, when you refer to serum treatment, you refer only to it in connection with eye diseases?—Yes, that is all I refer to, only in connection with eye diseases.

9963. You are not commissioned to go further?—I have a perfectly free hand; but I consider myself to be as a witness more useful when I speak of what I know in my own branch than of what you would learn better from somebody else.

9964. Is it your experience that tuberculin is valuable in cases of tuberculous eye disease?—Most distinctly. I have had a good deal of experience of it in that disease.

9965. And it is used sometimes for testing when you are not certain?—Yes, I used it so the other day.

9966. Injecting it into a patient?—Yes, I did it the other day.

9967. Has it not failed as a test with human beings; has it not been given up?—I know that it was very much out of repute for some time, but it is obviously coming back into use again now—used not quite in the way it was at first; and I know from my own knowledge and experience that for tubercle of the eye it is a most useful remedy.

9968. Then in referring to operations for acquiring skill as regards students, you refer to general surgical operations?—Yes, I do.

9969. You say they are of no value as regards eye operations?—I do not think that practically they will be of much use as regards eye operations.

9970. Then you do not agree with Professor Starling in that respect?—I do not think I can say that they would be of use in regard to eye operations.

9971. But would you go so far, then, as to advocate what is pursued in one or two colleges in America, that is to say, using dogs as patients for amputations and all other surgical operations, and treating them as patients—I mean, keeping them under observation after the operation?—I do not think that there should be any restriction upon the kind of operations that should be performed upon animals, if it is held by surgeons that they could gain skill in operating upon the human subject by that means.

9972. But would you advocate students being allowed to perform them?—If they can learn something from it towards operating upon their patients when they get their qualifications.

9973. For acquiring operative skill?—Yes. I believe, however, that the great gain would be in intestinal surgery more than what you mentioned just now, amputations, and things of that sort.

9974. (*Dr. Gaskell.*) Might I ask one question in connection with something you have just said? Do you distinguish different parts of the eye as to the amount of pain that is likely to be caused; are all parts of the eye equally sensitive to pain?—Oh, no.

9975. The vitreous humour, for example?—That would have none.

9976. The retina?—There is no pain.

9977. The internal chamber of the eye, unless enlarged by pressure?—The internal chamber of the eye is the vitreous humour, and it is not painful. There is no pain in the vitreous humour.

9978. Is not the pain in the eye mainly a question of the cornea and the conjunctiva?—The cornea and the conjunctiva; and the iris is very painful to be touched, and the ciliary body is very painful to be touched.

9979. (*Sir Mackenzie Chalmers.*) There is one question I forgot to ask you; have you any opinion upon this point—whether licences and certificates ought to be granted to physiologists who are not medical men? Have the Royal College any opinion upon that point?—I have not heard it spoken of.

*Mr. (now Sir)
H. R. Swanzy,
P.R.C.S.
(Ireland).*

12 June 1907

Mr. E. A. SCHÄFER, LL.D., SC.D., F.R.S., called in; and Examined.

Mr. E. A.
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9980. (*Chairman.*) You have been appointed by the Royal Society of Edinburgh as their representative?—Yes. I have also been appointed by the University of Edinburgh as their representative. I was not specially summoned to-day on behalf of the University of Edinburgh, but if it is convenient to the Commission, I can give evidence on behalf of the University of Edinburgh as well.

9981. To a certain extent, I suppose it covers the same ground?—Practically it covers the same ground.

9982. You have been kind enough to send us a print of what I take to be the message of the Royal Society of Edinburgh?—Yes.

9983. Could you give us just a short account of that Society. Is it a Society under Royal Charter, like the Royal Society of England?—I presume so. I do not know much about the history of the Society, but it is organised on exactly the same lines as the Royal Society in London.

9984. That is sufficient. Therefore it devotes itself to science generally, not to any particular science?—Yes, to science generally.

9985. And, amongst others, to physiology and pharmacology?—Amongst others to physiology, pharmacology, and the medical sciences generally.

9986. Has the body which requested you to come and give evidence a council?—Yes.

9987. Can you tell us what that council is; is it elected by the general members?—By the general members annually.

9988. How many are there on the council?—I cannot say positively. I should think about 15 or 20, including vice-presidents and the president.

9989. Are you a member of the council?—I am.

9990. Were you present at the meeting at which these views were drawn up?—I do not remember being present at the meeting.

9990a. Does this statement represent the unanimous view of the council?—I understand that there was no other opinion expressed at the meeting. I believe it represents the unanimous view of the council as the council was then present—of course, I cannot speak for every individual member; and it has been more than once before the council. Although I am not quite sure whether I was present at the first meeting of the council at which the subject was considered, I was a member of a sub-committee which was appointed to draw up this statement. I did not draw it up myself. I was not the convener of the committee, merely a member of it.

9991. As this represents the views of the Royal Society of Edinburgh, perhaps we had better take it as it is printed. Would you kindly read it?—"Statement by the representative appointed by the Royal Society of Edinburgh to appear before the Royal Commission on Vivisection.—The Royal Society of Edinburgh, being deeply interested in the advance of all branches of science, values the opportunity which has been given to it of expressing its views upon the important question which is at present being considered by the Royal Commission on Vivisection. From very early times the experimental method of investigation has been practised in physical and biological science, and it may be regarded as affording the most fruitful means of obtaining accurate information in both of these fields. In the latter department this method necessarily involves experiments on living organisms. The whole fabric of physiological knowledge rests on such experimentation, and those who are conversant with the general trend of physiological research, know that results of the highest importance to the welfare of mankind have thereby ensued. From the time of the discovery of the circulation of the blood to the recent brilliant results which have so greatly enhanced our acquaintance with the functions of the brain and nervous system, the history of this branch of science reveals a long list of remarkable discoveries achieved by this method of inquiry. The knowledge which has been thus acquired has had a material effect in procuring the cure of disease and the relief of suffering, and has thereby been the means of saving and prolonging innumerable lives. In the field of pathology, the advance which has been made by means of the experimental method has been no less striking.

Within the last thirty years the methods of the physician and surgeon in combating disease have been completely revolutionised. It is now possible to attenuate and perhaps destroy the virulence of many of the deadly complaints which afflict mankind. A new science—bacteriology—has sprung into existence, largely through experimental effort, and a new light has been shed upon many departments of pathology which were formerly obscure. Tubercle, diphtheria, yellow fever, tetanus, hydrophobia, anthrax, malaria, sleeping sickness, typhoid fever, and other diseases, among which I would include Malta fever, Nagana, and plague, "are now better understood, and by aid of the information gained by experiments on animals, most of them can be successfully grappled with." "The relation of man to his surroundings has thus been modified, and he now enjoys a degree of immunity from many of the dangers by which he is surrounded. In the sciences of pharmacology and therapeutics no advance in accurate knowledge is possible without experiments on animals. The action of drugs must be first tested in them, and the physician is guided by the information which is thus obtained. The Society, while fully admitting that every care should be taken that in the prosecution of experiments on animals no unnecessary pain is inflicted, is, at the same time, of opinion that the imposition of any further restrictions would impede the progress of physiological and pathological research, and would be attended by serious results to the progress of medical science and to the well-being of the community at large. Further, the Society would strongly deprecate any attempt to draw a distinction between experiments which are designed for utilitarian purposes and those which appear to have a purely scientific object in view. The history of science shows that it is not possible to draw such a distinction, seeing that so many scientific discoveries, which at the time they were made appeared to possess no utilitarian value, have in the course of time proved to be but the beginning of lines of investigation which have led to the acquirement of knowledge of incalculable benefit to humanity."

9992. Those are the views which have been submitted to the Council and approved by them, I understand?—Yes.

9993. The Council consisting, I suppose, of gentlemen who may be said to be representative of a considerable number of sciences?—Of all departments of science.

9994. I see at the end they advert to the desirability in their opinion of allowing experiments for the purpose of what we call pure research as opposed to research in order to discover a remedy for a particular disease; but I should like to ask you a little further: have your Society expressed the opinion as to whether or not experiments on animals should be used for purposes of demonstration in lectures to students?—No, that is one of the points to which I referred when I drew your attention to the fact that I was here to represent both the University of Edinburgh and the Royal Society. I consider that evidence on this point would be better tendered on behalf of the University of Edinburgh.

9995. Then I will not ask you anything further about the Royal Society, because I understand that these are their views expressed, and as they are not specially a medical body the importance of their view, I think we should all be of opinion is, that it is the view of scientific men generally on the value of experiments?—Yes.

9996. And specially in this case?—Yes.

9997. Perhaps any other evidence which you have to give we may take as being given on behalf of the University of Edinburgh?—I think you may take it as being given also on behalf of the Council of the Royal Society, the greater part of it at least, because any other evidence I have to give is supplemental to their statement. I have been given more or less a free hand by the Royal Society to give evidence on their behalf.

9998. Are you yourself in practice in Edinburgh?—I do not practise medicine.

9999. As a physiologist?—Yes, I am Professor of Physiology in the University.

10000. And therefore the Royal Society of Edinburgh are perfectly well acquainted with your views?—Quite.

10001. And with the nature of your experiments and generally?—I think so. Most of the work which we do, or a large part of it, is brought before the Royal Society of Edinburgh, and is published in their Proceedings and Transactions. It is partly brought before the Royal Society of London, and published in their Proceedings and Transactions, and partly in other journals.

10002. Having told us that, and that you have a free hand from the University and practically from the Royal Society too, I might take the rest without speaking to you as a representative purely but as giving your own evidence?—Quite so.

10003. With regard to what I was asking you about, demonstration before students, do you yourself demonstrate before students?—Yes.

10004. Has it been your practice?—That has been always my practice.

10005. Has it been the practice in Edinburgh generally for the Professor of Physiology?—It has been the practice in Edinburgh ever since physiology was taught in Edinburgh, so far as I can ascertain. It was the practice of my predecessor, Professor Rutherford; it was my practice at University College, London, when I was Professor there, and it has been the practice at Edinburgh since I have been there.

10006. And I take it from that that you approve of the practice?—I not only approve of the practice, but I do not think it is possible to teach physiology properly without showing a certain number of experiments to students. I do not consider that students can properly appreciate the importance of certain branches of the subject unless they actually see experiments performed upon animals.

10007. Could you at all classify what those subjects are, whether there are any special kind of subjects that require it more than others?—Yes. One subject is the action of the heart, the effect of the action of the heart in maintaining the pressure of the blood and the effect of certain nerves in influencing the heart and in influencing the blood vessels. The mere reading or hearing about these effects is an absolutely different thing from seeing them. I can myself very well remember the influence that was produced upon my mind when I first had shown to me by an experiment the effect of stimulating the vagus nerve upon the heart.

10008. Was that in London University?—That was in University College, London, when I was an advanced student.

10009. Who was the Professor then?—The Professor was Professor Burdon Sanderson, who was then Professor there of Practical Physiology. I have noticed myself the great interest, the keen interest, which intelligent students take in these experiments; they are vividly impressed by them in a manner in which they could not be by any narration of the results. For example, while observing the heart in an animal, the animal being, of course, in a condition of complete anaesthesia and absolutely insensible to pain, if one stimulates the vagus nerve and sees the heart immediately stop, not in contraction but in absence of contraction, in inhibition, produces a profound impression upon the mind regarding the influence which nerves are capable of exerting upon the organs of the body. And so with regard to secretion if one has placed a cannula in the duct of a salivary gland and on stimulating a certain nerve observes the secretion pouring from the gland, the impressions which are caused and the interest which is aroused, are altogether out of proportion to the effect which is produced when a mere narration of the fact is made by a lecturer. Such facts are so strongly impressed upon the mind by observation that they are never forgotten; they remain as definite accumulations of knowledge in the mind of the student. There are a certain number of fundamental experiments which I hold that physiologists ought not to be deprived of, and which should always be exhibited to students. One of these I have just mentioned, the action of the vagus upon the heart. I would add the action of the sympathetic nerve upon the heart and blood vessels, and the action of the depressor nerve upon the central part of the nervous system which presides over the blood vessels. There are also, besides the experiments upon secretion, which I have already mentioned, which is due to Claude Bernard the remarkable experiments of Bayliss

and Starling upon pancreatic secretion. These gentlemen have shown that the pancreas is capable of being stimulated to secretion by the injection into the vascular system of an extract from the mucous membrane of the intestine which contains the principle to which they have given the name of secretin. This principle, when it is injected into the blood vessels provokes an extraordinary flow of pancreatic juice, and they have shown without doubt that this is the natural mechanism for secretion of pancreatic juice in the body. The material is poured into the blood from the mucous membrane of the intestine, passes to the pancreas, and causes secretion of the pancreatic juice. Before their experiments we practically knew nothing of such chemical stimulation of glands, and they have opened up a whole vista of enquiry, and enabled us to take a much wider view of the subject of secretion than it was possible to take before. Then there is the subject of internal secretion. It is only within comparatively recent years that we have understood that certain organs in the body, the functions of which were previously unknown, have the property of passing into the blood substances which circulate in the blood to the different organs of the body, and which affect the nutrition of the body generally, or secreting glands, or the vascular system, or the nervous system. This subject of internal secretion is one of the very greatest importance. I know of no more striking experiment for demonstration than the very simple one of injecting into the blood vessels of an animal, in which the blood pressure is being taken, a small dose of an extract which is obtained from the supra-renal bodies. If a minute dose of a solution containing the active principle of the extract (not more than one part in 100,000, or less) is injected into the blood vessels of the animal it is seen that the blood pressure is sent up to an extraordinary height, the heart is stimulated to greatly increased activity, and a whole series of phenomena are produced which are due to the action of this drug upon the muscular and other systems in the body. Certain other internal secreting glands are also known to produce effects of a similar or slightly different nature. One of these has only quite recently been discovered; the influence which is exerted by extract of the pituitary body. The effect of this when injected into the blood vessels is, like that of the supra-renal, to cause a very marked contraction of the blood vessels generally, while it differs from that of the supra-renal in producing, not an accelerated action of the heart, but a steady- ing of the heart, a slower action of the heart, the force of which is, however, well maintained. And it has quite recently been found that, in addition to this action, the pituitary body has a specific effect upon the kidneys, so that if the kidneys are not at the moment secreting there may be an immediate and rapid secretion of urine. It is well known to surgeons that as the result either of the anaesthetic which is employed or of the surgical operation itself the kidneys often do for a time cease to secrete. Under these circumstances, if the kidneys in an animal are in this condition, the secretion being in abeyance, a small dose of this extract of the pituitary body produces an immediate and rapid secretion of urine. There can be very little doubt that this discovery will have an important influence in connection with the views which we hold regarding the secretion of urine, and in all probability will have an important influence upon the treatment of cases of suppression of urine. This is an experiment which can be shown with the greatest ease to a class, and upon an entirely anaesthetised animal; and I have myself noticed the impression that it produces in the minds of students who see it, an impression which cannot be produced by any amount of lecturing or display of diagrams. One other point I would specially mention as of importance for exhibition to men who are to be engaged in the practice of medicine and surgery, viz.:—the effect upon the movements of the body of stimulating certain parts of the brain. It is now well known in consequence of the experiments of Ferrier that when certain parts of the brain in monkeys are stimulated certain definite movements of the body are produced, and this discovery has had a far-reaching effect in moulding our views upon the functions of the brain. The original discovery was made by Fritsch and Hitzig in the dog. But it was owing to Ferrier's application of their methods to the monkey that physicians and surgeons have been enabled to apply to man results which have been obtained upon animals, because the conformation of the brain in the monkey, although

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simpler than that of man, is essentially similar, whereas the conformation of the brain in the carnivora (as the dog and cat) is different, and one cannot say that such and such a part of the brain, such and such a convolution, or such and such a fissure, represents exactly another convolution or fissure in man. In the case of the monkey and the ape we can make that assertion with the utmost positiveness, and now that these experiments have been confirmed and largely added to by the experiments of Sherrington and Grünbaum upon the brain of the chimpanzee and ape generally, their importance in relation to the practice of surgery and medicine in man has come out even more decidedly. This is a class of experiment which also makes a vast impression upon the student; he sees that what he has been taught in the lectures is not a myth or a fable; that it is an absolute statement of fact; he sees that the brain can be played upon, as it were, much in the same way as a musical instrument can; that by stimulation of this part a movement of the eyes is produced, by stimulation of that part a movement of the mouth, by stimulation of another part a movement of the finger or thumb, or of the toes. He sees for himself that this is so, and when he afterwards comes across cases in his practice in which there are convulsions due to some irritation of the brain, beginning in this, that, or the other part of the body, he knows what part of the brain is likely to be affected. These experiments—there are not many of them—do not require a large number of animals, because the experiments upon the heart and blood pressure and the effects of extracts and of the excitation of certain nerves can be performed in a single animal. The experiments upon secretion can be performed on one or two animals, the experiments upon the brain can be performed upon a single animal. Although a large number of facts can be shown, a very small sacrifice of animals is required. I may say that I have never found that horror of these vivisectional experiments which has been asserted by many anti-vivisectionists. It is entirely absent.

10010. (Colonel Lockwood.) Among the students?—Among the students; it is entirely absent.

10011. (Chairman.) We have had some witnesses who have told us of the great horror that is excited, and others have said, at least one or two, that great levity is excited. What have you to say with regard to that latter assertion?—I was not aware that levity was alleged, except in the book the "Shambles of Science," but I did not myself take that seriously.

10012. I do not think any other witness has said so?—Levity would not be permitted, I may say at once, by any class of students.

10013. Have you seen any tendency to that in lectures that you have either given or attended?—Never. The students with whom I now have to deal, and those with whom I had to deal at University College, were for the most part serious students, most anxious to learn. I do not mean to allege that there is not amongst such a class of students a man here and there who is desirous of finding something humorous in anything that a lecturer may say; and there are even some lecturers who even occasionally raise a laugh, but never in connection with vivisectional experiments. Levity might be shown at a lecture, but at the actual experiment and at the results of the experiment levity is impossible. Nobody who knows the class of students who study in Edinburgh could suppose for a moment that this would be tolerated. There is a remarkable public feeling amongst the students in Edinburgh which makes itself felt in the conduct of the classes. If your class sees that you are desirous of teaching them and of showing them experiments which are intended to add to their knowledge and to elucidate the facts which you have been teaching them in your lectures, they are absolutely quiet, and there would be no difficulty whatever, certainly during these experiments, in hearing the proverbial pin drop.

10014. Has that been your experience in London as well as in Edinburgh?—That was always my experience in London, but my experience in London is not of so great importance, because the classes in London were small in number, and a small class can be more easily controlled; but when one is dealing with large classes such as we have in Edinburgh one cannot control each individual student.

10015. I asked the question about London specifically because these accusations of extreme levity were made with reference to London demonstrations?—I should think things must have altered very much since my time if that were possible, because I never found in my long experience at University College the least tendency to levity.

10016. Both as student and lecturer?—Both as student and lecturer. I have now been teaching physiology in one capacity or another for 37 years, and I have a very large experience of classes of physiology and a very large experience of demonstrations and experiments, and I have never in a single instance found that there was the least tendency to levity when an animal was under experimentation.

10017. (Chairman.) You had just finished what you were saying about demonstrations, but I wanted to ask you this question. I understand from your evidence on that point that you think demonstration is useful for the purpose of showing to students decided facts, facts which are ascertained?—Yes, fundamental ascertained facts.

10018. And entirely ascertained facts?—Yes, entirely ascertained facts.

10019. Not matters of research?—No.

10020. And that, in looking at it in that light, it is not necessary to repeat the operations on many animals?—I do not understand you, because all experiments that are shown to students are repeated.

10021. But at one lecture you would experiment upon one animal?—Yes.

10022. To show the particular action of a particular drug, we will say?—Yes.

10023. You would not repeat that before the students on a number of animals?—No.

10024. But only at another lecture where you had occasion to show the same demonstration?—Yes.

10025. There have been some suggestions made that a number of animals are used up and thrown aside and others produced. What do you say with regard to that suggestion?—That is contrary to fact. No doubt the number of animals used for demonstration varies in different institutions. I imagine that in some they use more animals than we do in Edinburgh, because it is possible in some places, where there are fewer to see the experiment, to show more things, for there are some things that the student must be very close indeed in order to see; whereas in Edinburgh, where we have a large number of students, we show experiments to an audience of from 80 to 100.

10026. Is demonstration, in your opinion, useful with an audience as large as that?—Yes, in a specially constructed theatre. Our theatre is constructed exactly like an operating theatre. An operating theatre is constructed with a very steep auditorium, where the audience stand and look immediately down upon the operating table.

10027. But how far would the furthest person be?—He would not be much further than the length of this room.

10028. Say 20 feet?—The length of this room is a little more than 20 feet, I think.

10029. Say 30 feet?—I could give you the dimensions of the theatre,* but at any rate it has been arranged like an operating theatre, so that every member in the auditorium should be able to see most things. But in addition to that my practice is to allow the audience to file past. If, for example, the experiment is on the action of the heart or the pressure of the blood, and is going on continuously, they file past afterwards so as to be quite sure that they see the details of the experiment.

10030. But you could not, surely, at a distance of 30 feet, get any very useful idea as to whether certain secretion was being distilled from a certain vessel?—Yes, certainly, you can see the dropping quite easily; and the dropping is further recorded—there are special means of recording the falling of drops. The drop falls upon a lever, which effects electrical contact, and this moves another lever, so that the audience can not only see the dropping fluid, but can see its record on the smoked paper which is being moved along by clockwork. All this can be seen quite well in my demonstration theatre.

10031. Then there is another matter that I want to

* The distance from the furthest bench to the operating table is 15 feet. E. A. Schäfer.

ask you about. What do you say to the claim that is made by some, that experiments should be permitted for the purpose of gaining skill in operation?—I think that is rather a question for the surgeon than for the physiologist. I am speaking as a teacher of surgeons, but not as teaching them surgery.

10032. But still you are able to form an opinion?—Yes, I have formed a very decided opinion that it is in the highest degree desirable that any new operation should, if possible, be performed upon a suitable animal. For example, I know quite well that the eminence of Sir Victor Horsley as a brain surgeon has directly arisen from the experiments which he performed upon the brains of monkeys at University College many years ago. He would be the first to acknowledge that his reputation as an expert on that branch of the subject has arisen from such experiments. And obviously it would be an improper thing to attempt a new operation upon, say, the human intestine, without having tested the operation upon animals. I cannot conceive of this not being done. It would only mean the sacrifice of patients. I understand that the present law forbids it, and I am surprised that it is so. I had forgotten that that was the law myself, although I have studied the Act; I think that that portion of the law ought to be repealed. I suppose it was not contemplated in those days that such operations as are performed nowadays would be possible.

10033. What sort of operations do you think this sort of practice would be necessary or useful in, because the last witness, whose speciality was eye disease, seemed to think you would gain nothing for the purposes of operation on the eye from studying from the living object?—I am not able to say anything about the eye, because I am not an expert upon the subject, but I should have thought otherwise from general considerations; at any rate, I would rather myself not be operated upon by anybody who had not previously operated upon the eye in either animals or men?—I would not like to be the first person practised upon.

10034. Upon a living animal?—I would rather that the individual had operated upon other men.

10035. Everybody must take their chance of that; but I was speaking about a man beginning to practice operations on mankind without having previously experimented upon animals. The last witness said, I think, that in the matter of the eye, experiment on living animals was not an essential thing?—I really do not know. I suppose he has tried it himself. I suppose you asked him that question.

(*Sir Mackenzie Chalmers.*) He had not done it himself.

10036. (*Chairman.*) No, he said that he had not?—I cannot conceive, if the operation is done in precisely the same way, that an amount of knowledge might not be got from operation on living animals which could otherwise only be acquired by operating on man.

10037. But it would involve an immensity of practice upon animals if it is necessary, because I suppose you would say that a surgeon ought to experiment upon animals to acquire skill for every operation he would be likely to be called upon to perform?—I do not think so at all. I should not think so for amputation, for example, for one moment.

10038. Because amputating a rabbit's leg is not much use for amputating a man's leg?—No.

10039. But I was speaking rather of internal operations?—For such an operation, as in resection of a portion of the intestine, I do not think it is necessary nowadays to operate first upon animals, simply because the methods of operation have been so perfected, that anyone who has assisted in an operation will probably be able to do it perfectly well afterwards himself.

10040. What class of operation are you speaking of particularly?—I am speaking of any new kind of operation. Supposing, for example, no resections of intestines had ever been done before, then it would be absolutely necessary to try the experiment, first of all, upon an animal. It was done, of course, first upon an animal.

10041. But supposing that by vivisection of animals it had been ascertained that a particular operation might safely be performed in a certain way upon the human frame, what would you say then?—I do not think it would be necessary then to make experiments on animals in order to acquire manual skill.

10042. That meets very much what you say?—I do not think that it would be necessary.

10043. You are speaking about a man who is going to perform for the first time a particular operation—not necessarily limited to the very first time, but practically an entirely new operation upon mankind?—Yes, who has thought out a new method.

10044. Without having tried it upon an animal?—Yes, who has thought out a new method and wishes to apply it to mankind; but he does not know for certain what the result may be.

10045. But that confined it very much to a question of vivisection of animals simply for the purpose of acquiring knowledge?—Yes, it does amount to that.

10046. It is not operations for the purpose of acquiring manual dexterity?—The operations with that object, which I would have suggested myself as useful, would be eye operations; if eye experts think otherwise, I would not put my opinion against theirs.

10047. Then I only want myself to ask you one further question, and that is as regards anaesthetics. You have had, of course, very large experience in experiments upon animals which come under this Act, which requires complete anaesthesia?—Yes.

10048. I should like your opinion as regards both your own experiments and any experiments that you have seen performed by others, as to whether or not complete anaesthesia is always secured?—In all experiments which come under licence, complete anaesthesia is secured during the whole period of the experiment, however many hours it lasts.

10049. It has been suggested that complete anaesthesia may be obtained at first, but that animals are occasionally allowed to get out of that state of anaesthesia, and that if any more is applied it is too late, and after the animals have shown marked signs of pain by struggling, and so forth. What is your observation upon that?—That is not my experience at all. I do not mean to say that an animal will never make a movement when it is under anaesthetics; of course an animal will as a man will. But the object of the experimenter is to keep the animal under anaesthesia; his objects are likely to be entirely frustrated if the animal moves; and although the condition of anaesthesia may be more or less complete the amount of anaesthesia used in physiological laboratories is in almost every case vastly deeper than that which is used in the operating theatre. It does not matter very much to us whether the animal dies of anaesthesia or not; we lose our time only.

10049A. It has to die before it comes to?—Yes, but it does not matter to us very much, and we are much more inclined to push the anaesthetic because it is of the utmost importance that there should be no movements whatever, inasmuch as the least movement of the animal will entirely upset the delicate apparatus that we have to use. And the suggestion that we knowingly, or even unknowingly, allow the animal to come out of the anaesthesia is, on the face of it, an absurd suggestion, because it would absolutely defeat the object of our experiment.

10050. Have you observed in any operations that you have seen performed, either when you were a student or afterwards, any indifference or carelessness on the part of operators as regards anaesthetics?—I have never observed it. And I would not confine that answer to this country. I have witnessed a great many operations, in Germany and other countries, and the statements which fly about regarding the callousness of foreign physiologists to the sufferings of animals are wild, and quite unconfirmed by anything which I have seen. I myself worked for three months in Professor Ludwig's laboratory at Leipsic, where Dr. Gaskell was also working at the same time, and the system of anaesthesia employed was a very delicate one indeed, and had the result of rendering the animal absolutely quiescent during the whole of the experiment.

10051. That is in a country where it was not compulsory by statute?—That is in a country where it was not compulsory, and a country which has been specially vilified with regard to this subject. And I have seen at congresses of physiologists large numbers of operations performed, always under full anaesthesia. So that I think statements of that sort must be received very carefully indeed. There was a time no doubt, before anaesthesia was fully admitted, when surgeons in this country ridiculed the idea of

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anæsthesia and for some time refused to employ it. There may have been a reminiscence of that in early experiments, but that is long before my time. Within the period covered by this Act, I may say that I have never seen any operation performed without anæsthesia. Of course, I have seen operations in which the animals have been allowed to recover, and then the anæsthesia has passed off; but the actual operation I have never seen conducted without an anæsthetic.

10052. Are you speaking of this country?—I am speaking now of this country. I am also speaking of other countries.

10053. But assuming that in this country an anæsthetic has always been applied, the suggestion rather is, that it has not been applied fully and carefully, and so as to ensure complete insensibility to pain?—That suggestion has no grain of foundation.

10054. (*Dr. Wilson.*) May I take it generally that the same kind of experiments are resorted to to demonstrate your lectures as have been described to the Commission by Professor Starling?—I think generally, yes. I do not think I show quite as many as Professor Starling does, for the reason that I have mentioned.

10055. You use, of course, a large number of frogs for demonstrations?—Almost all our frogs are killed before we use them. I do not think that frogs are ever used which are not killed.

10056. The question has been raised, and I wish to put a few questions to you on the subject. You never, of course, submit any frog to an anæsthetic apart from the insensibility which is secured by pithing as a rule?—No, I think not. I have chloroformed them, but as a matter of fact they are killed. I do not think we ever use frogs which are not killed.

10057. Would you kindly describe the kind of pithing to which you resort?—The actual meaning of the word pith is the destruction of the medulla oblongata. I believe that to be the original meaning of the word. A pithed ox, for instance, is an animal in which a stab has been made in the medulla oblongata and the animal drops dead because its respiratory centre is destroyed; but by a pithed frog we generally mean complete destruction of the brain and spinal cord.

10058. Of both the brain and the spinal cord?—Yes. If I were to tell my assistant to pith a frog I should expect him to destroy the brain and spinal cord.

10059. Do you illustrate the experiments relating to circulation on a live frog?—Yes, on a frog with the brain destroyed.

10060. I think you gave evidence before the previous Commission?—I did.

10060a. I have been looking up your evidence, and I think you said that "in experiments upon the heart and circulation, it would not in many instances be allowable to remove the brain, because the removal would cause too great a shock to the animal"?—That is a mistake.

10061. You would like to correct that now?—If it is necessary to correct it, because it is perfectly certain that you can observe all the phenomena of the heart in circulation, even if the brain is removed. Without a qualification it was an incorrect statement.

10062. I made a note that it is in answer to Question 3865?—It depends upon what you mean by the word "brain." I was there meaning the whole of the brain, including the medulla oblongata.

10063. But not the spinal cord?—The spinal cord of course must be left, and the medulla oblongata for some experiments upon the heart and circulation must be left, and this is reckoned a part of the brain by anatomists. In that answer I have not the slightest doubt that I intended to include the medulla oblongata, but in the general popular signification of the term "brain" we mean the cerebral hemispheres. The popular signification of the brain is the cerebrum; and that, of course, can be removed without influencing the circulation, but you cannot remove the medulla oblongata without influencing the circulation. I have not the slightest doubt that in that answer I intended the brain to be understood to include everything within the skull; in the anatomical sense it is right; in the popular sense it is wrong.

10064. Then you contend that mere decerebration without destruction of the medulla oblongata renders any experiment upon a frog painless?—Absolutely.

10065. That is a point which has been raised, and it is disputed. For instance, it is admitted that a frog can jump and swim, and even, it is said, after it has returned to fairly normal conditions, can breed when the season comes round, even if deprived of its brain?—I do not see at all why it should not. That does not mean that it could have any conscious sensation. I do not know of any authority which holds that an animal deprived of the cerebral hemispheres can have any conscious sensation.

10066. It merely leaves the question open?—Yes.

10067. I do not want to press the point, but you yourself honestly believe that if a frog is only decerebrated it is deprived of all feeling?—I do.

10068. Now with regard to experiments on the larger animals. You of course have to use a good many dogs and cats?—Yes, in some experiments.

10069. Have you any doubt or difficulty at all in being able to conscientiously comply with the provisions of the Act, when you are carrying out even the most prolonged and severe operations before your students?—With anæsthetics, do you mean?

10070. Yes?—None whatever.

10071. You can always make sure?—Quite.

10071a. Would you be prepared to certify, on your soul and conscience, as they say in Scotland, that all experiments carried out on dogs, cats, rabbits, and other animals before your class are carried out to the best of your endeavour and belief, painlessly?—Yes, I would, certainly.

10072. It was stated by Professor Starling that in operating upon dogs to demonstrate a lecture, opportunity was occasionally taken to teach students methods of restoring or reviving the animals when the heart ceases to beat on respiration cases?—In consequence of anæsthesia?

10073. Yes?—In consequence of an excess of anæsthetic?

10074. Yes—that opportunity was taken to demonstrate to the students how the animal might be brought to, as it were?—Yes.

10075. In administering chloroform to patients, when an impending danger of that sort is created, is not the anæsthetic stopped at once?—I should hope so.

10076. Regardless of all risk of pain?—As long as there is insensibility there cannot be any recovery of consciousness to pain.

10077. But the anæsthetic is stopped at once?—I presume so.

10078. So that in a contingency of that kind, when you are operating upon an animal, you could not positively say—you could not be certain that sensibility may not have returned, if you are bringing the animal to?—You are assuming that the anæsthetic has been pushed to an extent in which the animal is practically dead, the heart or respiration having stopped, and that we are endeavouring to resuscitate the animal after that?

10079. Yes?—Absolutely the first thing to do is to shut off the anæsthetic. The animal having already absorbed such a large amount of anæsthetic, what you have to do is to get rid of some of it from the system. You then perform artificial respiration until the animal begins to respire of its own accord, and the moment that it does that you stop the artificial respiration, you allow it again to inhale the chloroform and air, and you go on with the operation. The animal has begun to respire, but its consciousness has not returned.

10080. How can you be certain of that?—We test consciousness in animals or in man by touching the cornea; that is one of the most sensitive parts of the body; and if we find that there is no reaction after touching the cornea, no closure of the lid, we know that there is no conscious sensation.

10081. What would be the agent that you would use—adrenalin?—No, artificial respiration.

10082. But for injecting into the veins you have mentioned adrenalin?—Yes, but I do not think we would apply that in this case. I think we would simply trust to artificial respiration.

10083. I think in connection with Dr. Oliver you were the discoverer of adrenalin?—More or less.

10084. But this adrenalin which is derived from the supra-renal capsules has failed, has it not, as a cure

for disease of these capsules—Addison's disease, for instance?—I have no knowledge. Addison's disease is a complicated disease. I do not think it is confined always to the supra-renal capsules; it is often due to phthisis, which affects the supra-renal capsules; and I do not think you would expect under those circumstances the giving of adrenalin to be of any pronounced benefit, but it is not a subject that I am competent to speak about; it is a clinical subject.

10085. Do you often use curare in this class of demonstrations?—Never. I have not used curare at all for years. I have not been engaged in any experiments which require the use of curare.

10086. It is very difficult to obtain it, I believe?—I believe it is difficult.

10086a. Would it not be very difficult to decide as to the completeness of the anaesthesia if curare were used along with chloroform?—Not if you maintained the same dose of chloroform. If you give curare you always give chloroform through an apparatus which delivers the regulation dose—a certain amount of chloroform mixed with a certain amount of air.

10087. By means of a Woulff's bottle?—There are special apparatus constructed for that purpose. I have devised one myself (which I think is better than the Woulff's bottle) which shows on an index the proportion of chloroform and air. What we do is this: we first chloroform the animal in a box or under a bell jar, when it is deeply under chloroform we put a tube into the trachea. We then connect that tube with this apparatus and pump air into the lungs—air saturated with chloroform, and air without chloroform. We observe that that mixture is sufficient to entirely anaesthetise the animal and to prevent any reflex action on touching the cornea. Then we can go on indefinitely with that mixture. I do not use curare. I have for years conducted no experiments which required the use of curare—which required complete paralysis of the voluntary muscles. We go on indefinitely without altering the apparatus. The animal will be kept alive for an hour, two hours or three hours, as the case may be, and will not move, and therefore we know that if we did give curare the animal would be equally anaesthetised.

10088. But have you not found it sometimes rather difficult to use chloroform for dogs with safety?—I made a special series of investigations on the action of chloroform and on the way in which chloroform kills. Those experiments were made with Dr. H. J. Scharlieb, and were published in the Transactions of the Royal Society of Edinburgh a few years ago. We found that we could with perfect security give chloroform in certain proportions, and that we could even increase the proportion to what would otherwise be a dangerous extent, if in the first place we added to the chloroform a small amount of alcohol, and also if prior to the administration of the chloroform we gave a small dose, a very small dose, of atropin under the skin. The way in which chloroform kills, unless it is pushed to an extremely dangerous extent, to which no administrator would ever think of pushing it, is by its action upon the nervous centre for the vagus nerve. It causes a direct inhibition through the vagus of the heart, which inhibition is so powerful that the heart stops, and it is very difficult indeed to get the heart to beat again. That effect of chloroform can be entirely abolished by the prior administration of a small dose of atropin, and we have very strongly expressed the opinion that in every case of the administration of chloroform there should be a prior administration of atropin in this manner. Whether this will become the practice of the medical profession or not I do not know, but it is my invariable practice to administer, especially to dogs, a dose of atropin, so that if they should get an excess of anaesthesia they shall not be liable to be killed by it.

10089. I have here a copy of a report of the Suspended Animation Committee in which I see an experiment described on a mongrel setter?—On what page?

10090. It is page 16: "Tube tied into trachea"?—

10091. In that case was an anaesthetic being administered at all?—"Anaesthetised with chloroform; $\frac{1}{4}$ grain morphia also given hypodermically."

10092. "Tracheal tube occluded for 4 minutes 20 seconds"?—Yes.

10093. But during that time the anaesthetic could not be being delivered by your apparatus?—A much stronger anaesthetic was being administered to the animal during that time—carbon di-oxide.

10094. You were asphyxiating it?—Obviously. It was absolutely insensible, and under any circumstances it takes several minutes for an animal to recover from chloroform.

10095. Here is another experiment on page 17: "smooth retriever," which was "anaesthetised with chloroform but died whilst being put under"?—Yes.

10096. That was one of the accidents?—Yes.

10097. You do meet with those?—Yes. No atropin was used in these experiments, it would have invalidated the object of the experiments.

10098. Then on page 19, Experiment IV., a "Dalmatian female." "This animal died whilst being put under chloroform." That is stated, is it not?—Yes.

10099. Then at the bottom of the same page—Experiment V.—a fox terrier." "The animal took the anaesthetic very badly"?—Yes.

10100. I am noting these to show that it is very difficult in some cases with dogs to safely chloroform them?—That depends. In these cases it did not matter very much whether the animal got an overdose of chloroform or not, because if it did we would take another animal. Our great object was to have the animal very thoroughly anaesthetised, consequently we took no special precautions to prevent its being killed by the anaesthetic otherwise than observing it carefully and endeavouring not to give a dose which would actually kill it. But the great object was to have it anaesthetised as deeply as possible. And I should explain that the expression "The animal took the anaesthetic very badly" means that the anaesthetic rendered the respiration rapid and shallow, so that there was a fear of the animal dying, not that it was imperfectly anaesthetised; it was rather too much anaesthetised, and went over rather too quickly.

10101. For those experiments, of course, you had a special licence?—No, those were done under the ordinary licence.

10102. Then there is Experiment XXXII., on page 48, "Moderate-sized mongrel terrier, male." "In this case also, as in the last, no anaesthetic was administered"?—Yes.

10103. That was one of your drowning experiments?—Yes.

10104. I suppose that was one of the cases in which you acted under a special licence?—Yes under a special certificate. I should like to explain that. In this series of experiments, which were undertaken in order to determine exactly what happens during death by drowning, all except two (and the total number, I think, was 36) were conducted under the influence of complete anaesthesia during the whole time of the experiment; but it was of the highest importance, that one could appraise the value of these experiments, to do a certain number of control experiments in order to observe whether, so far as could be determined, the phenomena would be the same without an anaesthetic as with an anaesthetic. I therefore got permission from the Home Secretary to do ten experiments without anaesthetics, and I did two of them. The results which were obtained with those two showed so conclusively that the anaesthetic did not invalidate the object of the experiment that I left the other eight experiments, and did not perform them at all; and these are the two to which I refer. In these two experiments the animals were simply drowned by being held under water and not allowed to recover at all; and the obvious phenomena, such as the pulse and respiration, were observed and a *post-mortem* examination was made in order to see whether the *post-mortem* conditions were the same when they were drowned without any anaesthetic as with an anaesthetic.

10105. Would you briefly describe your method of restoring, or resuscitation, as it is called?—I will with pleasure. It simply consists in laying the patient upon the floor or ground, kneeling by his side, placing your hands flat on the lower part of the back of the chest, and allowing your weight to fall upon the chest, so that the weight of your body presses the air out of the lungs. Then when you raise yourself the pressure of your body—the weight of your body, is removed, the resilience of the chest wall causes the chest to expand again, and air is drawn into the lungs. By repeating these movements a sufficient number of times per minute, from 12 to 15, you can completely respire a man, so that he will not feel any necessity

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for performing respiration himself. This I have shown by an experiment which I performed upon a gentleman who was working in my laboratory upon snake poison. He was led to believe that if, in cases of snake poisoning, artificial respiration were performed for an hour or two, the poison might have opportunity of eliminating itself, and wished to know whether by this method one could maintain respiration for as long as an hour. He allowed me to perform artificial respiration upon him for a whole hour without himself at any time wishing to respire, or taking any active part whatever in respiration. This method of performing artificial respiration was entirely suggested by the results which I had obtained in resuscitating dogs which had been drowned. I found that with dogs, which also have a resilient bony cage like man, it was possible to perform artificial respiration, simply by squeezing the bony cage, and placing the animal in a prone position, so that all fluid would flow out from the mouth, which was allowed to be dependent. Simply pressing was sufficient to drive out the water and air which was in the lungs, and in the mouth, and then the resilience of the bony cage would cause the air to pass in again.

10106. But could you make fit comparisons between experiments on dogs and experiments on man with Sylvester's method, for example?—I have tried Sylvester's method, of course.

10107. With dogs and man?—Yes; and I found it was impossible by Sylvester's method, either with dogs or man, to maintain respiration. It is quite impossible for you to allow another person to apply Sylvester's method for any considerable time; you must breathe; you do not get enough air into the chest by this method. You cannot do all the complicated motions involved in Sylvester's method in the space of time which is required to effect the requisite exchange of air per minute. You cannot do it efficiently. I have repeatedly tried it upon individuals, and found that it was quite impossible by it to get them in such a condition that they had not a *besoin de respirer*—a desire to breathe—a necessity for breathing. That is not the case with this pressure method, applied in this way. There is, as you doubtless know, another pressure method—Dr. Howard's method. Dr. Howard was an American who applied pressure when the body is on the back; his is also an efficient method; but has certain dangers. One is that in applying the pressure to the ribs you are somewhat liable in old subjects to break the ribs; and, as a matter of fact, I have been told by a gentleman who was present at a demonstration by Dr. Howard upon an elderly man in Dublin, that he did actually break some ribs. Another danger in cases of drowning, which came out very strongly in these experiments on drowning dogs, is that in drowned persons the liver is enormously congested, it is much too full of blood, and extends far below the ribs, and, if you press upon this part of the abdomen and chest, you are liable to rupture the liver; this would be a serious danger in applying Howard's method to cases of drowning. The method which I have explained to you, in which the patient is placed upon his face and pressure is applied to the back, has been applied in three or four cases of drowning with success by my students; to whom we teach it now always as a regular part of the curriculum. Two of these occurred at the seaside; these were cases of apparent drowning during bathing. One case in particular, of which a student told me, was at Shanklin. One of his fellow bathers got into difficulties and sank, and was not pulled out for nearly ten minutes. This student immediately applied the method, and in a quarter of an hour the man was up and walking away. An unfortunate case occurred to my knowledge, in which the method was being applied by one of my students. There were slight signs of recovery—I do not say that the patient would have recovered, because there are sometimes slight signs of recovery which pass off. But a doctor who knew nothing about the improved method came up—he only knew Sylvester's method—he ordered the patient to be turned over, and then set to work with Sylvester's method, when the symptoms of recovery immediately disappeared.

10108. But have you tested Sylvester's method and your own method spiro metrically on man?—Yes.

10109. And your own method shows that there is a greater respiration?—Yes, it shows that there is a much greater respiration; neither Sylvester's method nor the Marshall Hall method gave as much

air as we normally take in with ordinary respiration; whereas both the Howard method and my own method gave a sufficient amount. It was tested very carefully spiro metrically.

10110. And do you say that you could not have evolved your own method without experiments on dogs?—As a matter of fact, it was suggested in the course of these experiments. I would not say that I could not have evolved it without those experiments.

10111. Now, I want to ask you a few questions about those brain experiments to which you have referred. You have performed a great many experiments on monkeys, have you not?—Yes.

10112. And Sir Victor Horsley, of course, and Dr. Ferrier, in this country, besides a number of physiologists on the Continent, have performed experiments on brain localisation?—Yes, and there are a good many more in this country than you have mentioned. There are Professor Sherrington's experiments, for example.

10113. And Grünbaum's?—Yes, with Dr. Grünbaum. They have been very suggestive; and Dr. Beever's with Sir Victor Horsley.

10114. Have not many of the results been rather contradictory, and practically of very little value in brain surgery?—I should think not; but you must ask a brain surgeon about that. Sir Victor Horsley will give you the best evidence on that point. They are not contradictory, so far as animals are concerned; I will answer for that.

10115. Experimentation on animals has done nothing for insanity, for example. That is on the increase?—I am afraid it has not.

10116. Or epilepsy?—Probably not. You must apply to a clinician; I do not know.

10117. Perhaps it is not within your knowledge, but I have a note here of a statement made by Dr. Ferrier, who said at the British Medical Association meeting in Edinburgh as recently as 1898—that was long after his own experiments were carried out?—Yes.

10118. "The treatment of intra-cranial tumour forms rather a melancholy chapter in therapeutics"?—You must ask somebody else about that.

10119. You do not know about it?—I know nothing about it.

10120. I have several other instances of the same kind here of which you personally have no knowledge?—Absolutely none.

10121. But as regards these investigations of yours, is it not of interest to you to see how they are applied?—It is of immense interest.

10122. But you have not really time to read?—Yes, I read; but I am no authority on the subject. I could not say anything either for or against a statement of that kind.

10123. Will you take it from me that at the same meeting Dr. Ferrier further said that only 7 per cent. of cranial tumours are capable of being surgically dealt with?—I will take anything you like from you; but it is no good asking me that sort of question; you must ask a surgeon, or a neurologist.

10124. You see all these experiments are constantly paraded as being of such immense value to humanity and to the profession in medical and surgical practice?—I cannot gauge their value, or their want of value, I am afraid.

10125. Well, with regard to the operations themselves on animals, is not even your own mapping out of brain functions disputed by other physiologists?—No, I think not—only on unimportant details.

10126. I will ask you this question. Is it not logical to infer that as there is such a wide difference in the size, conformation, and arrangement of convolutions and fissures between the brain even of a monkey and that of a man, that useful deductions as to localisation of the functions of the human brain, based on experiments on a monkey, must be liable to a considerable amount of error?—No, I think not; I think the analogy is fairly complete, as has been shown by cases in which the brain has been exposed in man. In such cases it has been repeatedly found that excitation of the same or the corresponding part of the brain in man to that which yields a certain result in a monkey will give the same result. That has been repeatedly shown in man.

10127. In all these experiments on monkeys the operations themselves are no doubt carried out painlessly, but, at the same time, a great deal of pain must be suffered afterwards when the animal is under observation?—There is no sign of it, absolutely none.

10128. But you cannot say that there is none?—I can only say that there is no sign of pain. The animal is in a perfectly normal condition, eats its food, and runs about the cage, except for such paralysis as we may have produced as the result of the operation. If there was any sign of pain we should immediately kill the animal; but we have never had occasion to do this since the introduction of antiseptic methods.

10129. Have not experiments on the brains of animals been carried out ever since the time of John Hunter, during the whole of last century?—And long before that. Experiments performed before the introduction of antiseptics may well have been painful.

10130. (*Sir Mackenzie Chalmers.*) By reason of the after results?—Yes, because inflammation was then possible. But there is no such thing now. There is no sign of inflammation when you experiment on the brain. The conditions are perfectly normal.

10131. (*Dr. Wilson.*) But you cannot say that there is no pain?—You can only say that there is no sign of pain, no sign of *malaise*, no sign of illness whatever. You would be surprised to see how well these animals are, and look.

10132. Do you still believe it is necessary to continue these experiments on animals?—When we come to that question you must understand that there is no finality in knowledge. Our knowledge is an ever-widening circle, and as it widens there are more points to investigate.

10133. But I am referring to useful knowledge applicable to medicine and surgery?—All knowledge is useful; but what shall be applicable to medicine or surgery we cannot at the moment say. Who would have thought that Pasteur's experiments upon fermentation would be applicable to medicine and surgery? Who would have thought that there was useful knowledge in them? And yet that has led to the whole of Listerism.

10134. No?—Yes, directly.

10135. No?—Yes. Pasteur's experiments on fermentation led directly to Listerism.

10136. But by Listerism I understand the use of the spray?—Nothing of the kind. That is a mere detail.

10137. Really. I have seen Lister operate, but he afterwards gave up the spray. That was entirely the method?—That is not Listerism.

10138. We will have that out with some other witness. But was not all aseptic surgery a revolt against Listerism? There was a revolt against Listerism you know?—But aseptic surgery is the most complete carrying out of Listerism.

10139. But Listerism is antiseptic surgery?—No, certainly not. The principle of Listerism is the elimination of micro-organisms; you can eliminate those by antiseptics or by asepsis.

10140. Well, I did go into that discussion at the time, and, as I understand Listerism, it was what was called antiseptic surgery?—You must ask a modern surgeon.

10141. But, you see, you make a strong statement?—Yes, and I am quite prepared to stand by everything that I say. My experience is derived from animals alone, but that experience is very extensive, and I have never seen any sign of pain in an animal which had been anaesthetised during the operation, and treated either antiseptically or aseptically afterwards. There are certain parts of an animal that we would not treat antiseptically, because the antiseptic itself might be a source of irritation; but, as a general rule, it is safe to apply a certain amount of antiseptic to the outside of a wound, because it ensures, especially in animals in which one cannot cleanse the skin so thoroughly as we can in man, that there shall be no invasion of germs from the outside. But the principle is the same. The principle is the exclusion of germs. In one case you kill them, and in the other case you prevent their entrance, but the essential principle of Listerism is the exclusion of germs. There is no question about it, I should think.

10142. But did not Lord Lister use the spray to kill the germs?—Yes, because he thought that the germs were more universally present than they are. He thought that they were universally present in the air, and that he must exclude air which had not been treated with antiseptics. But he has himself long since recognised that that is not necessary.

10143. I will not press that question, then, further, as you do not pretend to be a surgeon?—But I do pretend to understand something about that subject, because I deal with it every day.

10144. But you do not seem to have read up the subject very closely. Have you even seen Lord Lister operating under the spray?—He does not operate at all now.

10145. Have you ever seen him operating under the spray?—No, I have never seen him operate at all. I have operated myself under the spray, but I do not operate under the spray now.

10146. Was there not a revolt against Listerism, as it was called?—It was falsely called Listerism, if it was so called.

10147. No, it was called Listerism—the use of the spray, the use of antiseptic dressings and antiseptic ligatures. All that was included under the term Listerism?—Ligatures were not necessarily antiseptic. They were treated by antiseptics, but they were aseptic ligatures. But Listerism is the exclusion of germs from wounds. That is how I should define Listerism. How do you define Listerism? I am not examining you, I admit; but still, as part of the discussion you ought to tell me how you define it.

10148. I define it as I was taught. Killing germs in the air is Listerism.

10149. But was there, then, no revolt against Listerism?—No.

10150. But the revolt against Listerism was carried on by surgeons, who operated on the peritoneum?—I would not call it a revolt against Listerism; I would call it a modification of the method.

10151. I will not press this question further; I will get it out of someone else?—I would certainly rather that you asked a surgeon.

10152. (*Dr. Gaskell.*) Have you found that by giving atropin with chloroform you have always been successful?—Do you mean to say that it is impossible to kill the animal?

10153. I mean have you ever had any deaths?—Not with careful administration. Without atropin, with the most careful attention you may have deaths.

10154. Is that method of giving atropin more or less confined to your laboratories, or is it used elsewhere?—I think it is chiefly confined to our laboratories.

10155. You gave us a number of experiments that you advocated for students. Do I correctly understand that that included all that were advisable, or only samples?—Those are samples. I think other experiments might be shown, those are some fundamental experiments which I jotted down just before I came here.

10156. I noticed that you made no mention of respiration experiments?—One naturally shows respiration experiments along with the others. When you investigate the action of the vagus upon the heart you record its action upon respiration at the same time. I did not mention respiration experiments, I admit.

10157. I meant rather Hering and Breuer's experiment?—I am not in the habit of showing that.

10158. You do not consider it important?—I consider it important, certainly, but as a matter of fact I am not in the habit of showing it.

10159. Do you consider that the demonstrations that you do at Edinburgh are more or are less than what are done in other places?—I think they are rather less, because we have so large a number of students to deal with that as it is we are compelled to divide our classes for demonstrations into four batches, and we are compelled to repeat the same experiment four times.

10160. Have you read "The Shambles of Science"?—Yes.

10161. Have you noticed the nature of the demonstrations given there?—Yes.

10162. In your opinion were those justifiable?—Which are you referring to?

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10163. All those quoted in "The Shambles of Science." Perhaps you do not remember them?—I am afraid not. Do you mean to say exactly as they are described?

10164. No; I mean the kind of experiment, the object of the experiment. There were experiments, of course, for advanced students?—Yes, they were experiments for advanced students. I may say that my experiments are none of them for advanced students. My experiments are experiments to the whole class. All students come to them.

10165. (Sir Mackenzie Chalmers.) Of what year?—They come to them in their second year.

10166. (Dr. Gaskell.) Do you give another course for advanced students?—We give a course to advanced students, but they do not have demonstrations in that course.

10167. Not special demonstrations?—Not special demonstrations; they come to the others.*

10168. In your time at University College was such a course for advanced students ever given as is described in "The Shambles of Science," or is that an innovation?—I think it is an innovation. There was not such a complete course given for advanced students in my time.

10169. Do you think it is a good innovation?—A very good innovation.

10170. Do you think that more care ought to be taken as to finding out the status of students before they go to those demonstrations?—I do not quite understand what care is taken there. It seems to me, from what I have read and what came out in the case of Bayliss v. Coleridge, that reasonable care was taken that the students should be *bonâ-fide* students and should be advanced students. They were recommended by the teachers in their respective schools. The University of London is quite different now from what it was when I was there. They now admit students from all schools to certain demonstration lectures. In my time, only my own students, every one of whom I knew, came to the demonstrations.

10171. I should rather like to know from you whether you would advocate any change in the law as it is at present administered or stated in the Act?—Perhaps I may read a statement with regard to that.

10172. I should like you very much to make a statement, because I hoped that I should have heard one already?—The statement is this. Although the Act has not been without benefit, because it has given to all intelligent laymen a definite assurance that no wanton cruelties (such as were basely insinuated before the system of licensing and inspection was instituted) are possible, and the reports have shown that all experiments are carried out with the least possible amount of pain, and in 99 out of 100 cases are absolutely painless from beginning to end—it must be admitted that it somewhat tends to hamper research. There is often too great a delay in the allowing of certificates, and by the time the inhibition to use them is removed (the normal period of inhibition being one week according to the Act, and the actual period of detention being often very much longer, at the discretion of the Secretary of State), the best opportunity for conducting the experiments may have lapsed. It was quite clearly the intention of the Act that all certificates should be granted by two eminent medical or scientific authorities, and although power was taken to disallow or suspend a certificate which had been granted, nevertheless this was obviously intended only to be used in case of any abuse or infringement of the Act. The Act nowhere gives power to the Secretary of State to impose any conditions on the certificates, but this power has been illegally assumed by successive Secretaries, and certificates are thereby materially modified and their usefulness seriously impaired. I would like to give an instance of that which occurred to myself. I mentioned two experiments which I performed on animals in connection with the subject of drowning. I asked for permission to perform certain experiments without general anaesthesia, because I was anxious to determine whether every condition was exactly the same without anaesthesia as with anaesthesia; accordingly I asked for permission to put a cannula into the artery of a dog's leg with complete local anaesthesia—cocaine, and during the drowning to make an experiment upon the blood pressure in that dog. I obtained a certi-

ficate for that purpose, but there was written over that certificate, "The preliminary operation which is suggested under local anaesthesia is hereby disallowed." (or words to that effect.) I consider that that was an illegal action on the part of the Secretary of State, but I had no remedy because the Secretary of State would simply have disallowed the certificate altogether, and I was compelled to do the best I could under the circumstances. This I put forward as an instance of a research, and an important research, being hampered by the authority of the Secretary of State acting, so far as I am able to ascertain, without expert advice and, indeed, contrary to such advice. To enforce this illegal position the threat of suspension of the certificate is held over the head of the applicant. Further, all applications are referred, first, to the Association for the Advancement of Medicine by Research; and, secondly, to the Inspector under the Act. I do not object to a confidential reference to the Association for the Advancement of Medicine by Research, because that Association is composed of experts on the subject who are very capable of forming a judgment as to the expediency of proposed experiments, and their judgment can only have the effect of strengthening the position of the Secretary of State when he is called upon to decide on any doubtful question; nevertheless, it must be conceded that even this is clearly outside the four corners of the Act. On the other hand, the Inspector has to do with the administration of the Act, and is not necessarily an expert upon the subject of physiology or pathology or pharmacology; he has no special knowledge of what is or is not necessary for the advancement or knowledge in the special branches of these subjects; his duties are defined by the Act, and consist in visiting registered places for securing a compliance with the provisions of the Act. Personally, I have not a word to say against any advice which may have been given by the present or past inspectors in Great Britain; but the abuse of the position of the inspector has come out very glaringly in the evidence of the Inspector for Ireland, whose advice seems to have been acted upon for a time in opposition to that of the granters of certificates. I have studied the Act carefully, and I can find nothing in its provisions which warrants an appeal being made from the experts who are authorised to grant certificates to officials who are not experts in the science and are not conversant with the necessities of the investigation proposed to be employed. That is what I have to say about the present restrictions. I would strongly deprecate, on behalf both of the Royal Society of Edinburgh and of the University of Edinburgh, the imposition of any further restrictions whatever. Any restrictions on scientific work are detrimental to its progress, and further restrictions of experiments on animals might prove disastrous to the progress of physiology and medical science in this country. Since the revival of scientific medicine in the last two centuries we have taken a leading part in contributing to its progress. In no other country in the world is more regard paid to the ethical aspect of the treatment of animals, and the care which has always been exercised by physiologists in this country to prevent pain in the performance of experiments, is a reflection of a general sentiment which animates all class of society. (The callousness of the laity generally with regard to the sufferings which are inflicted upon animals intended for the table, or which are involved in pursuit of pleasure, is due, I believe, in the majority of cases, to sheer ignorance.) Nevertheless, I have not found any disregard of animal suffering to prevail amongst physiologists and medical men of other countries, in spite of the fact that in some countries the people generally are far more callous regarding animals than are we ourselves. It is strange that this should be the only country in the world in which restrictions are placed on the use of animals for the purpose of advancing medical and scientific knowledge, in spite of the fact that there is overwhelming evidence that the actual amount of pain inflicted is infinitesimal compared with the results achieved and compared with that which is daily and hourly inflicted in the pursuit of pleasure or of commerce. Even quite recently a clause was inserted in an Act of Parliament prohibiting stray and unclaimed dogs seized by the police being employed for the advancement of medical knowledge. The silliness of such a regulation is self apparent, for these animals are condemned to die, and are in many cases killed by a process of suffoca-

* Mr. Schäfer subsequently wrote to say that he is informed by Dr. Sutherland Simpson, the University Lecturer on Experimental Physiology, that this year for the first time he has shown a single experiment on a rabbit, under Certificate C, to the advanced class of Experimental Physiology.

tion; at the best they are killed by the same anæsthetic as would be employed in the laboratory; and the result is that other dogs have to be killed in the laboratory and the number of animals sacrificed is increased by so many. Who benefits by this? Certainly not the canine race.

The necessity of employing dogs has been emphasised by others who have given evidence, but it cannot be too strongly insisted upon. No other animals are available for experiments in important fields of physiology. Among these I would instance experiments upon the heart and circulation, upon the secreting glands and the digestive system, upon the kidneys and the formation of the constituents of urine, upon lymph formation, upon metabolism, and lastly experiments on the nervous system, in certain of which animals which can be trained must be employed. Dogs are the only animals which are available for many such experiments.

10173. Do not you think it would be a good plan if animals which are kept alive under Certificate B were not necessarily obliged to be kept in the laboratory in which they are operated upon; that they might go to a farm, for instance?—I was not sure that they were necessarily kept in the laboratory.

10174. We have understood so here; that they are obliged to be kept in a licensed place?—Certainly, if it is the case it is unnecessary and might be detrimental. As a matter of fact, I do not think I have understood that. I do not know whether I have infringed the Act myself in that respect.

10175. They have to be kept in a licensed place, I understand?—Supposing that a horse is being used for the preparation of serum?

10176. That does not come under it?—It might be done experimentally. Are you not to put it out to grass? It is nonsense.

10177. (*Sir Mackenzie Chalmers.*) There is a special form of certificate for that; the Board of Agriculture get special certificates.

10178. (*Dr. Gaskell.*) But the ordinary case is that the animals have to be kept in the laboratory, in a town very often, and it has been strongly advised by others that Certificate B should be altered in that respect. I wanted to know your opinion on that point?—There is no reason whatever for it. The new place would be registered.

10179. You naturally agree with that?—Yes.

10180. Would you also agree with this, which has been suggested to us: that in case of inoculations those inoculations should not necessarily be done by the licensee, but that authorised laboratory assistants should be allowed to do them?—I do not know. I think I should like to reserve my opinion on that. I have not thought about it. I would not allow my own laboratory assistants to do any experiment for me or to make any observations.

10181. I am only speaking of mere inoculations?—I would reserve my opinion.

10182. Many laboratory assistants are experienced in that respect?—Yes, of course. What you really mean is that laboratory assistants should be licensed?

10183. No, that was not the suggestion made to us. The suggestion made to us was that the licence to the professor should carry with it the power of the laboratory assistant to do a simple thing like an inoculation.

10184. (*Sir William Church.*) There are pathological researches at the present time going on in which perhaps 100 inoculations take place as quickly as possible?—Yes.

10185. At the present time it is a great infliction that the head, who is perhaps the only man who holds a licence in connection with that research, should have to spend all his time in making these 100 or 200 inoculations—pricks?—Yes, if that is what you mean I should certainly agree that that should be done, because a man who does that is acting simply as a machine.

10186. It has to be done by a man who holds the licence?—It has to be done under his sanction. I did not understand the question.

10187. (*Dr. Gaskell.*) I am sorry. That is all I meant. Would you be inclined to confine the Act to warm-blooded animals—a modification in that direction?—Yes, I should. I think it is absurd to suppose that cold-blooded animals feel in the same

way that warm-blooded animals do. I do not see why it should not be so confined.

10188. I believe the Act originally was framed with those words?—Yes, it was so framed. Nobody who has observed fishes, for example, can possibly imagine for one moment that they feel pain in the same sense that warm-blooded animals do.

10189. Then there is one other question that I want just to have your opinion upon. Do you use morphia often in your laboratory?—No, I do not use morphia. I used to use it more often, but I use it less often now, because I use atropin, and they are somewhat antagonistic.

10190. Would you say that morphia only dulls pain and does not remove it?—Certainly not. A sufficient dose of morphia absolutely removes all signs of pain.

10191. (*Sir Mackenzie Chalmers.*) You yourself are a medical man, are you not?—I have a medical qualification. I am on the Medical Register.

10192. What I want to ask you is this: Do you think that licences and certificates under the Act ought to be granted to physiologists who have not any medical qualification?—Certainly. If you are not to grant them to physiologists who have not a medical qualification you would eliminate some of our most eminent physiologists.

10193. The difficulty is if a man has not a medical qualification what guarantee have you of his qualifications?—You have the guarantee of those who sign the licence and certificates, and of the director of the laboratory where he is to work. There is no instance of private work in private laboratories. Some of the very men I have had to do with have had no medical qualification. One of the most eminent workers in my own laboratory at the present moment is not a medical man, he has gone through no medical mill, and has no medical qualification—Dr. F. A. H. Marshall; he is a doctor of science, but he is one of the best experimenters I know. There is further the very eminent instance of the Professor of Physiology of Cambridge, and Dr. W. B. Harvey, also of Cambridge.

10194. None of whom have medical qualifications?—None of whom have any medical qualification at all. Physiology is only accidentally and incidentally a medical science; it is the science of life, and as medicine depends upon life, physiology has come to be included with medicine; but it must be treated as an independent science, and to exclude anyone on the ground that he has no medical qualification is to my mind ridiculous, for if a man has special knowledge of a subject and a special qualification for working at it, what more is required? And when one can adduce instances of men who have advanced the science so materially as those whom I have instanced, the objection is a *reductio ad absurdum*.

10194A. The next point I want to put to you is this. You mentioned certain experiments of your own on dogs, but I did not quite catch the number of dogs that you experimented upon. You performed two experiments without anæsthetics. How many did you perform with anæsthetics?—36 altogether—two without anæsthetics and 34 with anæsthetics. A certain number of those were not actually experimented on because they died whilst being put under the anæsthetic.

10195. And as the results of the experiments on those animals, in your opinion and that of other competent people, you have discovered a more fruitful method of resuscitating the drowned?—Yes.

10196. Has that method been in any way communicated to the Royal Humane Society?—It was only communicated by my having sent a copy of the paper to the secretary—in no other way. It is adopted by individual instructors in various parts of the country. I think it is unfortunate that the Royal Humane Society, the Life-saving Society, and the National Life Boat Institution have none of them taken it up, although it is well known to physiologists, and is becoming widely known to medical men. But societies like those are apt to fossilise; it is very difficult to get anything new instituted in connection with them. They have all their instructors trained in the old methods, they have all their leaflets printed, they do not know that one method is any better than another, and they have no means of judging.

10197. As a matter of fact, your method is easier to teach?—It is very much easier to teach.

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10197A. And much easier to practise?—And much easier to practise. It can be done by one man or one woman, or even a boy or a girl; it requires no physical strength. I believe many lives would be saved if it were generally known, because it can be applied so promptly and easily.

10198. (Chairman.) By anybody?—Yes; too much time is lost by the present methods, with all their preliminary proceedings—laying the patient down, turning him over, pulling out the tongue, undoing garments, and so on, when every second is of importance.

10199. (Sir Mackenzie Chalmers.) I may take it generally that by the sacrifice of 36 dogs we have a much more prompt means of restoring the drowned, which may in time save thousands of lives?—I believe that is so.

10200. As regards dogs themselves, I believe in Edinburgh superfluous dogs are destroyed by drowning?—They used to be destroyed by drowning; they are now destroyed by suffocation, I believe. I think they are put into a chamber with coal gas.

10201. So that at any rate the two dogs which were not under anæsthesia simply suffered the common death of superfluous dogs?—Yes.

10202. I noticed that in your statement you said that as regards new drugs, drugs must be first tested on animals?—Yes.

10203. We have had evidence to the contrary, and I want you to develop that a little one way or the other. We have been told that you can draw no inference whatever from the operation of a drug on an animal as to its operation on man. What is your observation with regard to that?—If you take a drug that you know nothing about, and administer it to a man, first, there is a certain element of danger, and, secondly, you can only judge of its action upon man by external signs, whereas upon animals you can judge very accurately its action on different organs of the body. You cannot do that on man. Take a drug which is supposed to act on the liver; we know that many drugs are supposed to act on the liver, and are called cholagogues; when tested upon animals, that many are not cholagogues, that they have no action on the liver; and yet for scores of years these drugs may have been administered in diseases of the liver with the idea of stimulating the liver to action, which idea, when subjected to the test of experiment, immediately vanishes.

10204. In the case of an experiment upon an animal, you verify the experiment by a post-mortem examination?—Whenever it is necessary.

10205. When applied to man, you would hardly like to test the experiment in that way?—No.

10206. We were told by a gentleman who represented homeopathy the other day that among homeopaths all experiments were made upon man, and not upon living animals. Do you think there is any danger in that?—I do not think there is any danger in the doses that homeopaths give.

10207. But in gradually increasing doses, I am afraid you would not like to do it on human life?—I think it might be all right, but one can never know with any new drug what dose might be poisonous.

10208. (Chairman.) The witness said that in testing a drug they did not use homeopathic doses; they used the drug in its ordinary form, and tried it as high as they could?—I am afraid I am no authority on the subject of homeopathy, but I would put an extreme case: Suppose a new drug having such a powerful action as cobra venom, and suppose you tested it in ever so small a dose upon a man, it would probably kill him.

10209. (Sir Mackenzie Chalmers.) We have heard a good deal for and against experiments made before students. In your opinion it is essential, to complete the education of a man who is going to practise as a doctor, that he should see certain experiments on animals?—That is my very strong opinion.

10210. I wanted to ask you this, further. It has been alleged that witnessing experiments on animals has a demoralising influence on students, and tends to make them inhuman. From your 37 years' experience as a teacher, what do you say to that?—I say that it is absolutely without foundation. The men I know best are the men who have been my assistants, who have been engaged as students and as teachers in operating upon animals. I should

say that amongst all my friends there are none more humane than these who are constantly engaged in experimenting upon animals. So far as the effect upon students is concerned, it is obvious that what you have mentioned cannot be the case. The great mass of medical men who are turned out nowadays have seen experiments on animals—they certainly have at Edinburgh, for my predecessor showed many more experiments than I do, and hundreds and thousands of students passed through his hands, and I have not heard that the Edinburgh medical graduate is more callous than any other medical man who has not seen experiments on animals.

10211. To Dublin medical men experiments were denied for some years?—Yes, they were not shown in Dublin for many years; I do not say that those gentlemen are not more tender-hearted than others, but I have no evidence of it.

10212. It has been suggested that by seeing these experiments certain individuals, at any rate, get a desire to perform cruel experiments. Have you ever known an instance of that?—Never.

10213. It has further been suggested to us that in these demonstration experiments, as well as others, very light anæsthesia is practised. Is that according to your experience?—That is certainly contrary to the fact. I have very considerable experience of demonstration experiments, and I know as a matter of fact the great danger, in our experience, is the loss of the animal from over-anæsthesia. We are very careful that there shall not be any sign of movements, because that might be misinterpreted by the students. Although we ourselves know that movements do occur in cases of anæsthesia—every surgeon knows that—it is possible that a young student might not understand it. He knows it perfectly well after he has been in the operating theatre.

10214. I do not know if you have read Miss Lindaf-Hageby's evidence?—I read the second part of it while I was waiting upstairs.

10215. She apparently witnessed a good many demonstration experiments, and she told us that continually there was movement in the animals?—I do not know. I think that one experiment that she witnessed was on a choreic dog. There was, of course, movement in that.

10216. Would an anæsthetic stop the chorea?—I do not know; I should think not that form of chorea. Of course, movement invalidates experiment. You cannot work properly with movements; you are liable to have your apparatus injured.

10217. At any rate, in your laboratory during your 37 years' teaching, did the animals make purposive movements and struggle?—Certainly not; never.

10218. You may, I suppose, get movements when you are killing an animal?—If you kill the animal by pithing, you would have movement of muscles, which would not be voluntary movements, but simply due to excitation of the motor nerves that you were cutting through. But the usual plan is to kill the animals with chloroform; in fact, that is the invariable plan with us.

10219. You push it rather further?—Yes, we either do that or inject chloroform into the blood vessels, which kills them immediately.

10220. Miss Hageby spoke of hearing cries from some of the animals seen under demonstrations. Is that possible?—It has never happened when I have been present. I should think it is quite impossible.

10221. In your 37 years it has not happened?—No, never.

10222. Do you agree with the evidence that we have had, that when an anæsthetic is administered sensation disappears before power of movement disappears?—That is quite well known to be the case.

10223. Do you further agree that power of movement returns before sensation returns?—That is also well known.

10224. Is that sensation generally, or is it sensation to certain stimuli?—I should say sensation generally. There are higher and lower forms of sensation. The higher forms of sensation are, of course, the last to return.

10225. (Dr. Gaskell.) When you say that you have never heard any cries in your experience, have you

never heard a sound coincident with respiration of a sort of whining character; I mean to say, through the respiration tube, principally through a little blocking in the tube?—Yes, that may occasionally occur if there is mucus in the tube.

10226. (*Sir Mackenzie Chalmers.*) A sort of r  le?—Yes. I suppose by this time one hardly considers any member of this Commission as belonging to the laity—they must know by this time much about the subject; but I may be permitted to explain that it is very well known that patients when they are taking chloroform, although they are perfectly without sensation, make all sorts of noises, sing, scream, and make ridiculous utterances without being the least conscious of it, and without having any memory of having done so when they wake up. And in the same way one of the symptoms which is observed when a dog is put into a box or given chloroform is, just before he has become profoundly an  sthetised, a succession of loud noises, not exactly a bark, not exactly a whine, and not exactly a howl, but peculiar noises which might be taken by anybody who was not an expert on the subject to indicate that the animal is under the influence of pain. It is nothing of the sort at all. It belongs to the same class of noise that patients make who sing and cry out when they are going under chloroform, just before they are completely under its influence.

10227. (*Chairman.*) Does that differ with different dogs as it does with different human beings?—Yes, very much—some are perfectly quiet, and others make a great deal of noise. I do not like to hear it myself, and I am afraid that the students who are not always in the room at the time might sometimes think that we are ill-treating the dog, whereas, as a matter of fact, the animal is simply in a box with vapour of chloroform being pumped into it.

10228. (*Sir Mackenzie Chalmers.*) Do you use pure chloroform or A.C.E.?—I do not use A.C.E. I use chloroform with a small proportion of alcohol, one-tenth of alcohol; I find this quite as efficient as A.C.E., and simpler. With A.C.E. you introduce three drugs into the animal. When you use an an  sthetic you must introduce a drug, and it is better to reduce the number.

10229. Have you ever experienced any difficulty in giving an an  sthetic to dogs and keeping the dogs under an  sthesia?—In my experience dogs are more easy to an  sthetise than any other animal. One is much more liable to lose a rabbit than a dog with chloroform, especially if you give the dog a prior dose of atropin. It is very rarely indeed that I have lost a dog after introducing atropin. That would be due to sheer carelessness in the an  sthetic being pushed to an extraordinary extent. We cannot in a laboratory always have the services of a skilled an  sthetist, and this makes a great deal of difference; we are sometimes obliged to ask a person who is not a skilled an  sthetist to give the chloroform. And, as everybody knows, anyone who is not a skilled an  sthetist is much more likely to kill a patient than a skilled an  sthetist, because he will push the an  sthetic too far if he sees the least movement, and thinks the patient is coming out.

10230. In the case of an animal if you push the an  sthetic the worst that can happen is that the animal dies prematurely?—Yes.

10231. So that you can push the an  sthetic better with an animal than with a human being where you wish the man to recover?—That is so. There is the element of anxiety in giving an an  sthetic to a patient. That is not the case with an animal, because it does not matter seriously if it dies; consequently you push it more than you do with a patient.

10232. Then there is one other small point. We had some evidence last time, I think, as to various ways of pithing frogs, and it was suggested to us that the spinal cord might be severed from the brain and purposive movements would still go on; and, although the spinal cord is severed from the brain, the frog would feel pain. What is your opinion as to that?—I think all the evidence that we have is contrary to that. I think the evidence from man, if a man is to be compared with a frog, is conclusive.

10233. When once you have severed the spinal cord from the brain of man?—There is no sensation below the point of severance. The fact that movements are produced is well understood. You might just as well

say that the tail of a newt, which continues to writhe and perform spontaneous movements, has all consciousness and volition left in it. No one for a moment would hold such a thing.

10234. (*Dr. Gaskell.*) That is just what they do say?—I mean no expert authority. 12 June 1907.

10235. (*Sir Mackenzie Chalmers.*) Or a person who has studied the subject and is capable of forming an intelligent opinion?—No.

10236. As regards the use of atropin, has that been applied as yet to human beings?—It has.

10237. In conjunction with an an  sthetic?—Yes; Dr. Scharlieb, who did the experiment with me, and is an expert an  sthetist in London, tells me that he would use it always, but he cannot always get surgeons to agree that it shall be used.

10238. Do you think, as the result of your experiments on animals, that a possible improvement has been made in administering an  sthetics to human beings which may have the result of saving life?—Yes; but I would not take the credit of this to myself. It was first shown by the experiments of Dr. Embley and Dr. C. J. Martin, of Melbourne, who is now at the Lister Institute, that the fatal effect of chloroform is usually produced on the vagus, and that cutting the vagus (and giving atropin has exactly the same effect as cutting the vagus, because it cuts off the influence of the vagus from the heart) entirely abolishes this inhibitory effect of chloroform. So that although I have carried the giving of atropin into practice and I have recommended that it should be given in all cases of an  sthesia, I do not wish to take credit for the discovery of the fact.

10239. Have you discovered any dangerous results from giving a dose of atropin when given with an an  sthetic?—No, none whatever; the dose required is very small. In a large dog, for example, 1-50th of a grain of atropin is quite enough to keep the animal in a safe condition for three hours. In the case of man, I do not think a larger dose is required, because man is more susceptible to atropin than the dog.

10240. Of course the atropin itself has no effect on the sensation of pain or on the an  sthesia which is produced?—No.

10241. Then there is one other question I want to ask you as regards the Home Office Administration. You agree that under the Act the Home Secretary can suspend or disallow a certificate?—Yes.

10242. Does it not follow from that that he may impose conditions?—I do not know that it does follow from that, but he can impose conditions on the licence.

10243. I am speaking now of a certificate?—There is nothing in the Act to say that he shall alter the certificate. The certificate is granted by two authorities, and there is nothing in the Act to say that he shall alter the certificate.

10244. But an alteration, I suppose, is alternative, and possibly preferable to disallowing it?—When he puts himself in the position of the grantors, I do not think that he is right. He makes himself an expert authority on the subject.

10245. At any rate, he is the person responsible to the public for the operations that go on?—I think under the Act it was intended that the grantors should be responsible to the public. Otherwise I do not see the use of them.

10246. The Secretary of State has to answer in Parliament, which the grantors have not to do?—Yes, of course.

10247. Then as regards the place of the experiment, I did not quite understand your suggestion. You thought it was unnecessary to keep the animals in the place?—I do not think it is of any importance that they should be kept in the same laboratory.

10248. Is it not simply for the purpose of inspection; that if the animals could be moved about the inspector would not be able to inspect them?—I think the inspector would be able to inspect them if they were removed to another registered place.

10249. You would confine your amendment of the law to allowing them to be moved from one registered place to another registered place?—As for example, from a laboratory in town to a farm in the country, which might also be registered for the keeping of animals.

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10250. (*Chairman.*) Is there anything else you wish to bring before us?—Something has been said about the importance of clinical observation, and as to the possibility of clinical observation superseding experiments altogether. I should like to illustrate the defects of mere clinical observation unassisted by experimentation on animals by a statement regarding the discovery of the active principle of supra-renal extract. Dr. George Oliver had been making a large number of clinical observations upon the effect of various organ extracts upon the circulation, but had been unable to arrive at any definite conclusions regarding them. Amongst these was extract of supra-renal capsule, extract of thyroid gland, extract of brain, and so on. He consulted me as to what steps might be taken to arrive at a clearer understanding with regard to their action, and I invited him to investigate their physiological action along with me upon animals in the laboratory. This we proceeded to do; and the result of the investigation was that the majority of the extracts from which he supposed that he had obtained definite results in man gave no indications of physiological activity; whereas, on the other hand, the extract of supra-renal capsule gave such manifest indications of activity that it was quite clear a very important principle was contained within this organ. The properties of this principle we then proceeded to work out, and the result of the investigation led to the discovery of adrenalin, which has proved of immense physiological, and will in all probability prove of great therapeutical and clinical importance. I would also point out that two methods must always be employed in physiology in investigating the functions of an organ. The first is the method of stimulation or addition; the second, the method of removal or subtraction. This may be illustrated by experiments upon such organs as the ductless glands. If we again take the case of the supra-renal capsule, we find that the effects of addition are, first, increased contraction of the blood vessels; second, increased action of the heart; third, contrac-

tion of the uterus, and, generally, a tonic influence in all parts supplied by the sympathetic nerve. If, however, the addition be excessive and prolonged, certain chronic effects are produced, the most striking of which is an atheromatous condition of the arteries, which may eventually lead to aneurism. The effect of removal, on the other hand, is in most animals death within a few hours with symptoms of extreme vascular and general muscular weakness, these symptoms being similar to those which result from the gradual destruction of the gland by disease. So again with the thyroid body. The effects of addition or increase of its secretion are the production of a condition of nervous excitation, and certain ill-understood effects on nutrition. The effects of its removal vary in different species of animals. In some the result is *nil*, possibly owing to the fact that there may be small accessory glands of the same structure and function left behind in the body. In other cases, on the other hand, there is a general condition of malnutrition of the connective tissue produced, together with depression of the functions of the higher parts of the brain, sometimes combined with an excitable condition of the lower levels of the nervous centres, leading to increased reflex action and even to convulsions. All these effects can be counteracted by thyroid administration. The second method of experimentation necessitates the keeping of the animal after removal of the organ for days, weeks, or months, in order to understand the condition of the experiment; and the animal used must be one the organs of which are generally similar to those of man, if the conclusions which are drawn from the experiments are to be applicable to man. On this account, and for this second method of experimentation in particular, it is in very many instances essential that dogs should be the subject of experimentation. This is especially true for work upon the alimentary and glandular systems, whereas for work upon the central nervous system monkeys as well as dogs require to be used.

TWENTY-THIRD DAY.

Wednesday, 19th June 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. McFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

The Hon. STEPHEN COLERIDGE, called in; and Examined.

10251. (*Chairman*.) You are Secretary of the National Anti-Vivisection Society, I believe?—No; I am Honorary Secretary; Mr. Stewart is the Secretary.

10252. How long have you held that position?—I have been Honorary Secretary for ten years—since 1897.

10253. You have been good enough to send us a short note of the subjects on which you desire to give evidence. There is only one point that I wish to call your attention to—that is, your reference to the ethical question. I only want to say as regards that that we have had a great deal of evidence upon that question, to which we attach due importance, I hope, and that we have become pretty well possessed of the opinions and views which are held upon it. We are anxious to have the views of your Society upon it, too; but I hope that in consideration of the great amount of evidence we have heard, you will put that part of your case as briefly as possible?—I have prepared a very short statement, which I think I might as well read, because it will, at any rate, afford the Commissioners material for cross-examining me upon it.

10254. Let me ask you first about your Society, so as to have it on the notes. When was it founded?—About 1875 or 1876; just before the passing of the Act.

10255. And it is a very large Society?—Yes, by far the largest in the world.

10256. I dare say you can tell us how many members there are?—I have got the figures here. To illustrate its increasing importance, I give the income for each five years since 1887: £8,651 for the first five years; £15,712 for the next; £26,352 for the next; and £36,097* for the last five years, showing a large increase.

10257. Does membership involve a subscription?—Yes. We have a great number of honorary members, whom we have elected, and a very large number of our branches have very large numbers of members each; but some only subscribe a shilling, and such small subscriptions, so that it is impossible for us to ascertain the names of everybody.

10258. You have not a fixed subscription?—Yes, for headquarters—10s.

10259. And I may take it, I suppose, that while the evidence is your evidence, it will represent generally the opinions of your society?—Yes; whenever I wish to dissociate myself in any way and give a personal note to my evidence, I should say that I was not in agreement. But generally, I am in agreement with my society.

10260. I think now you had better give us your statement, and you will allow me, if I think you are getting off that line at all, to interrupt you?—Yes. I assume that it is not exactly like a court of justice. I have put down, so that there should be no question about the claim of my society really to represent the anti-vivisectionists, the force and strength of the other societies, and, without desiring in any way to set up an odious comparison, I think it is proper that I should say that the income of the London Anti-Vivisection Society, which is next in importance to the National, amounts to about a quarter, and that the income of the British Union amounts to about one-eighth of that of my society. There is the Manchester Society for the Protection of Animals from Vivisection, with an office at 9, Albert Square, Manchester, and an annual income from donations and subscriptions of £205 3s. 8d. There is the Parliamentary Association for the Abolition of Vivisection, with an office at Parliament Mansions, Victoria Street, London, and an annual income from donations and subscriptions of £174 9s. 6d. I think you have had four witnesses from that society. There is the Bristol and West of England Anti-Vivisection Society, with an office at Bank Chambers, Clifton, Bristol, with an annual income from donations and subscriptions of £164 10s. 9d. There is the Friends' Anti-Vivisection Association, with no official address, and an annual income of £118 5s. 5d. There is an Electoral Anti-Vivisection League, with an address at 92, Ladbroke Grove, W., and an income of £83 6s. 4d. There is the Society for the Abolition of Vivisection, founded by the late Mr. Jesse, who left it some money on condition that it never associated itself with any other Anti-Vivisection Society. This exclusive association has an income from donations and subscriptions of £60 9s. 6d. a year. It also enjoys the interest from Mr. Jesse's bequest. I have ascertained that there are 79 members of that society. Then there is the Scottish Society for the Suppression of Vivisection. I have asked for their last year's report, but up to date I have not received it. That is another very old Association, but the membership is not nearly so large as is the membership of the Scottish branch of my own society. There is a World's League of Opponents of Vivisection, which addressed its letters from 32, Sackville Street, which, according to the published report, has 63 members, with a total yearly subscriptions and donations of £56 0s. 1d. There is a Church Anti-Vivisection League, with no official address in its yearly report and an income of £54 4s.† There is the Bolton Anti-Vivisection Society, with no official address, and an annual income from donations and

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* Mr. Coleridge subsequently wrote to say that this sum included the "Coleridge Defence Fund."

† The Secretary of the Church Anti-Vivisection League subsequently wrote saying that the League had an official address and an income of £60 15s. 2d.

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subscriptions of £39 1s. There is a Society for United Prayer for the Prevention of Cruelty to Animals, especially with regard to the practice of vivisection; it appears to have no office; its income is £33 16s. 11d.* There is the Pioneer Anti-Vivisection Society with no official address, and an annual income of £6 17s. 6d. This exhausts the Anti-Vivisection Associations as far as I am aware, and I think leaves no doubt of the justice of the claim of my Society to represent the cause of anti-vivisection in this country. I cannot give you the exact membership of our own Society. The Birmingham Branch is very large, but they subscribe, some of them, only a penny or twopence, or sixpence. The first striking achievement of my Society, with which I had the honour to be associated, was the prevention of a public meeting held at the Mansion House in 1889, the object of which was to start a fund to erect a Pasteur Institute in England. We prevented that public meeting taking place, and the result was that no Pasteur Institute was set up. England to-day is, I believe, one of the few European countries of importance that is spared a Pasteur Institute, and in England there is no rabies. Another remarkable achievement of my Society has been the long agitation against the diversion of Hospital Funds in London to Medical Schools, which are places registered for vivisection under the Act of 1876. We conducted that agitation with such persistence that King Edward's Hospital Fund were at last obliged to appoint a Committee, composed of Sir Edward Fry, the Bishop of Stepney, and Lord Welby, to inquire into the charges we had publicly formulated. That Committee, after hearing evidence from all parties, made a report which confirmed the statements we had published. It was stated that where money was paid over to schools out of hospital funds, the schools remained "debtors to the hospitals in respect of these pecuniary contributions made to them," and recommended that in future the funds of hospitals and schools should be kept entirely distinct.

10261. That is the particular statement that they confirm?—Yes, that is so. In spite of this remarkable report, however, no school has repaid a penny of the money diverted to it from the hospital; and in some cases the deliberate diversions of money subscribed for the sick poor to schools registered for vivisection continues, with the knowledge of and without any effective protest from King Edward's Hospital Fund, which thereby contemptuously disregards the recommendations of the distinguished Committee appointed by itself. I present to this Commission a publication, for which I take the entire responsibility, briefly called my "Guide to the Charitable," which sets out the financial diversions of hospitals to schools up to the present date (that is for purposes of cross-examination). In the last few years we have conducted public controversies in the "Fortnightly," "National," and "Contemporary" Reviews, and many other periodicals; we have held mass meetings in the largest halls in London; we have founded thriving and earnest branches all over the three Kingdoms. We have conducted a Parliamentary campaign in favour of the Bill I have drafted, and have kept the question of vivisection so prominently before the public that it has been found necessary to appoint this Royal Commission to inquire into it. We claim to represent the consensus of opinion of almost all the greatest names that have adorned the history of England in the field of thought. In *Cymbeline* the Queen suggests to Cornelius, her doctor, a process of investigation similar to the modern inoculator's experiments under Certificate A. He says:—

"I will try the forces
Of these thy compounds on such creatures as
We count not worth the hanging (but none human),
To try the vigour of them, and apply
Allayments to their act; and by them gather
Their several virtues and effects."

Upon which Cornelius, in a spirit far different from the modern vivisector, replies:—

"Your Highness
Shall from this practice but make hard your heart:
Besides, the seeing these effects will be
Both noisome and infectious."

From which passage we may claim the greatest name in England and the world for our cause. And it may be interesting to the Commissioners to know that my attention was first called to this striking passage by my late friend, Sir Henry Irving, himself an ardent

anti-vivisectionist. Andrew Marvel, in a striking passage, says:—

"Heaven's King
Keeps register of everything,
And nothing may we use in vain;
E'en beasts must be in justice slain."

Dr. Johnson dismissed the vivisector with one crushing sentence:—"What is alleged in defence of these hateful practices everyone knows, but the truth is that by knives, fire, and poisons, knowledge is not always sought, and is very seldom attained. I know not that by living dissections any discovery has been made by which a single malady is more easily cured. And if the knowledge of physiology has been somewhat increased, he surely buys knowledge dear who learns the use of the lacteals at the expense of his own humanity. It is time that a universal resentment should arise against those horrid operations, which tend to harden the heart and make the physician more dreadful than the gout or the stone."

10262. Where is that from?—I can find it you; it is a fine passage I think. And the cause of anti-vivisection counts, and has counted, among its supporters, living and dead, Cardinal Manning, Lord Tennyson, Robert Browning, John Ruskin, Thomas Carlyle, James Anthony Froude, Freeman, the historian, the great Lord Shaftesbury, who was the first president of the society, and filled that office till he died, George Meredith, member of the Order of Merit, General Booth, the Lord Chancellor, Mr. John Morley, member of the Order of Merit, Cardinal Gibbons, James Martineau, Spurgeon, Lord Brampton, Wagner, Leslie Stephen, Sir Edwin Arnold, Mark Twain, Tolstoy, Victor Hugo, Dean Stanley, and a host of distinguished men and women in every walk of life whose opinions upon a matter of conduct cannot be disregarded, and Her late Majesty Queen Victoria, who was in favour of the total abolition of vivisection. As the humble spokesman of this constellation of great writers, I feel the extreme inadequacy of my powers fitly to give voice to their solemn condemnation of painful experiments on animals as a practice repugnant to the promptings of humanity and degrading to mankind, but I desire to be permitted to say that, inasmuch as four generations of my family before me have earnestly supported this humane cause, my own convictions have come down to me as a consecrated inheritance. My society desires to see vivisection totally abolished by law, but it will strive for and accept any measures that have for their object the amelioration of the condition of vivisected animals. We are guided in our policy inside Parliament and without by the consideration that we are the faithful representatives of the hapless creatures in the laboratories who cannot speak for themselves. We base our objections to vivisection as sanctioned and protected by the Act of 1876 on the plainest moral ground. We hold and invite the Commissioners to maintain that the right of man to take animal life involves the duty of taking it with as little accompanying pain as possible. We are prepared to show that under the existing law there is no limit to the agony that may legally be inflicted upon animals, and we abominate a statute that authorises gross inhumanity. We say that to keep an animal alive and torture it is a wicked act, that no profession of lofty motives will ever make that wicked act right; if this were not so, it might be right to rob a bank with the professed intention of devoting the stolen money to the services of the poor. The whole question of man's rights over and duties towards the helpless animals given into his dominion by the Ruler of the Universe, is a moral question, and has nothing to do with science. And if all the benefits so boldly claimed as having been conferred upon physiology by this practice were in deed and truth so received, the issue would remain solely a matter of conduct upon which the least scientific of us has as much a right to be heard as the most expert vivisector; and we maintain that if there be any cruelty inseparable from this particular science every thoughtful man who takes a wide view of human well-being must conclude that it is far better for us all to do without physiology than to be without pity. I have carefully read the evidence of Mr. Byrne on behalf of the Home Office, and as it stands it has, in my opinion, justified to the fullest extent the expressed and published conviction of my society, that the whole truth about the administration of the Act of 1876 by the Home Office could never be elucidated without cross-examination by counsel properly instructed.

* The Secretary of the Society for United Prayer for the Prevention of Cruelty to Animals subsequently wrote to say that the Society had an official address and an income of £89 0s. 8½d.

We should have preferred to substantiate the charges which it is our duty to make against those officials out of the mouth of Mr. Byrne, their representative; but as that is now apparently impossible, I shall endeavour, on behalf of my society, to prove the indictment we have drawn by unimpeachable evidence. (1) I am here to charge the Home Office officials with repudiating the most important duty deputed to them by Parliament, *videlicet*, that of protecting animals from unjustifiable suffering; (2) I am here to charge the Home Office officials with having constituted themselves the injudicial defenders of the vivisectors from criticism by my society in the past, and in their evidence tendered before this Commission; (3) I am here to charge the Home Office officials with having appointed inspectors who have displayed such bias that they have thought it their duty not to make detective efforts to protect animals from illegal treatment; (4) I am here to charge the Home Office officials with having made entirely disingenuous statements in their official utterances, and with having constituted themselves the mere spokesmen of the vivisectors; (5) I am here to charge the Home Office officials with accepting the suggestion made to them by some nameless adviser that to starve animals for days is not cruel when they must know perfectly well that if any one of them starved their own horses (if they have any), the nearest policeman could successfully prosecute them for cruelty to animals under Martin's Act; (6) I am here to charge the Home Office officials with suppressing in the Annual Parliamentary Return the names of those who take upon themselves the very grave responsibility of signing the certificates exempting licensees wholly or in part from the obligation to employ anaesthesia in their vivisections, although in the Report of the former Royal Commission we find this clear direction: "We recommend that his (the Home Secretary's) advisers be from time to time elected and nominated by himself. Their names should be made known to the profession and the public." (7) I am here to charge the Home Office officials with shielding the names of such licensees as they know to have broken the law, although the former Report contains these words:—"Abuse of the power conferred by the licence must, of course, render the holder liable to its withdrawal, but this will involve great disgrace," a phrase that clearly indicates that the framers of that Report contemplated the publication of offenders' names, for no great disgrace can appertain to a secret remonstrance of a secret withdrawal of a licence. (8.) I am here to charge the Home Office officials with preparing for the Home Secretary evasive and insufficient replies in the House of Commons to plain questions on the administration of the Act, and with making evasive and insufficient replies themselves in official correspondence with my Society, and with leaving perfectly proper questions unanswered altogether. (9.) I am here to charge the Home Office officials with putting forward annually a Parliamentary Return in which it is asserted, on the official authority of the Government Department, that not a single experiment in thousands inflicted upon animals in Great Britain with its permission can be specified as entailing any pain at all, when all the while this plausible assertion is based upon no better evidence than the bare assertion of the vivisectors themselves, who are not to be expected to report themselves cruel men. (10.) I am here to charge the Home Office officials with placing a certain vivisector year after year beyond the reach of the safeguards erected by the Act to protect animals from illegal treatment by licensees, by giving him permission to vivisection in private places, thereby placing him beyond the possibility of legal inspection; because by Clause 10 of the Act of 1876 the inspector has no right of entry into, or inspection of, any unregistered place.

: 10263. What is the name of that particular person? —Victor Horsley. (11.) I am here to charge the inspectors certainly, and the Home Office officials apparently, with having made no inquiries, public or private, into the vital question of the character for humanity of the licensees to whom they have delivered over the animals to be vivisected. Although those officials must be aware that the Report of the Royal Commission in 1876 contained this pregnant warning: "It is not to be doubted that inhumanity may be found in persons of very high position as physiologists," and although the Act of 1876 placed upon the Home Secretary the tremendous responsibility of personally protecting animals from that undoubted inhumanity. (12.) I am here to charge the Home Office officials with

having placed themselves in improper private confidential relations with a private society composed of supporters of vivisection entitled to no more consideration than the National Society composed of opponents of vivisection.

10264. (*Sir Mackenzie Chalmers.*) Would you mention that society's name?—It has been called in the House of Commons the Society for the Propagation of Scientific Research; its real title is the Society for the Advancement of Medicine by Research. These manifold charges, which I am prepared fully to substantiate before leaving this Commission, justify, in the opinion of my Society, the imputation, which we do not fear to make, that the Act which was intended by Parliament to constitute the Home Secretary the guardian of the vivisected creatures, has been so administered that this solemn responsibility has not been impartially fulfilled. So safeguarded are the vivisectors from the possibility of anybody but themselves knowing what happens in the laboratories, that the opportunity of raising the impenetrable veil that covers their doings can never occur but through their own initiative. Through the hasty tamerity of Mr. Bayliss, who brought an action against me, we did for once ascertain from the evidence of that vivisector and his friends the history of the life and death of one at least of the victims of the laboratory. Their evidence proved that under the present law a deep incision can be made into the body of a live dog, and it can be deprived, by the tying up of a duct in its inside, of the proper use of one of its internal organs; it can then be sewn up again and put in a cage, and left in that cage from December to February to see what the result would be of that operation upon it. In February it can be taken out of the cage and a fresh incision can be made into its body to see whether what has been done to it produces inflammation or not. The wound can then be closed up again with a pair of steel forceps. Then with the steel forceps closing up this wound, the living dog can be handed over to a second vivisector, who proceeds to fasten it down tight on to a board and to open its neck with another deep incision, exposing the gland; he can then fix little pipes on to the end of the arteries, he can put a tube into its windpipe, and attach electrodes to its cut-out nerves. The dog in that condition can be tied down on that board for about an hour, and then handed over by this second vivisector to a third operator, who finally puts an end to the miserable dog's life by plunging a knife into its heart. Now the anaesthetics during these prolonged and fearful mutilations can legally be applied by an automatic pump in another room, connected to the dog under the floor by a tube, and this pump, on which alone the insensibility of the dog depends, can be left in the sole charge of a laboratory boy. The vivisectors say that this automatic pump is sufficient to maintain the unconsciousness of the dog throughout the whole operation; but, inasmuch as the dog cannot tell us whether it is unconscious or not, and as no analogy can be set up with human anaesthesia, because no surgeon in his senses would attempt to anaesthetise a man or a woman with an automatic pump in another room under the management of a laboratory boy. I maintain that we may reasonably refuse to accept the opinion of these vivisectors that such means are efficient to maintain total unconsciousness. Further, we have a perfect right to hold the opinion—and we do hold it—that the sufferings of the dog between these vivisections when it was in that cage were very grievous. To procure this priceless information from the vivisectors themselves in the witness-box cost altogether about £5,000; but in my opinion this revelation of what vivisection really is in this country under the present administration of the law was well worth the expenditure. As to the assertions of the vivisectors who have come before you that they individually have never inflicted pain in their experiments, I am not here to ask you to believe them or to disbelieve them; but I assert that the present law, as administered, cannot possibly protect animals from the extremest torture at the hands of any licensed vivisector who happens not to be humane. The comfortable hypothesis of the Home Office appears to be that a person who applies for a licence and receives certificates is necessarily humane. To that hypothesis we distinctly demur. Neither the dignitaries of the physiological circles nor the vivisectors who are banded together in the private society for the advancement of their practices, nor the inspectors appointed by the Home Secretary, testify to the personal character or humanity of the applicant for licences

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and certificates. They aver nothing but the applicant's scientific fitness. Scientific fitness is no guarantee of humanity, nor is the habit of cutting into living animals necessarily the concomitant of a tender heart. Therefore we assert that the present law and the present administration of it do not protect animals from the worst conceivable fate. We represent a growing volume of indignation against such a law and such an administration of it. There have been members of my society so moved by the hideous possibilities of the laboratories as to be hardly restrained from violence. The sufferings of dumb creatures appeal with overwhelming force to an ever-increasing proportion of our countrymen and countrywomen, and, as I believe, to those who are the truest judges of right conduct. I am reluctant to make a personal attack upon any vivisector; I have come here on behalf of my society to denounce a system and a practice rather than to expose individual barbarities. But I may perhaps be permitted to say that the permission recently given to a vivisector to drown, resuscitate, and drown again dogs without any anaesthetics, which have been already alluded to here, have been regarded by my society with particular detestation.

10265. (*Colonel Lockwood.*) That is Schäfer you mean?—Yes, Schäfer?—I know not how many thousand upon thousands of men, women, and children have been saved from drowning the world over by dogs since they became attached to mankind as their faithful friend. Not long ago I read of a dog that had saved ten lives from drowning, and perished in a gallant but unsuccessful attempt to save an eleventh. It seems to many of us an act that is degrading to humanity to reward all this splendid devotion by taking dogs and subjecting them to these horrid experiments, and it seems to me inexplicable that any man could be found to do such dreadful business. The physiologists tell us that they can learn more from the dog than from other animals. To that proposition we oppose no contradiction. From the dog we can learn courage, constancy, loyalty, steadfastness, faith, love; but it is not these qualities that interest the vivisector; it is the weight of its spleen or the pressure of its blood that elicits his curiosity, and he digs into its living body in his horrid quest. I desire to bring before the Commissioners the sixteen experiments performed by Dr. Crile in Sir Victor Horsley's laboratory, in one of which the foot of a dog was deliberately crushed "under incomplete anaesthesia." Apart altogether from the question of pain, this series of experiments, involving the most repulsive operations, in which every conceivable outrage is perpetrated upon bodies of the victims, have filled decent people who have faced their perusal with disgust and horror. I am here to say on behalf of my society that if physiology cannot be pursued without such loathsome practices it is high time it were abandoned. We are confident that we are expressing the feelings and sentiments of the vast majority of Englishmen when we assert that the mere power so to treat living creatures does not involve the justification of such treatment as a matter of conduct. The knowledge of what will happen to a dog when its feet are crushed in pincers and boiling water is poured into its inside seems to me, as a layman, as remote from any practical service to humanity as is the discovery of a variable star or the five hundred and sixty-eighth asteroid. But, looking through a telescope and tearing a living dog to pieces are very different human actions, and this fundamental difference must ever place physiology as now pursued in a degraded position altogether distinct from that of the noble sciences. Physical pain is perhaps not the worst evil that can afflict mankind. To us, "with such large discourse looking before and after," bodily pangs are less insufferable than remorse, ruin, jealousy, or a broken heart. But with animals this is not so; to them physical pain is the worst of all evils. They have no armour of the mind wherewith to summon to their aid an intellectual fortitude. They can look up to no martyr's crown, they cannot bring their miseries to the feet of a pitiful God. Therefore it is that our sympathies are so deeply moved by the spectacle of animal suffering, therefore it is that we say that to pursue knowledge through the agony of animals is an act only possible to a man whose heart is dead. If it could be proved that anybody's life had been prolonged by these practices our answer is that if the vivisectors could make us all live to be a hundred it would be a miserable exchange for an extinction of pity in the human heart. It is comparatively unimportant how long we live; what matters is, how we live. There is one more

pathetic concomitant of vivisection which is totally ignored by the physiologists, but which I desire to enforce upon the attention of the Commissioners. Grievous and terrible as are we believe the physical sufferings now permitted by law to be inflicted upon dumb animals, this is not all for which the physiologists and the Act of 1876 are responsible. The knowledge that horrible mutilations may be daily and hourly executed upon the bodies of living creatures with no adequate security, for their insensibility makes very many humane people profoundly miserable, it rises day and night between them and their peace of minds, it haunts their lives waking and asleep, it deprives them of joy in this world which otherwise might be theirs. I am here to ask that this mental anguish, so immense in its aggregation, should not be regarded as of no account by this Commission. In all my reading of the writings of the vivisectors descriptive of the most appalling experiments, I have never met with a single expression of regret that such treatment of helpless creatures was a painful necessity, or a single word of sympathy with the miserable victims in their laboratories, and I am here to tell the Commissioners that the 300 odd vivisectors in the kingdom are responsible for a rising and widespread feeling of bitterness against the whole medical profession, with which in the public eye they are confused. This in the future may be not the least price the physicians of the country will find they have to pay for their supine acquiescence in vivisection as it is now allowed by law. The Commission by recommending Parliament to afford a real protection to vivisected animals have it in their power to check the spread of a deep and abiding enmity to a whole profession, which will be as deplorable as it will be inevitable if the law remains as it is. It will be observed that my Society has been forced to the conclusion that, by placing the interests of the animals in the hands of the Home Secretary, the framers of the Act of 1876, unwittingly, no doubt, made a fatal mistake. The Home Secretary has practically delegated much of his responsibility to the Home Office officials, who have in their turn delegated their responsibility largely to a private association of vivisectors called into existence for the very purpose of representing their own interests as opposed to the interests of the animals as represented by my society. And a state of things has thereby been created and maintained for years which we invite all impartial people to condemn as little short of a public scandal. What might have happened had a Home Secretary come into office who held the opinion that to inflict any amount of suffering was justifiable if it were claimed that science might possibly be advanced thereby, must be as obvious as deplorable, unless the solemn aspiration with which the Royal Commission of 1876 closed their Report is to be regarded as having been entertained in vain. "We trust," they said, "that your Majesty's Government and Parliament of this kingdom will recognise the claim of the lower animals to be treated with humane consideration." Under the present law what has happened in the past and what legally might happen in future fill those I represent with indignation and alarm; and we earnestly invite this Commission to consider with care and to recommend to Parliament the Bill which I have drawn, and which I now present to it. And now before I proceed to advance from these general statements to the production of the evidence for their support, it is my duty on behalf of my society to enter very respectfully my protest against the exclusion of the Press from this Commission, and the exclusion of counsel to represent our society. The ultimate publication of these proceedings will not contain much that the public may rightly claim to know, and of which the presence of the Press would have kept them informed. But my society have deemed it nevertheless to be their duty to direct me to bring their case before the Commission, and, of course, what I myself may say here is not said confidentially, but is intended to be entirely and with as little delay as possible public.

10266. (*Chairman.*) What are the matters of fact that you are proposing to deal with?—I now propose to deal with these charges which I bring against the Home Office seriatim. The first charge that I bring against the Home Office is that of repudiating the most important duty deputed to them by Parliament, viz., that of protecting animals from unjustifiable suffering. The Report of the Royal Commission of 1876 recommended that "Experiments upon living animals, whether for original research or demonstra-

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tion, should be placed under the control of the Secretary of State." It further says: "The holders of licences should be bound by conditions, and breach of the conditions should entail liability to forfeiture of the licence." Now this is what I am emphasising: "The object of the conditions should be to ensure that suffering should never be inflicted in any case in which it could be avoided, and should be reduced to a minimum where it could not be altogether avoided." What I have to say about that is this: I maintain that when you are reading the old Act of 1876, it is essential to read it in conjunction with the recommendations made by the Commissioners who reported in 1876, and upon whose Report that Act was passed; and, therefore, I say that the Act of Parliament clearly laid upon the Home Secretary the duty of seeing that suffering should not be inflicted if it could possibly be avoided, and in fact placed the whole question of pain in his hands.

10267. (*Sir Mackenzie Chalmers.*) Would you give us the actual words you rely upon?—Yes—"the object of the conditions should be"—.

10268. No—the words of the Act, please; what are the words of the Act?—I am reading from the Report of 1876.

10269. The Report was afterwards followed by the Act?—That is what I have said. I have said that, in my opinion, and I hope in the opinion of the Commissioners, the Act of Parliament must be read in conjunction with the Report upon which it was founded, and that where distinct recommendations are made in the Report it is the duty of the Home Office to carry out those recommendations within the four corners of the Act.

10270. (*Chairman.*) You could not quite say that; I think no lawyer could say that that was his duty under the Statute. It may be an independent duty?—An independent duty, yes. If you look at the Statute as no doubt you have, the Statute begins by calling itself "An Act to amend the law relating to cruelty to animals," it therefore is a law entirely dealing with the question of pain. The whole Act is to place the regulations protecting animals from unjustifiable suffering in the hands of the Home Secretary and his officials. I mean, of course, that the Home Secretary personally, I presume, delegates it to his officials. Therefore I say that if the Act is read as the result of that Royal Commission, it must be taken that the duty is placed upon the Home Secretary to protect animals from unjustifiable suffering. Now, having as I consider established that, I refer you to Question 129, where Colonel Lockwood asks Mr. Byrne "What expert opinion guides the Secretary of State for the Home Department in deciding as to the painfulness of an operation"; and the answer is "The Secretary of State has not to decide whether an operation is painful or not." Therefore I say that the Home Secretary repudiates, in the words of Mr. Byrne, who, I presume, represents him, the duty which, in my opinion, was placed upon him under the Act.

10271. (*Sir Mackenzie Chalmers.*) Would you read the context there. The question was a question of statistics there?—(*After referring to the evidence.*) It is a perfectly fresh question where Colonel Lockwood begins; it has no relation whatever to the question before it.

(*Chairman.*) It appears to be so.

(*Sir Mackenzie Chalmers.*) But it was on the discussion of the new form of Returns—it arose out of that.

10272. (*Chairman.*) Will you continue, please?—Further down I find, at No. 163, that Sir William Collins asked Mr. Byrne—this is again on the question of pain. "Is that regarded as a painful procedure?" Sir William asked at Question No. 162 in regard to inoculation; and Mr. Byrne replied: "It is not a painful procedure, but it is considered a painful experiment." Then he is asked: "And is so classed?" and his answer is: "There is no classification of experiments in the last Report into painful and not painful; but it is regarded as undoubtedly painful by the Home Office." So that on one page the Home Office repudiates all responsibility about pain, and on the next page they say that they do regard a certain class of experiments as painful. Therefore it is quite clear that they do not altogether speak consistently upon this matter. Then the next question is: "Do I rightly understand that there was formerly a classification into painful and painless?" and the answer is:

"Yes; if you will be so good as to look at the end of my memorandum relating to the Annual Return under the Act, you will find that at various stages attempts, mostly unsuccessful, were made to divide the experiments returned into those painful and those painless. That has now been given up, and the last Return does not even profess to do so." So that the Home Office have absolutely abandoned all attempt, so far as I can see, to take upon themselves the duty which I maintain is theirs. Perhaps now, while I am on Certificate A, I might say a word or two about Certificate A. I notice—and this is very interesting, I have made a great study of this—that before 1888, when Mr. Busk was the Inspector, in all the Annual Reports (and I have read them all very carefully through), there is no allusion of any kind to any conditions being attached to Certificate A. Down to 1888 I know Mr. Busk was Inspector, and there was no allusion anywhere in the Reports, as I have said, to any conditions being attached to Certificate A. And therefore it is a fair deduction that down to 1888, under Certificate A, any amount of torture might have been inflicted, because it relieves the operator altogether of the necessity of using anæsthetics in his operations.

10273. I do not quite follow why you say that?—Certificate A represents Sub-proviso 2: "Experiments may be performed without anæsthetics on such certificate being given as in this Act mentioned, that insensibility cannot be produced without necessarily frustrating the object of such experiments." Each year there is a Report and letterpress, and in the letterpress of late years there have been allusions to conditions which have been always attached to Certificate A before it was issued. Before 1888 there is no evidence from the letterpress and Reports, that any such condition was ever attached to it before it was issued; and therefore, I say, that down to 1888 there seems no doubt that under the administration of the Home Office permission was given to inflict any amount of torture upon animals, because experimenters were permitted to perform experiments without anæsthetics simply; all they had to do was to say that insensibility could not be produced without frustrating the object of the experiment; so that, if the experiment was to find out the result of pain, for instance, it obviously would not have been of any use if the animal did not feel the pain. I am only bringing this forward to show that under the present law any amount of torture not only may be, but I am afraid has been inflicted.

10274. (*Sir Mackenzie Chalmers.*) Now, let us go to since 1888?—I am going on to since 1888. In 1888 Mr. Erichsen became Inspector, and in his report for 1888 we find these words: "The 'inoculation condition' introduced last year, by which it was rendered imperative on the part of the experimenter to destroy the animal under anæsthetics, provided pain had been developed as the result of the conditions to which the inoculation gave rise, has been in all cases enforced." I regard that as a very proper and humane proviso that should be added to Certificate A, and it clearly apparently was added to Certificate A in 1887; in the Report of 1888 we find it. In the Report of 1889 we have an announcement to the same effect—namely, that as soon as pain is developed under inoculation, the animal is to be destroyed. In 1890 I find a grave alteration was slipped into this Report. In this Report we find the "inoculation condition" is thus defined: "This condition renders it imperative on the operator to destroy the animal under an anæsthetic if severe pain has been induced, so soon as the result of the experiment has been obtained." That is a very different condition, you see. That lets in any amount of pain, provided the experimenter is of opinion that the result of his experiment has not been obtained; whereas, before that, there is nothing about the result of the experiment having been obtained. "If pain ensued, animal destroyed," was a further condition down to 1890. Therefore we see in 1890 this slips in, this permission of pain. In 1891 the words are: "The animal shall be killed if pain result." That appears to be a reversion again to the more humane conditions. And practically no allusion is made to the matter again under Dr. Poore's régime.

10275. (*Colonel Lockwood.*) Dr. Poore succeeded to Erichsen?—Dr. Poore succeeded to Erichsen in 1892. The last of Erichsen's Reports says: "The animal shall be killed if pain result." It seems that Erichsen reverted to his more humane condition before he retired, or died—whichever it was, I forget. Then

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comes in Dr. Poore, and Dr. Poore never says a word about it. He maintains the mystery of silence as to the conditions under which Certificate A is issued, and in 1900 he retires. Then in 1901 Mr. Thane takes office; and this is what he says in his first Report: "In the event of pain ensuing, the animal to be killed 'as soon as the main result of the experiment has been attained.'" That obviously is, in my opinion, an inhumane condition. It lets in, so far as I can see, any amount of pain under Certificate A. The vivisector has only to form the opinion that the result of his experiment has not been obtained, and the animal can be subjected to as much pain as he thinks fit to inflict upon it. That is all I have to say on Charge 1. My next charge is that I charge the Home Office officials with having constituted themselves the injudicial defenders of the vivisectors from criticism by my Society in the past, and in their evidence tendered before this Commission. I wish to draw the attention of the Commission to Dr. Crile's book on "Surgical Shock."

10276. (*Sir Mackenzie Chalmers.*) There is one preliminary point I should like to ask you. On what do you base the distinction that you draw between the Home Office and the Home Secretary—between the officials and the Home Secretary?—I draw it on this ground. I suppose you are aware, like everybody else, that the answers prepared for the Home Secretary are prepared in the Office; I have common sense enough to know that, and I presume that the Office is therefore responsible for the answers given by the Home Secretary; that is all. I make no distinction, except that I do not attribute them to the Home Secretary himself.

10277. You do not surely regard the Home Secretary in answering to Parliament as a mere mouthpiece—a sort of telephone from the Office?—It is a matter of degree.

10278. (*Chairman.*) That is what you do regard him as, I understand?—The answers, I have not the slightest doubt, are prepared for him in the Office.

10279. They are prepared and drafted in the Office, no doubt, but the Home Secretary is responsible; and not only is he responsible, but, I think, not infrequently he suggests that they should be different?—It is not for me to draw a distinction; I am not inside the Home Office.

10280. (*Sir Mackenzie Chalmers.*) Constitutionally the Office does not exist; there is only the service of the Home Secretary, but practically, of course, everything has to be prepared for the Home Secretary, and he may or may not, according to his individuality, exercise his right?—That is too technical a point for me; that is for the Home Secretary and his officials to settle between them. I am quite aware that all the charges which I am now bringing have nothing to do with Sir Mackenzie Chalmers, because he was not there; and further, the Home Secretaries are gone too. There stand the permanent officials.

10281. (*Chairman.*) You put the blame on the permanent officials?—Certainly.

10282. (*Dr. Wilson.*) On the Department?—Yes, I regard the Department as my adversaries in this matter.

10283. (*Chairman.*) I think my expression was more accurate, because the Home Secretary is one of the Department; he is the head of it?—Yes.

10284. (*Sir Mackenzie Chalmers.*) I only wanted to know the reason for it?—That is it. I refer now to this book on "Surgical Shock," and somebody had better verify my quotations. Would you like it, my Lord?

(*Chairman.*) As this is relating to the Home Secretary, perhaps Sir Mackenzie had better have it before him, and I will listen.

(*Sir Mackenzie Chalmers.*) I have never seen it. (*The book was handed to Sir Mackenzie Chalmers.*)

(*Witness.*) If you look at Experiment CXXXIII—I do not know the exact page—you will see the description.

10285. Let us see who Mr. Crile is?—He is an American who came over here and had a licence given him from the Home Office to perform certain experiments, and he did them in Sir Victor Horsley's laboratory. The first 16 were done in England, and I will not allude to the others, except that just in passing I must allude to two which were not done in England;

and I will tell you why. I want to show that the personal taste of Dr. Crile does not prevent him from torturing animals. In experiment CXXXIII. we find these words (I am just going beyond England for the moment); he says "Experiment lasted two and a-half hours. Flame was applied to the dog's paw, and we are told, in the control experiments, as well as in this, the dog was not under full anæsthesia. In the former the animal struggled on application of the flame."

10286. That was an American experiment?—I think so; it was not done in England, anyhow.

10287. And he put as the anæsthetics chloroform and ether?—I do not care what they were; they were not full.

10288. (*Chairman.*) You only read that experiment to show that Dr. Crile is capable of performing what you consider a cruel operation?—Yes; and there is one other experiment, No. CXXVII., just before that one, "The animal did not take the anæsthetic well, and a part of the experiment was made under incomplete anæsthesia, the anus and rectum of this dog were then 'dilated.'" These quotations I merely read to show that the personal taste of Dr. Crile does not prevent him from inflicting torment, in my opinion. Now I come to what was done in Sir Victor Horsley's laboratory. I will go by steps. On page 17, Dr. Crile states "In the first 16 experiments which were carried out in the University College, London," so and so.

10289. You now, I understand, are going to read descriptions given by Dr. Crile of operations carried out by himself in Sir Victor Horsley's laboratory?—Yes, that is how he describes them himself in his own book. On page 23, Experiment V, performed on the 8th of May, 1895, is thus described:—"Healthy male foxterrier, three years' old. Central pressure in femoral. Preliminary section of the inferior branches of the right stellate ganglion, by dissection between the scapula and the spinal column, resecting two ribs. Under incomplete anæsthesia, crushing of foot caused a very sharp rise, followed by an equally sharp decline of pressure. This was repeated several times." Here we have Dr. Crile's own description of crushing the animal's foot under what he himself calls "incomplete anæsthesia." That was done by Dr. Crile under a simple licence, which provided that the animal must be under complete anæsthesia during the whole experiment, and be killed before it recovers consciousness. I want to point out that in Dr. Poore's Report these 16 experiments were not described as painful, and therefore we must assume that the Home Office regarded them as painless, or chronicled them as painless. Over Dr. Poore's signature are these words:—"In experiments performed under licence alone, the animal suffers no pain, because complete anæsthesia is maintained from before the commencement of the experiment until the animal is killed." Therefore the Home Office Inspector, in his own Report, states for the public delectation that animals when vivisected under licence alone are always under complete anæsthesia.

10290. Are the words "incomplete anæsthesia" in the account which Dr. Crile gives of his own experiment, the account which has been given in by him to the Home Office?—Whether it was sent in to the Home Office or not I do not know.

10291. It makes a difference, when you are bringing a charge against the Home Office?—I laid the book before the Home Office afterwards.

10292. It makes a difference whether Dr. Poore was informed that it was done under "incomplete anæsthesia." We are informed in this book by Dr. Crile that it was so; but I presume that Dr. Poore's Report would be founded upon the Report sent in to him by Dr. Crile?—We have it in evidence—I have read it—I cannot at the moment refer to it, but I am sure I can find it—that it is an instruction to all vivisectors to send to the Home Office any pamphlets or publications detailing and accounting for the experiments which they are licensed to perform; and, therefore, I assume that if Dr. Crile did his duty and fulfilled the conditions upon which his licence was given to him, he must have sent that book to the Home Office.

10293. We do not know that it was printed at the time?—Anyhow, I sent it to the Home Office later on.

10294. I mean, that we do not know whether at the time it was before Dr. Poore?—That is a matter for the Home Office to answer. I do not know; I cannot tell.

10295. (*Sir Mackenzie Chalmers.*) It is an American

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book, and curiously enough it does not seem to have any date. It seems to have been an Essay which was awarded the Cartwright prize for 1897?—Yes.

10296. That is two years afterwards?—Yes.

10297. (Chairman.) That book itself could not have been submitted to the Home Office at the time. However, when we examine the Home Office Reports, we shall see what there was?—I am coming on to that in a moment. I then drew the attention of the Home Office to these experiments which were done in Sir Victor Horsley's laboratory.

10298. When you say then, will you tell us when that was?—I wrote to the Home Office on the 18th May, 1899.

10299. (Sir Maekenzie Chalmers.) That is four years afterwards?—It may have been.

10300. From 1895 to 1899?—Yes, that is right. I do not have these books sent to me, you see. Somebody at last sends me a thing, and I at once act when I see it. Then the Home Office asked me to lend them the book—that happened next; so I lent them the book; and their answer was that on receiving my letter, "The Secretary of State caused investigations to be made into these experiments, and the information he has received from competent persons, who either witnessed most of the experiments, or have read the account of them, is to the effect that in no instance had the animals experimented upon been subject to pain."

10301. (Chairman.) You told us that you wrote to the Home Office calling attention to this. Was your letter calling attention to Dr. Crile's book generally, or was it calling attention to the statement that this particular dog was under incomplete anaesthesia?—I drew their attention to the experiments which Dr. Crile had done in Sir Victor Horsley's laboratory, and I drew their attention to their own statement in their corresponding statement for the same year alluding to complete anaesthesia, and pointed out the discrepancy. I say that these investigations which the Home Office made can, so far as I can see, have had no other nature than inquiry from the persons responsible, and, therefore, on their defence. It is quite incredible to suppose that inquiries as to whether a dog was put to torture in a particular laboratory of the persons who are responsible for what goes on in that laboratory, will result in anything but a distinct assurance that no pain was inflicted. Human nature being what it is, you cannot expect anything else. I say that those investigations can have had no other nature than inquiry from the person whose conduct is impugned—whether he did it or not; and I say that to make such an inquiry as that, and then write back to me and give me the result of those inquiries as settling the matter, is, in my opinion, simply defending the vivisectioners from criticism, and nothing else. They went on and said: "The use of the term incomplete anaesthesia has probably given rise to the misunderstanding. With regard to this," the Home Secretary says, through the hand of Mr. Cunyngame, "the Secretary of State is advised that there are various degrees of anaesthesia. In 'complete anaesthesia' the narcosis is very profound, and the 'reflex movements,' that is the involuntary movements in response to an external stimulus, are suppressed. One of the last of these reflex movements to disappear is the blinking of the eyelids when the surface of the eyeball is touched, thence called the 'corneal' or 'conjunctival reflex.' But short of this is the condition termed by Dr. Crile 'incomplete anaesthesia,' in which the creature is quite insensible to pain, although the corneal and other reflexes can still be obtained." Now "it is quite usual for even severe operations to be performed on man in this state of 'incomplete anaesthesia.'" That is the statement of the Home Office, presumably advised by—well, I do not know whether it was Dr. Poore, I suppose it was; they do not tell me by whom they were advised, and of course I cannot tell.

10302. On what ground do you assume that it was Dr. Poore?—I would assume that Dr. Poore is their adviser, and in the next charge I shall proceed to show that Dr. Poore was a man of very strong bias, wholly incapable, unless through very serious and strenuous effort, of forming an unprejudiced opinion on such a matter as this.

10303. I am afraid that we find a great many of those people on both sides?—No doubt, my Lord, but not, I should hope, in an official position. I am not in an official position. I admit all sorts of prejudices.

10304. I would not go so far as to say wholly in

capable of forming an opinion; you might say considerably prejudiced?—Well, considerably prejudiced—biassed I will say. Having received that letter, of course I had to consider what was my position. Here was the Home Office ready to tell us, on the advice of some nameless adviser who no doubt took the responsibility upon himself, that it was "quite usual for severe operations to be performed on man in this state of incomplete anaesthesia," that is, before the corneal reflexes were abolished. Well, I thought that I would test that, and I took the liberty of writing a letter to a certain number of the most important and eminent surgeons of the day, informing them that I had been told on very high authority that "surgeons now frequently perform severe operations on man under incomplete anaesthesia, that is, before the corneal reflexes are abolished." Those are the words of my letter to the surgeons. I put it perfectly fairly, exactly as the Home Office put it to me. The replies were very remarkable.

(After some deliberation.)

10305. (Chairman.) As some of the Commissioners wish the letters to be read, will you please read them?—In the first place, I ought to read my letter to the surgeons. It was as follows: "Dear Sir,—A statement has recently been made to me in writing by a very respectable authority that surgeons now frequently perform severe operations on man under incomplete anaesthesia, that is, before the corneal reflexes are abolished. I know of no way of answering this statement but by obtaining definite information from persons like yourself, whose large experience renders their authority on such a matter conclusive. I should, therefore, really be very much obliged to you if you would be so good as to enable me clearly to assert on your authority that, provided complete anaesthesia can be given with safety to a patient, and provided the nature of the operation itself does not render it dangerous to the patient's life, complete anaesthesia is always administered in severe surgical operations. You may possibly be aware that I am one of those who on humanitarian grounds oppose the vivisection of animals as practised in this country, but I hesitate to believe that patients in hospitals are treated with inhumanity. I hope you will pardon this intrusion upon you, which arises from the prominent position you occupy in your profession.—Believe me to remain very faithfully yours. (Signed) Stephen Coleridge." I will now read some of the answers that I received. I ought to say that I am not going to pick out those which favoured me at all. I will give you the whole. There were only two of an adverse view who associated themselves with the Home Office view, and those two replies were from licensed vivisectioners who were unconnected with any hospital. The first is from Dr. Bond, F.R.C.S., senior surgeon and lecturer at the Westminster Hospital. He says: "I think you have been very much misinformed, and it would be very interesting to me to know the very high authority on which your information is based. Hundreds of operations are performed yearly at Westminster Hospital, and never to my knowledge is the operation commenced under incomplete anaesthesia." I informed Dr. Bond afterwards that the information reached me in a letter from the Home Office. Dr. Edmund Owen, F.R.C.S., Member of the Council of the Royal College of Surgeons, senior surgeon at St. Mary's Hospital, and senior surgeon at the Hospital for Children in Great Ormond Street, says: "Certainly, in my experience complete anaesthesia is always secured in severe surgical operations."

10306. (Mr. Tomkinson.) Are those the whole of the letters?—No.

10307. Did the first letter not refer to the question of the eye?—No; they answered my letter. I defined incomplete anaesthesia in my letter, "that is, before the corneal reflexes are abolished."

10308. There is no allusion to that in the reply?—No, they have taken my definition and used the words "incomplete anaesthesia." Dr. Timothy Holmes, F.R.C.S., Vice-President of the Royal College of Surgeons, and treasurer and consulting surgeon at St. George's Hospital, says: "I differ entirely from your informant who says that it is quite usual for even severe operations to be performed on man in a state of incomplete anaesthesia. . . . Never did I hear of performing serious operations with incomplete anaesthesia, and if you know anything of operative practice you would know that it would be as embarrassing to

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the operator as painful to the patient. . . . If you accept this statement as true you are quite at liberty to make use of it to your informant; or if your informant will, in his own name, repeat his statement in any respectable paper, I will in my own name repeat the above." Dr. Jonathan Hutchinson, F.R.C.S., late President of the Royal College of Surgeons, consulting surgeon at the London Hospital, and consulting surgeon at the Royal London Ophthalmic Hospital, says: "I have not the least doubt that when complete anaesthesia is considered safe, and on the patient's part desirable, it is always secured." Dr. G. May, consulting surgeon at the Royal Berkshire Hospital, says: "I have been attached to the Royal Berks Hospital about forty-five years, and have never known a case of operation where anaesthesia was not complete. This is done not only to spare pain, but because it is thought to be less dangerous." Dr. T. J. Walker, M.R.C.S., who has been for a very long time senior surgeon at the Peterborough Infirmary, says: "Taking my authority for what it is worth, you may assert on my authority that, provided complete anaesthesia can be given with safety to a patient, and provided the nature of the operation itself does not render it dangerous to the patient's life, complete anaesthesia is always administered in severe surgical operations." And this last one is most interesting. Dr. Thomas Bryant, F.R.C.S., Surgeon Extraordinary to Her Majesty the Queen, consulting surgeon at Guy's Hospital, and late examining surgeon at the University of Cambridge, says: "I have received your circular, and read it with surprise, as you clearly have been hoaxed." I replied to that letter that the august quarter from which the information emanated seemed to me to preclude such an hypothesis. I had two replies from two licensed vivisectioners, one from Professor Horsley, to the effect that he was out of the country, and the other from Professor Schäfer, that the Home Secretary's advisers are quite correct, and that incomplete anaesthesia "is consistent with complete unconsciousness." As I say, Sir Victor Horsley did not answer, but Professor Schäfer at that time was not, and, so far as I know, still is not in any way connected with an hospital.

10309. You said that there were two who differed?—I thought that there was. I see one did not answer; it is my mistake.

10310. (Dr. Gaskell.) Did you say that Professor Schäfer said that the Home Office statement was correct?—Yes, in reply to that letter, although I gave no indication that it was the Home Office at all; he deduced, no doubt, that it was the Home Office.

10311. You had not told him that it was the Home Office?—No.

10312. (Chairman.) Are those the whole of the letters?—Yes, except one which I left out.

10313. I did not mean were they all the letters, but have you read the whole of each letter?—I believe so; but I can get the originals. I am not sure. I did not read one from Dr. Herbert Snow, surgeon at the Cancer Hospital, who says, "You must have been misinformed so far as my knowledge goes upon the point at issue," as he has been a witness here.

10314. (Colonel Lockwood.) Those are not *précis* of the letters; they are the entire letters?—So far as I know. I placed all the replies before the Home Secretary in a letter to the Home Office, and I received no further reply from them. I remain of the opinion that those collected opinions of those great men outweigh the anonymous advice received by the Home Office. I will not say that Dr. Poore, but whoever it was, I think he misinformed them; and at any rate, I maintain that with those letters before them the Home Office ought to have recognised that they were of great weight. But they never answered me in any way, and, so far as I know, Dr. Crile was never censured.

10315. (Sir Mackenzie Chalmers.) Do you know when Dr. Crile returned to America?—No. I will not be quite certain, but I think that even subsequently to those experiments, and subsequently to the receipt of all those letters from me by the Home Office, Sir Victor Horsley, in whose laboratory the crushing of that dog's foot under incomplete anaesthesia took place, still received permission from the Home Office to vivisection where he liked, and not in any registered place.

10316. (Chairman.) Was Dr. Crile at that time on a visit to England?—So far as I know.

10317. He never took up his abode here to practice?—

No, he went back to America, and he has issued another book since in America. Now the next point I will take up on this same charge is in relation to a question asked by Mr. Weir of the Home Secretary.

10318. (Colonel Lockwood.) You mean the present Member for Ross and Cromarty?—Yes, I think he is. "I beg to ask the Secretary for State for the Home Department if he will state the number of persons in England, Scotland, Ireland and Wales respectively, licensed to practice vivisection, and how many of them hold certificates to dispense with anaesthetics." Sir Matthew White Ridley replied, "The number of persons licensed at the present time in England is 145; in Scotland, 52; in Wales, one. The number holding the certificate dispensing with anaesthetics is, in England, 86; in Scotland, 30; in Wales, none. The only figures in my possession as regards Ireland are those for 1895. In that year the number of licensees was six, of whom one held a certificate dispensing with anaesthetics." Now comes the point; this is a voluntary statement on the top of the answer: "In giving the honourable Member these figures, I may remind him that the certificate in question is never given for operations involving serious pain, but only for such operations as inoculations or hypodermic injections." That answer, I am going to assume, was prepared for Sir Matthew White Ridley in the Home Office, and I say that that answer might be intended for the public to believe, and undoubtedly had the effect, and would have the effect, of persuading the public that in all cases of certificates exempting the licensee from the use of anaesthetics, such certificates are only issued for such operations as inoculations or hypodermic injections, when all the while there is Certificate B being issued; and I say that such an answer as that is not a fair answer. That statement of Sir Matthew White Ridley's, I maintain, is quite unfair. It really only refers to Certificate A, and Certificate A plus E, and omits all reference to experiments under Certificate B. But the question does not omit reference to Certificate B. The question is: "How many of them hold certificates to dispense with anaesthetics." I say Certificate B dispenses with anaesthetics.

10319. (Sir Mackenzie Chalmers.) No?—Certainly Certificate B dispenses with anaesthetics immediately after the initial operation is over; and under Certificate B the moment the initial operation is over (the opening of the animal and fixing the electrodes to the ends of the nerves and tubes down the veins), there is nothing in the law and nothing in the certificate to prevent the animal remaining disembowelled on the table for hours while observations are made upon its blood pressure and various things, while stimulations of electricity are passing through it. Under the present law there is nothing to prevent it, and I say that that is what horrifies the public; and that is carefully hidden in this answer.

10320. (Chairman.) I have not been able to follow you, not having the question and answer before me, but do you say that looking at the question the answer is distinctly a misstatement, or is it something which a person who did not know the Act might misapprehend?—That is it.

10321. That is a very different thing, of course. A person who asks a question in the House of Commons is generally supposed to know with reference to what he is asking it, and he can understand the answer?—Perhaps I have made a mistake. The question, in my opinion, involves Certificate B; the question certainly involves "How many of them hold certificates to dispense with anaesthetics." Certificate B is a certificate dispensing with anaesthetics.

10322. (Sir William Church.) No?—I beg your pardon; it is so worked in practice, and it is so chronicled every year in the report under certificates dispensing with anaesthetics.

10323. (Sir Mackenzie Chalmers.) The animal is allowed to recover. May I just call your attention to the words of Certificate B; it is on page 7. Have you got a copy?—I have not got the Certificate here at the moment, but I know it by heart.

10324. It authorises a person "To perform on living animals certain experiments described below, such animals being, during the whole of the initial operation of such experiments under the influence of some anaesthetic of sufficient power to prevent their feeling pain"?—Yes, during the whole of the initial operation of such experiment, the experiment consisting of first opening the dog, and then chronicling what may

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happen to the blood pressure and other things, during hours, perhaps, of subsequent experiments.

10325. (*Dr. Gaskell.*) No; that is all the initial experiment?—No, I beg your pardon.

10326. (*Chairman.*) I do not think this comes too much, really, as a charge against the Home Office; because it is admitted that the answer to the question is one which, technically to a person who understood it, is a correct answer, but one which might lead a person who was not familiar with the Act to misapprehension. It is really putting too much upon the Home Secretary to suppose that he is to make his answer with reference to a person who does not study the Act?—The question is a very clear one. The question asked by Mr. Weir was how many of these gentlemen hold certificates to dispense with anæsthetics, and the Home Secretary gives only those who have Certificate A. He suppresses apparently those who have Certificate B, because he adds the rider: "In giving the honourable member these figures I may remind him that the certificate in question"—that must mean only Certificate A—"is never given for operations involving serious pain, but only for such operations as inoculations or hypodermic injections." I say that that is a disingenuous answer.

10327. To me, hearing you read it, it seems rather a forced construction to put upon it. I think that the unbiased person, whom we have not yet been able to find, would say that it was rather a forced construction to put upon it?—The question involves all certificates dispensing with anæsthetics.

10328. (*Sir William Collins.*) Are the words "Certificate A" mentioned in the question?—No; the words are: "Certificates to dispense with anæsthetics." I say that Certificate B dispenses with anæsthetics, and therefore it should have been included in the answer.

10329. Did you say that you were going to refer to some return indicating that Certificate B is classed with those certificates dispensing with anæsthetics?—Certainly. There is in the last Report: "Certificates held by each licensee dispensing with the obligation to kill the animal before recovering from anæsthesia"; that is coupled with Certificate E E, and is called "Permitting experiments on dogs, cats, etc." "Dispensing with the obligation to kill the animal before recovering from the anæsthesia."

10330. (*Chairman.*) It is not expressed there that Certificate B is to dispense with anæsthesia. It really seems to me a small point, I confess. You must have some more important things to come to?—Very well, my Lord; I pass from that then. I next come to the case of the "Chemist and Druggist," of December the 14th, 1901, in which there is an account of an address delivered by Professor Sherrington to a number of chemists and druggists, when the professor is reported to have called upon Dr. Grünbaum to demonstrate the action of the snake antitoxin upon some rabbits. The Report then goes on: "The first was inoculated with cobra venom, the second with antivenom, and the third with the cobra venom and antivenom. The first died in about 30 minutes, completely paralysed by the venom of the snake. The second, although it had also been subsequently inoculated with cobra venom, lived, and was full of life. The third, which had subsequently been inoculated with the anti-venom, also lived, and exhibited a considerable amount of liveliness."

10331. (*Colonel Lockwood.*) Where was this?—I think in Liverpool.

10332. (*Sir Mackenzie Chalmers.*) What was the date of it?—It was published in the "Chemist and Druggist" of the 14th December, 1901. The points that I raise here on this are these:—First of all, I say that Dr. Grünbaum here broke the law. He broke it first, according to this account, by demonstrating at a lecture without Certificate C; that is the first breach of the law that he made—he had no Certificate C, and he was called upon by Professor Sherrington to demonstrate the action of the snake antitoxin upon some rabbits, and he proceeded, according to the account, to do so. Secondly, he appears to me to have broken the law by performing an experiment merely to demonstrate what was already known with this Certificate A; he has Certificate A, but it was to demonstrate something that was already known.

10333. (*Chairman.*) It would not be against the law on his part if it was certified to be a proper case for experiment, would it?—Yes.

10334. With Certificate A?—Yes.

10335. Even although it was passed by the President of the College of Physicians or Surgeons?—Yes, Certificate A does not permit you to get outside the Act. If you are to demonstrate something that is already known you cannot do it. Section 3, Sub-section (1) of the Act provides that the experiments must be performed "With a view to the advancement by new discovery of physiological knowledge or knowledge which will be useful for saving or prolonging life or alleviating suffering."

10336-7. (*Sir Mackenzie Chalmers.*) Generally?—No, it is a question of commas; there are no commas at all. (*After a pause.*) I say "A new discovery." Then there ought to be a comma: "Of physiological knowledge or knowledge which will be useful to them for saving or prolonging life." It must all be a new discovery.

10338. (*Dr. Gaskell.*) No?—Certainly.

10339. (*Chairman.*) A new discovery of knowledge, do you mean?—Yes, it must either be a new discovery of physiological knowledge, or other kind of knowledge which will be useful to them for saving life, or alleviating suffering. It must be for a new discovery of one of those two forms of knowledge.

10340. Surely the terms are that the experiment must be performed "With a view to the advancement" (leaving out some words for the moment) "of knowledge"?—No; it is the new discovery that is the point of the whole section, surely.

10341. The advancement by a new discovery of physiological knowledge?—But the new discovery is the whole point, I think.

10342. Yes, but the new discovery is "The advancement of physiological knowledge, or of knowledge"?—Yes, I think you must take it so; because if you look over the page there is a certificate exempting the holder of it from this particular.

10343. But what I rather meant was. Supposing that a description of what is proposed to be done is sent in, and a licence is asked for, it would not be necessary in that case, if he was licensed generally; he could do the experiment under the general licence, if he had it under Section 1?—No, not without anæsthetics.

10344. No, with anæsthetics?—There was not any anæsthetic in this case; there is no suggestion of it.

10345. But Sub-section (1) is the sub-section which deals with the performance of an experiment which must be done under anæsthetics?—No.

10346. Then the other Sub-sections create an exception?—No, you begin at Section 3, Sub-section (1); you have not got to the question of anæsthetics at all then. It is the beginning of the Act; it begins at the very beginning; it says that you must not do these things except for the purpose of new discovery, that is by Sub-section (1)—nothing else. You must not go on doing the same thing over and over again. That is what it is for. It has nothing to do with anæsthetics at all. The question of anæsthetics comes over the page.

10347. (*Sir William Collins.*) Is your point that, apart from certificates altogether, this was a case in which either a licence could not have been granted for such a purpose, or if a licence had been granted he was transgressing it?—Yes; if a licence was ever held at the time I am prepared to show that Dr. Grünbaum broke the law in what he did.

10348. (*Chairman.*) Sub-section (4) is material, is it not, "Experiments may be performed not directly to the advancement by new discovery of physiological knowledge, or of knowledge which will be useful for saving or prolonging life or alleviating suffering, but for the purpose of testing a particular former discovery"?—Yes, that is so. He had not got that certificate either.

10349. (*Sir William Collins.*) Is that Certificate D?—Yes; he had not got that either. Therefore he was absolutely outside the law, and had no business to do it. He first broke the law, I say, by demonstrating at a lecture without Certificate C. This was really a lecture where there were a large number of persons present—chemists collected there to hear the lecture, and he was invited by Professor Sherrington to do this particular thing which he had no right whatever to do, and he performed the experiment merely to

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demonstrate what was already known with Certificate A; therefore he violated the section which I have just read, Section 3, Sub-section (1). And, further, I contend that it is highly probable that by performing an experiment before the general public he violated Section 6; because I see no reason why a collection of chemists should be regarded as anything else than the general public. Section 6 says: "Any exhibition to the general public, whether admitted on payment of money or gratuitously, of experiments on living animals calculated to give pain shall be illegal." I do not see why a collection of chemists who go to a place should be regarded as anything else but the public. Therefore I maintain that under several headings Professor Grünbaum was entirely outside the Act of Parliament. I drew the attention of the Home Office to these various performances of Dr. Grünbaum, and they replied that it would have attention—that is the first thing. I drew their attention to it on the 17th December, 1901; I waited till the 20th February, 1902, and had no reply. I asked them then if they would kindly give the matter their attention, and then they wrote back to say, "I am directed by the Secretary of State to inform you that he has caused careful inquiry to be made into the matter. The experiments were performed under a Certificate A, the authorities who granted the certificate being, no doubt, of opinion that such experiments tended to the advancement, by new discovery, of physiological knowledge, or of knowledge which would be useful for saving or prolonging life or alleviating suffering."

10350. (Colonel Lockwood.) That is Sub-section 4?—Yes, that is it, Sub-section 4 of the proviso. That is practically indicating that the authorities who granted the certificate granted him Certificate D. But he had not got that.

10351. (Chairman.) Would you read the words again. That is the same Sub-section (1), is it not?—No, I am on Sub-section (4), which would take him out of Sub-section (1).

10352. What was it you were reading?—I was reading from the letter of the Home Office to me.

10353. What does the letter of the Home Office say?—The letter of the Home Office says: "The experiments were performed under a Certificate A," not D, "the authorities who granted the certificate being, no doubt, of opinion that such experiments tended to the advancement, by new discovery, of physiological knowledge, or of knowledge which would be useful for saving or prolonging life or alleviating suffering."

10354. Those are the words of Sub-section (1)?—Sub-section 4.

10355. They are the exact words that I have in my hand of Sub-section (1)?—Your Lordship is quite right—Sub-section (1). "Dr. Grünbaum states that the report of the proceedings is somewhat misleading, in that the experiments were not performed until after Professor Sherrington had finished his address, and that they were not in illustration of a lecture. He has, however, conveyed an assurance to the Secretary of State that such experiments in similar circumstances will not be repeated." In my opinion, and I think in the opinion of any fair person, it is quite clear that the Home Office asked Dr. Grünbaum what his account of this was, and Dr. Grünbaum said that the newspaper was all wrong, that this perfectly impartial person I suppose had called upon Professor Grünbaum to demonstrate the action of snake poison, and he demonstrated it to nobody at all—they had all gone apparently. But it is quite clear that the reporter was there.

10356. (Dr. Gaskell.) May I ask, were these experiments done in a physiological laboratory?—No doubt it was in a registered place; I am not saying that it was not. I do not suggest that it was not.

10357. Was Dr. Grünbaum working at snake poison at the time?—I do not know; I suppose he was. That I know nothing about.

10358. After the lecture was over he went on with his experiment, and some of the chemists came and looked on?—You are putting that to me?

10359. That is the suggestion of Dr. Grünbaum, I mean?—No, I do not think that is quite so. "Dr. Grünbaum states that the report of the proceedings is somewhat misleading, in that the experiments were not performed until after Professor Sherrington had finished his address." The "Chemist and Druggist"

does not say that. The "Chemist and Druggist" says that when Professor Sherrington had finished his address he called upon Dr. Grünbaum. Dr. Grünbaum states "that they were not in illustration of a lecture."

10360. That is all I meant to convey?—Dr. Grünbaum considered that he was going on with work which he was doing before, and the chemists and druggists saw it.

10361. (Sir John McFadyean.) Have we any information as to the precise object of Professor Sherrington's address?—It is headed "A Physiological Experiment in Relation to Pharmacology."

10362. (Chairman.) That would not be on snake remedies?—I am not a scientific man, my Lord. Would you like to look at it?

10363. (Sir John McFadyean.) Therefore, if Professor Sherrington was lecturing on an entirely different subject, Dr. Grünbaum's demonstration could hardly be part of the lecture. Would you mind letting me look at it?—Certainly. (Handing a newspaper to Sir John McFadyean.) You see the newspaper reporter asserts practically that Dr. Grünbaum demonstrated this to the chemists and druggists assembled, and to himself too. He describes what he saw. It took 30 minutes; the wretched animal died in 30 minutes completely paralysed by the venom of the snake. That is his description of what he saw. I maintain that he was one of the public, and Dr. Grünbaum broke Section 6 of the Act.

10364. (Sir Mackenzie Chalmers.) Just on that point do you say that if one single member of the public is present at an experiment, that breaks the Act?—Yes, one genuine member of the public.

10365. For instance, if you or I went in?—Certainly I should consider myself one of the public.

10366. If any one of the public went in?—I have been invited to go into laboratories, but I do not propose to assist physiologists to break Section 6.

10367. That is your view of the law?—Certainly. I do not set up to be a very great lawyer, but Section 6 seems to me to be quite clear. If I may go in, I presume that anybody may go in—a cabman off the next rank, or anybody.

10368. (Chairman.) You have given us the complaint made, and the answer of the Home Office?—Yes.

10369. And nothing more happened?—No. I think Dr. Grünbaum got his licence again.

10370. (Sir John McFadyean.) Professor Sherrington does appear to have referred to snake poison in his lecture?—Dr. Grünbaum still has a licence. That is the complaint I have to make upon that. I will just rehearse it again. I maintain that he broke the law by demonstrating at a lecture without Certificate C, by performing an experiment merely to demonstrate what was already known by Certificate A, and by performing an experiment before the general public in contravention of Section 6. I maintain that those breaches of the law took place, and that the Home Office associated itself with his defence. Now, the next case I want to draw attention to is that of Messrs. Dixon and Brodie, in the "Journal of Physiology," volume xxix., No. 2.

10371. (Sir Mackenzie Chalmers.) Will you give us the date?—The "Journal of Physiology" is very badly dated, but I give the page. It is page 144. I think at the very beginning they give you the date; but those volumes are a collection of papers. On page 144 these two vivisectors make the following statement: "In studying these reflexes we have found it of the utmost importance to avoid the use of chloroform or ether as the anæsthetic. The experiments must, therefore, either be performed upon unanæsthetised animals, upon animals anæsthetised with morphia, or upon decerebrate animals. Our experiments were usually conducted under one of the two latter conditions, but in a few instances were repeated upon animals lightly anæsthetised with chloroform." We say that this kind of light anæsthetisation—when we are told that to be of any use, if the animals have not morphia or are not decerebrated, they must be unanæsthetised—awakes in us the profoundest distrust.

10372. Could you let me look at that paper?—Certainly (handing the volume to Sir Mackenzie Chalmers).

10373. (Chairman.) Was complaint made of it to the

Home Office?—Yes, I made complaint. This was defended by the Home Office, so far as I can make out, on the bare assurance of the authors of the paper; they speak of "The authors of the paper."

10374. (*Sir Mackenzie Chalmers.*) The paper itself does not refer to any specific operation or any specific operation on any specific animal?—I daresay it does not. I am dealing with the particular passage which I have quoted.

10375. (*Chairman.*) On some animals lightly anaesthetised?—"Upon animals lightly anaesthetised with chloroform." It was a long time before I got an answer to that complaint either. I wrote on the 30th of March, 1903, to the Home Office, and I did not get a reply till the 9th of July; they then answered me, and this is what they say. "Adverting to your previous letter, I am to point out that on page 102 of the volume referred to it is explicitly stated that, except in the case of animals killed by pithing, all were anaesthetised. On page 144 it is said that in a few instances the animals were lightly anaesthetised with chloroform. The authors of the paper explain what is denoted by the term 'light anaesthesia,' viz.: that the animals were anaesthetic"—that is the word; "*i.e.*, they could not feel pain, but that the anaesthesia was not carried so far as to abolish all reflexes. The inquiry which has been made confirms that none of the experiments described in this paper were performed on unanaesthetised animals"—I did not say that they were—"Under these circumstances there is no reason for thinking that the provisions of the Act, viz., that the animals must, during the whole of the experiment, be under the influence of some anaesthetic of sufficient power to prevent the animal feeling pain, was not complied with." So that the Home Office defends an experiment upon an animal of which the authors say that these particular experiments must be performed either upon unanaesthetised animals, or upon animals anaesthetised with morphia, or upon decerebrate animals, and they allude to experiments not under the last two conditions, the experimenters themselves saying that they must be unanaesthetised to be of value. They cover up the unanaesthetisation, in my opinion, by speaking of them as "lightly anaesthetised with chloroform"; because if they were anaesthetised properly with chloroform, according to their own description, the experiments would be valueless.

10376. We have had doctors giving their account of what they understand by the expressions "lightly anaesthetised" and "completely anaesthetised"; but it is a medical question, is it not? Supposing that the Home Secretary, who is not himself a skilled physiologist, is informed that "lightly anaesthetised" is an expression which is consistent with the animal not feeling pain, would not the Act then have been complied with?—That entirely depends.

10377. I mean, as regards the charge against the Home Office. I am not considering whether Messrs. Dixon and Brodie's experiments were lawful?—The answer that I should make to that question is, that it depends upon where the Home Secretary goes for his information. If he goes to the Association for the Advancement of Medicine by Research, which is composed of vivisectioners, I should say that the information is not sufficient.

10378. Then would you go so far as to say that the Home Secretary must go to somebody who holds anti-vivisection opinions?—Certainly not. Let him go to Mr. Sewell—to some person who is always operating on dogs for their benefit; let him go to some well-known thoroughly-skilled dog doctor.

10379. You exclude the whole medical profession from that—the ordinary M.D.'s?—So far as I know the ordinary M.D.'s, they are not vivisectioners, therefore their opinion as to what is proper anaesthetising for a dog would not have the same weight with me, and ought not, I presume, to have the same weight with the Home Secretary, as the expression of such a man as Mr. Sewell.

10380. Assuming that the Home Secretary went to a person who was a skilled physiologist, and he gave him that explanation, you say that he ought to appeal to a veterinary surgeon?—I think so. I think he should go to an impartial person, and I do not think, looking at it from the point of view of the Home Office, that a licensed vivisectioner is an impartial person on this question. And on this occasion you must remember to

whom the appeal was made—to the authors of the paper. What answer could the Home Secretary expect to get from the authors of the paper, whatever they said in the paper, except that the animal did not feel anything? He never could have expected the authors of the paper to write back and say "we tortured these animals."

10381. Supposing that the Home Secretary had advisers who advised him that "light anaesthesia" is an expression which may cover, in the minds of medical men, absence from pain—I do not know how it is—but supposing he had been so advised, would you say that he was to blame for saying that he found that the law had not been broken?—My answer would be this: that supposing in the former case he had consulted Sir Victor Horsley and Professor Schäfer, he would have got the answer which I have read, that of course, the experiment was completely painless. But if he had consulted Dr. Boyd, if he had consulted Mr. Jonathan Hutchinson, if he had consulted the great surgeons whom I consulted, he would have got the answer that I did, that such a thing was not anaesthetic, and that the corneal reflexes should be abolished before the operation is begun. My complaint against the Home Office is that they go to the vivisectioners themselves for advice as to what is painful, and that they have done so always. I shall come on by and bye to the provisions of my Bill, of course. I now come to another case. In Volume XXVI. of the "Journal of Physiology." On page xxxviii. of the Proceedings of the Physiological Society of March 16th, 1901, you will find that Mr. J. Herbert Parsons, in a report of his experiments on dilatation of the pupil from cerebral stimulation, makes the following statement:—"The following are some of the results obtained: (1) Dilatation of the pupil, like other effects of cortical stimulation, is best obtained with slight anaesthesia."

10382. (*Sir Mackenzie Chalmers.*) Is that a general abstract statement, or is he describing certain specific experiments which he made?—At this distance of time I cannot say; you have it there.

10383. (*Chairman.*) What is your complaint about it; did you complain to the Home Office?—Yes, I complained that they allow this slight anaesthesia, and the Home Secretary referred me to page 368 of that volume, where I should find it stated that "the animals in question were completely anaesthetised and insensible to pain throughout the experiments." That, I presume, is a general statement with which vivisectioners very frequently now begin a statement of all their experiments, and then when you come to detail you find these words, "slightly anaesthetised." They cannot both be true. Therefore all that the Home Office did when I pointed out that this man himself said that he did these experiments under slight anaesthesia was to point out that he himself, the same man, says somewhere else that the animals were completely anaesthetised.

10384. The whole question in this case, as in the last, seems to be whether slight anaesthesia means sufficient anaesthesia to prevent pain, or whether it means something less than that?—Yes.

10385. (*Colonel Lockwood.*) This same paper which you quote says, "The animals in question were completely anaesthetised and insensible to pain throughout the whole experiments"?—That is a general statement of the whole lot.

10386. (*Chairman.*) That is the statement, of course, of the physiologists there at work—I know you do not admit the statement—that though light anaesthesia it was complete anaesthesia in the sense of no pain being felt?—The word "*αναίσθητος*" means "without feeling, insensible." You must either be sensible or insensible. I decline to accept "slightly anaesthetised."

10387. We cannot press the derivation so far?—What does anaesthetised mean? It means without feeling. You cannot be slightly without feeling. You either feel pain or you do not.

10388. A drug which will produce anaesthesia means a drug which will produce that state of things, but whether a drug will produce that anaesthesia to a different extent, slight, incomplete, complete, and yet there may in each case be absence of pain on the part of the animal, I am not able to say. The term "light anaesthesia" seems to be used by physiologists, at any rate, as being an expression which does cover absence of pain?—If I may say so, I regard such expressions in

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the mouths of physiologists as expressions for the purpose of putting them inside the Act of Parliament.

10389. The Act of Parliament says nothing about slight anaesthesia at all?—It does not.

10390. It says that the animals must be under anaesthetics?—Yes.

10391. And there must be no pain?—Yes.

10392. The Act is complied with whether you put the word "slight" in or anything else if there is sufficient anaesthetic to prevent pain?—Yes.

10393. It comes back to what I said, whether in the mouth of those who used the term, assuming them to be speaking honestly, which you do not admit, the words "slight anaesthesia" cover absence of pain; they say that it does?—My point is that when they say that it does they say it to cover themselves from the Act of Parliament.

10394. The whole question is whether it is true?—We regard it with profound distrust, and we believe that if a properly authorised inspector were present he would see that the anaesthesia was complete.

10395. It would no doubt facilitate our inquiry if we were to adopt your view that we should believe nothing that is said by physiologists, and that those who speak on the other side are to be believed?—I do not take that line.

10396. That is rather what you suggest?—No, I say that when a person's own conduct with regard to the Act of Parliament is impugned, it does seem to me somewhat inadequate to go straight to him, the person impugned, and to take, without any question, his explanation of what he did.

10397. It was only with reference to an expression that you used just now that I said that. You assumed that it was said by physiologists in order to cover up a breach of the law, or you used some such expression as that?—A man is accused of a breach of this Act of Parliament.

10398. Not until after he has used the words?—After he has used the words he is accused of a breach of the Act of Parliament; he is asked by the Home Office to account for himself. What could he do to put himself inside the Act of Parliament but say that the words that he used, whatever they were, were sufficient for the Act of Parliament. You could get it down to an infinitesimal amount of anaesthesia, and you would find that they would maintain, if they were asked themselves, that it was sufficient.

10399. (Sir Mackenzie Chalmers.) These are the gentlemen who themselves saw the experiments, and conducted them; who else could say whether the animal suffered or not?—Quite so. There should be an inspector there. That is my answer.

10400. (Dr. Gaskell.) Have you ever seen such an experiment?—No; as I have said already, I do not propose to assist any vivisector to break Section 6 of the Act.

10401. (Chairman.) I see in Gould's Medical Dictionary "anaesthesia," the condition of total or partial insensibility, particularly to touch?—What is the date of that book? It is since the Act, I should think.

10402. It is 1900?—Exactly, that is since the Act?—Who is the author of it?

10403. I suppose Gould's Dictionary of the day would be since 1876?—But the definition of anaesthesia is brought up to date to fit the Act.

10404. You said the word anaesthesia in its medical sense?—No; I said in its English sense.

10405. No, in its Greek sense?—Yes, its Greek sense.

10406. But in its medical sense apparently there are some who contend that it means something less than that?—Yes.

10407. The dictionary is, I see, by Dr. George M. Gould, and it is published in London by H. K. Lewis, 136, Gower Street, 1900.

10408. (Sir William Collins.) It includes partial insensibility within the meaning of anaesthesia?—That is very important. Then I may take it that light anaesthesia as taken from that book does mean sensitiveness to pain, if that definition is to be taken.

10409. (Chairman.) Not from the book which I quoted from just now. This is of course the same gentleman who wrote the account, who said that at the time of the operation the animals were completely

insensible to pain?—Now we are getting off the dictionary on to the views of the person.

10410. Of course I merely quoted the dictionary, but it does not govern it at all; it was not very material, but it was apropos of what you were saying about the true translation of the Greek word.

(Sir Mackenzie Chalmers.) May I read the next definition of anaesthetic? It is rather curious: "Anaesthetic, without feeling, insensible to touch or pain; a substance that produces insensibility to touch or to pain; diminished muscular action, and other phenomena. Anaesthetics may be general, local, partial, and complete."

10411. (Chairman.) That is not so material. I thought perhaps the gentlemen when they were writing their account had present to their minds the definition?—My contention is very simple. My contention is that slight anaesthesia means a condition not insensible to pain.

10412. And you complain of the Home Office not having treated that as an offence after its being brought to their notice?—I say that throughout the Home Office have constituted themselves the defenders of the vivisectors from criticism, instead of being the impartial judges. I go on to the next charge now. I charge the Home Office officials with having appointed inspectors who have displayed such bias that they have thought it their duty not to make detective efforts to protect animals from illegal treatment. That, I think, is almost admitted, but as I am on it I should like to go on to show the bias of other persons besides Dr. Poore. That charge is based upon an answer of Sir James Russell's to Question 530: "When I took office he" (that is Dr. Poore) "wrote to me to tell me that I was to visit registered places about three times a year, but he expressly said that I was not expected to act as a detective," that is to say he was to have more consideration for the vivisectors than for the animals. That is what it amounts to in my opinion. He was further asked at Question 532: "Have you been instructed to make surprise visits," and his answer was "Never."

10413. Yes; I remember the evidence?—Dr. Busk, who was Inspector from 1876 to 1886, alluded to the agitation conducted by my society as "a senseless and mischievous agitation." That is not a wholly impartial description of us, I should think. Then Dr. Poore resigned, or, anyhow, left the Home Office. He ceased to be Inspector in 1899 some time after the correspondence which I had over Dr. Crile, and he signalled his departure from office by making an oration called the Harveian oration, delivered at the Royal College of Physicians in London by him, and it was reported in the "British Medical Journal" of October the 21st, 1899, which is in the room. In that he says: "So with regard to another burning question there be those who apparently hold the view that a guinea pig is of more value than many babies" (I do not know whether he is alluding to me; I never valued a guinea pig above a baby); "and that operations on the lower animals, no matter how carefully they be done, if done for the purpose of research, diagnosis, or relief, and not with the object of making mutton tender, pigs fat, or horses quiet, are to be denounced as atrocities. With such as these it is useless to argue. But seeing that many honoured members of our profession have themselves been vivisected by the envenomed tongues and sharp pens of a few noisy people, it may be well to point out that no conviction for cruelty or breach of law has ever been obtained." For a very excellent reason, that is that the Home Office themselves, who were responsible for Dr. Poore being Inspector, would have to advise the Home Secretary to give assent in writing to a prosecution. It is not very likely that with Dr. Poore there we should have got assent in writing, "and that hearsay and the misinterpretation of physiological writings is not evidence, at least on this side of the Channel."

10414. Is that so, that the Home Secretary cannot issue a prosecution without the consent in writing of Dr. Poore?—No; I say that the Home Office themselves are responsible for Dr. Poore being Inspector, and he would have to advise the Home Secretary to give assent in writing to a prosecution.

10415. What you said was that it is easily explicable, because the Home Secretary could not issue a prosecution?—I did not mean that. What I mean is that we cannot issue a prosecution without the written

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consent of the Home Secretary, who would go for advice to Dr. Poore.

10416. You did put it so that the Home Secretary could not issue it without the consent in writing of Dr. Poore?—I did not mean to say that.

10417. You mean that the Home Secretary must issue a prosecution, and that he would not issue it without consulting Dr. Poore?—We cannot begin a prosecution without the written consent of the Home Secretary, and the Home Secretary would certainly not give us that consent without asking Dr. Poore, and that is the man who says that we value guinea pigs above babies, and so on. He went on to say: "The money collected by these strange apostles of mercy is not spent in the law courts, where initial cross-examination would be possible, but in attempts to institute a sort of medical excommunication, and in efforts to dam the stream of charity by sophistical utterances such as Tennyson pronounced to be ever the blackest of lies. Meanwhile it behoves the medical profession to bear a wary eye lest fanatics should seek to impose immoral tests upon applicants for hospital appointments, tests, be it remembered, which would have excluded Harvey from the service of St. Bartholomew's. No hospital governor who coquets with tests of this kind can possibly command the respect of our college, nor can we admit that such an attitude towards research can do anything but harm to the cause of the suffering poor. In this country we have always wisely recognised the uses of opposition. The stability of our political organisation has been largely due to the fact that the party in power is subjected to the ruthless criticism of the opposition, the toad ugly and venomous, which wears yet a precious jewel in its head." So it has been in medicine, where scientific advance has been checked not only by the criticism of the learned, but occasionally by the clamour of the prejudiced and ignorant." With regard to that ebullition of Dr. Poore's, which he made immediately after leaving the Home Office as Inspector, the moment he was free of his trammels, it seems to me that a man who made that speech must have exercised the most strenuous efforts to preserve his unbiassed attitude of mind towards anybody who criticised vivisectors. The Royal Commission on Vivisection in 1876, on page 21, said: "The inspectors must be persons of such character and position as to command the confidence of the public no less than that of men of science." I say that Dr. Poore, who made such a speech as that, could not command the respect of the public. I admit that he did not make it until he left the Home Office, but it is obvious that that attitude of mind could not have arisen in a day, and it is probable that he indulged in that sort of expletives when he was inspector. I think, therefore that the Home Office ought not to have retained him, and that he was quite an improper person to deal fairly between the poor animals and the vivisectors.

(After a short adjournment.)

10418. (Chairman.) Will you proceed with your next statement?—I come now to the extreme difficulty of finding out how these certificates are signed, and how long they are effective. I find from information from the Home Office that so far as I can make out, Dr. Poore himself signed certificates exempting their holders from using anæsthetics, and was supposed to inspect the same persons whose certificates he had signed. I venture to think that if these certificates are to be signed at all, and given to these vivisectors exempting them from the use of anæsthetics, it is highly unfortunate that the person who signs them should be the inspector himself.

10419. (Sir Mackenzie Chalmers.) In what capacity did Dr. Poore sign them?—Dr. Poore signed certificates before he was made Inspector, but those very certificates remained in force for some years after he was Inspector, and were effective. I venture to say that it would have been more proper if those gentlemen had applied to somebody else to sign their certificates when Dr. Poore became Inspector.

10420. (Chairman.) They have to be signed every year, have they not?—That, my Lord, is a question which I have found it impossible to ascertain. The Home Secretary in his place in Parliament said on July 24th, 1899, in the House of Commons: "All certificates expire on the 31st of December of the year in which they are granted."

10421. The certificate—not the licence?—Yes. I was speaking of certificates. Under that you would

imagine, of course, that in the year when Dr. Poore was appointed Inspector, at any rate at the conclusion of that year, any certificate signed by him would have expired, and that therefore in the ensuing years the gentlemen who held his certificates would have had to go elsewhere to get fresh ones, but I found that some years after that, it was the fact that certificates were still being used, and held by persons who still held certificates signed by Dr. Poore. I therefore wrote to the Home Office and asked them about it. I stated that in the Yearly Report for 1899 "Dr. Thomas Stevenson is stated to have received a certificate signed by 'the Professor of Medical Jurisprudence University College London.' Both the Medical Directory and Whitaker's Almanack for the current year give the name of Dr. G. V. Poore as the holder of this Professorship."

10422. Dr. Poore was appointed Inspector in 1891?—Yes, he had been inspector ever since 1891, and here in 1899 was a certificate still held, signed by a person called the Professor of Medical Jurisprudence University College, London, when both the Medical Directory and Whitaker's Almanack for the current year, 1899, gave the name of Dr. G. V. Poore as the holder of that Professorship. I therefore drew the attention of the Home Office to it, and after some correspondence, I received the following reply: "I am directed by the Secretary of State to point out that his answer"—that is to the question in the House of Commons to which I have referred—"was that the Inspector whilst holding the office of Inspector under the Act, had not signed certificates. The certificate to which you refer was signed by Dr. Poore, before he held the office of Inspector." So that it amounts to this, that although we are told that all certificates expire on the 31st of December in each year in which they are granted, in 1899 there was a certificate signed by Dr. Poore, according to the Home Office, before he was Inspector, which must be before 1891, still in force.

10423. Running for eight years?—Apparently so, though the certificates are supposed to expire on the 31st December in each year.

10424. (Sir Mackenzie Chalmers.) Is that all the letter?—I was going on. "I am to explain that it is necessary for Dr. Stevenson to hold this certificate in order that he may carry out his professional and official duties in connection with the investigation of poisoning, and it is not limited in time, but is kept in force from year to year by the annual renewal of Dr. Stevenson's licence." So that apparently, although we are told that these certificates expire every year, we are told on the other hand that they are kept alive every year.

10425. Dr. Stevenson is the Home Office analyst in poison cases?—I do not know who he is, I will accept your statement.

10426. (Colonel Lockwood.) He is the man who gives evidence on trials?—Very likely. Therefore we are told in the House of Commons by the Home Secretary that all certificates expire on the 31st December of the year in which they are granted, whereas in the letter to me I am told that they are kept alive from year to year.

10427. (Chairman.) Not "they"?—Kept in force from year to year.

10428. What I understand the Home Secretary to say is that there is an exception made in that case, and possibly in some others, where a person is holding a particular official position and is only doing official work. That is what I should gather from his answer. I should not gather that there is no rule at all that an ordinary certificate expires on the 31st of December?—You must make what deduction you like. I do not make any deduction except that it is a very confused statement. The two statements, one on the floor of the House of Commons, and the other from the Office at Whitehall seem to be contradictory.

10429. It certainly seems to that extent that it would have been more accurate according to his second answer, if the Home Secretary had said, "With the exception of certain officers all certificates expire on the 31st December."

10430. (Sir Mackenzie Chalmers.) You did not get a further question asked in the House to explain it?—I do not know at this moment. I may have or I may not. I will proceed with this letter—there is a little more said: "It is the practice of this Department to make licences annual, terminating on December the 31st in

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each year; and all certificates, whether exhausted or not, cease to operate on that day, because no person can perform a prohibited experiment without a licence. But on the renewal of his licence the holder of a certificate which is not exhausted nor disallowed may proceed with his investigation, as he is not required by the Act to obtain a fresh certificate every year." That, I maintain, is a flat contradiction—"he is not required by the Act to obtain any fresh certificate every year," and in the House of Commons—"All certificates expire on the 31st of December of the year in which they are granted." The two statements are wholly contradictory and irreconcilable. I admit that the fact that Dr. Poore's certificates were kept alive for years after he was Inspector is not exact evidence of his having displayed bias; it is more evidence of the carelessness of the Home Office in not seeing that a man did not hold the two offices and inspect the men who held his own certificates. Before I pass from that I will just rehearse my charge. My charge is that the Home Office have appointed inspectors who have displayed such bias that they have thought it their duty not to make detective efforts to protect animals from illegal treatment.

10431. (Chairman.) Then what is your next charge against the Home Office?—My next charge is that I charge the Home Office officials with having made entirely disingenuous statements in their official utterances, and with having constituted themselves the mere spokesmen of the vivisectors. On page 4 of the Parliamentary Report for the year 1904 the Inspector, speaking of one class of experiments says: "All performed under licence alone and under Certificate C, 1,102 in number, are unattended by pain." I think that is a most improper statement for a Parliamentary Paper, which goes broadcast all over the world, and is taken to be an assertion of fact on the authority of a great officer of the State. As he cannot pretend that he or his assistant was present at the vast majority of these experiments, this statement can be of no more weight than the uncorroborated assertions of the vivisectors themselves, upon which alone it must be founded, and I therefore assert that it would be more becoming for the Inspector to say that these 1,102 experiments were stated by those who performed them to be unattended by pain. That is a proper statement for him to make. If he was an unbiassed official, why should he adopt and endorse the statement of the vivisectors that all these experiments were unattended by pain? It is a remarkable commentary on the sweeping declaration of the entire painlessness of experiments done under licence alone, that on page 77 of the very same Report four vivisectors in Ireland appear to have admitted that pain accompanied their experiments so conducted.

10432. (Sir Mackenzie Chalmers.) Ireland, of course, is not under the Home Office?—No; but are the animals in Ireland more sensitive to pain, or is the Inspector more truthful? If you will pardon my asking the question, I did not mean to put it in the form of a question. That seems, at any rate, a question that should properly have been put to Mr. Thane when he was here. Further down, on page 4, the Inspector commits himself to the following assertion in respect to experiments done under Certificate B, allowing the animals to survive their operations. I have the quotations here if anyone likes to verify them: "Experiments involving the removal of important organs, and even parts of the brain, are performed without causing pain to the animals." If this refers only to the operative procedures under Certificate B it is disingenuous, because we admit, at any rate, that if the law is followed under the operative procedures the anaesthesia ought to be complete.

10433. (Chairman.) Is that the Report for 1904?—It is issued in 1905 for the year 1904. I do not know whether the Inspector expects people who have operations performed on themselves to believe that animals do not suffer as they themselves have suffered after recovering consciousness. I do not know whether that statement is deliberately framed so as to relate in fact only to the period of unconsciousness, while it is intended to convey to the reader the idea that the whole experiment is without pain.

10434. These are all cases of recovery afterwards?—Yes, this is Certificate B.

10435. Would you mind reading the Inspector's statement again?—"Experiments involving the removal of important organs, and even parts of the

brain, are performed without causing pain to the animals."

10436. "And"—would you go on?—I have not got it.

10437. (Sir Mackenzie Chalmers.) "And after the section of a part of the nervous system these degenerative changes are painless"?—That may be so.

10438. (Chairman.) That completes the sentence?—Yes. He does not even commit himself to saying that that is always done. In either case, I characterise this as an *ex parte* defence of vivisection and the vivisectors, and I think it is altogether unbecoming in a Government Inspector. On page 5 the Inspector writes a paragraph on inoculation experiments done under Certificate A, the plain object of which is to suggest to the public that no acute suffering occurs in such experiments. This is what he says: "The substance administered may give rise to poisoning or set up a condition of disease, either of which may lead to a fatal termination. To administer to an animal such a poison as diphtheria toxin, for example, or to induce such a disease as tuberculosis, although it may not be accompanied by acute suffering, is held to be a proceeding 'calculated to give pain,' and therefore experiments of the kind referred to come within the scope of the Act 39 and 40 Vict., c. 77." I call that a disingenuous statement. The Inspector cannot be unaware of the fact that terrible suffering accompanies death by tetanus, induced by inoculation under Certificate A, that very many of the 8,292 inoculations done on behalf of the Cancer Research Fund (I will not use the word Imperial myself in connection with such a thing) entail considerable and prolonged suffering. The animals were given diseases, and they must have entailed considerable and prolonged suffering, and that many of the 29,705 inoculations inflicted lingering and painful disease on the animals used. He must be aware of that. If he really is unaware of these facts, he could have learnt them with a little investigation. If he is aware of them, the paragraph is clearly disingenuous.

10439. Just read the paragraph again—the part that you say is disingenuous?—"The substance administered may give rise to poisoning or set up a condition or disease, either of which may lead to a fatal termination. To administer to an animal such a poison as diphtheria toxin, for example, or to induce such a disease as tuberculosis, although it may not be accompanied by acute suffering, is held to be a proceeding 'calculated to give pain,' and therefore experiments of the kind referred to come within the scope of the Act 39 and 40 Vict., c. 77." I call that a disingenuous statement; that is covering up from the public eye, in a public Report, the fact that a great deal of very severe suffering is caused under Certificate A.

10440. You mean that it suggests that nothing worse, at any rate, can happen in those cases?—Yes, for example, he carefully takes the diphtheria antitoxin and tuberculosis as two cases which are very likely not accompanied by severe suffering, but he must be perfectly well aware that experiments are conducted in tetanus and other things which do cause very grievous suffering, and he ought to say so. He has admitted it further on in the evidence, but this was written before a Royal Commission was going to sit. In either case, whether he is really unaware of the fact that serious suffering follows Certificate A in many cases, or whether the paragraph is disingenuous if he is aware of it—in either case I ask whether an individual who is either ignorant of even elementary knowledge of the conditions of many diseases, or who writes disingenuously in a Government Report, is a proper person to be Inspector under the Act. On page 6 the Inspector says that he and his colleagues found the vivisectors "desirous of acting in every way in accordance with the letter and spirit of the Act." What in the world did the man expect? He need not assure the public that the vivisectors do not break the law when he makes one of his rare visits to the laboratory. The anterior portions of the Report sufficiently indicated the Inspector's own appreciation of the letter and spirit of the Act. And it would indeed be remarkable if the vivisectors found any fault with it. At the head of Table IV. (I am now coming to what the Department says, not the Inspector), and repeated on several pages, the Department makes the following statement:—"N.B.—In experiments performed under licence alone, or under Certificate C, the animal suffers no pain, because it is kept under the influence of an anaesthetic from before

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the beginning of the experiment until it is killed." Now, I say that the Home Office themselves who issued that statement can have no authority for it but the assertion of the vivisectors. They have admitted that their Inspectors are not present at any part of the vast majority of such vivisections, and if the intention of the Act was, and is, that the Home Office officials are to constitute themselves the endorsers of the uncorroborated statements of the vivisectors no Inspectors would be needed or provided by the Act. The Home Office officials themselves simply assume and assert publicly in this statement on no authority but that of the vivisectors themselves that they never infringe the provisions of the Act, and they thereby set themselves in open antagonism to the law, I maintain which, by providing inspection, informs the Home Secretary that he is not to be satisfied by the uncorroborated assertions of the vivisectors. I venture, therefore, to assert that this "N.B." which I have quoted is a biased *ex parte* assertion entirely unbecoming in a Government Report.

10440a. What is the evidence that you suggest the Home Office ought to get? Of course, they ought to have the account for what it is worth of those who actually perform the experiments?—I say that the proper heading would have been not to assert that when those experiments were performed the animal suffered no pain, but that the vivisectors informed them that in those experiments the animals suffered no pain. Then the public could take that assertion for what it is worth.

10441. Apparently their adviser, Dr. Poore, or whoever it was at that time, advised them that it was so?—Dr. Poore was no more there than you or I.

10442. So far as his evidence goes, he is the Inspector?—Yes, he is the Inspector.

10443. And so far as the value of it goes it was in the same direction apparently?—Whether a man be an Inspector or a policeman, or anything else, he cannot know what happens in places where he is not.

10444. Neither can the Home Secretary?—That is my objection.

10445. He does get the evidence of the people who were on the spot?—He gets the evidence of the vivisectors themselves.

10446. They were the people on the spot?—I should not have the least objection to their putting a heading "N.B.—In experiments performed under licence alone or under Certificate C, if the law is properly observed, the animal suffers no pain," or "The vivisectors inform us that the animals suffered no pain," but they say "The animals suffered no pain." They were not there. They have no right to say that, and that goes to the public as an authoritative statement.

10447. (Sir Mackenzie Chalmers.) They see a certain number of experiments, and that is a sample, is it not?—They saw one entire experiment in one year, I think—one physiological experiment throughout.

10448. I think they saw a good many?—I have an answer on that very point in the House of Commons.

10449. I want that point cleared up?—If is a vanishing point; what they see is absolutely a vanishing point. I do not blame them for that; they cannot be there unless the law makes it necessary. I will come back to that. I know I have got it. It is something very small, you may take it from me, and I can prove it. I want to draw attention to Question 206 on page 10 of the Minutes. This case was put to Mr. Byrne:—"To give an example, the other day an experimenter wished to have an experiment without anaesthetics in this case. He wished to feed kittens on cow's milk instead of cat's, and a certificate was granted that the experiment might be performed without anaesthetics," and the answer was "Such an application would be granted by the Home Office." My comment upon that is, is feeding a cat with ordinary cow's milk an experiment calculated to give pain? I regard that simply, if I may say so, as flouting those who hold the views that I do. That question and answer seem to me to be a sort of ebullition of a spirit of raillery. No anti-viviselector that I ever heard of would suggest that giving a kitten cow's milk was an act calculated to cause it pain and the necessity of going to the Home Office to licence such a thing. It is an attempt to pour ridicule upon the whole of those who with me wish to stop grievous pain to animals. You do not suppose that I, or any of those whom I represent, would support anything so absurd as that.

10450. On that point the experimenter would have

been liable to prosecution if he had not a licence?—Why?

10451. Because it is an experiment, and it is an experiment which may cause disease in the animal?—How could the experiment of giving cow's milk to a kitten cause pain? There is nothing in the Act to prevent your trying which of two dogs will first find a bun under a sofa.

10452. It is a feeding experiment, and the licensee applied to the Home Office for a licence, because the result of changing the food in that way might have been to cause disease, and he wanted to find that out?—I pass from that.

10453. It was considered, and the licence was granted?—I do not deny it.

10454. (Chairman.) I do not say that it is likely to do it, and it sounds to me very unnecessary, but supposing that the kitten had died from the change of diet, I suppose a complaint might have been made?—My Lord, those whom I represent are not that sort of people. I protest against that being given as serious evidence—that is all. Then at Question 231 (this is very important, I think) Mr. Byrne was asked, "Has any application to be allowed to prosecute a licensed person ever been made and refused?" "No," says Mr. Byrne, "no application has ever been made and refused. An offer to give the Secretary of State's consent was once made." We all see what that is meant to convey, that we have no case, and cannot go to the Court. My answer is, of course, that we cannot make an application. How am I, or any one of the public, to obtain evidence of breaches of the law in cases which are not public? We can get no evidence of what goes on in laboratories except from the printed and published admissions of those who do the acts, and a man who has done an illegal thing is not likely to publish it abroad. They sometimes do, but if they do it is generally six months afterwards, so that I think this innuendo here, that we have made no complaints because there are no breaches of the law, is a very improper one on the part of the Home Office. I should like them to explain to us how we are to prosecute, however badly the Act is carried out! The next question that I want to refer to is Question 248, "Is there no red tape there?" and the answer is, "No, it is a necessity under the Act; the Secretary of State is bound to act in that way. It is curious that of the 60 contraventions, quite a substantial number were contraventions indicating a keen desire on the part of the operator to provide greater security for the animal not suffering than the Act allows him to take." The suggestion there of Mr. Byrne is that the Act of Parliament forces a sort of cruelty apparently upon the vivisectors; that if they were to follow the Act they would have to be cruel. "If he has a certificate, for instance, authorising him to carry out a certain investigation" (this is the instance he gives, you see) "without anaesthetics, and he, in an individual case, thinking the anaesthetic will do no harm, applies it to save the animal pain, then he has committed a breach of the Act. The Secretary of State cannot help that at present under the existing law." I venture to submit to Sir Mackenzie Chalmers and the Commission that this is not so. He could do the experiment under licence alone, which does not compel him to kill the animal unless it is in pain after the anaesthetic has passed off, or it has been seriously injured. And therefore the whole of that answer, I think, is most unhappy and unfortunate.

10455. (Sir Mackenzie Chalmers.) That refers to Certificate A, does it not?—Certificate A permits him to omit anaesthesia; it does not forbid him to operate under his licence alone.

10456. He wants the animal to recover?—So it may under his licence alone.

10457. No?—Yes, pardon me.

10458. Take the case of an inoculation experiment—that is what we are really thinking of?—That is quite right. What Mr. Byrne here says, is that if he has Certificate A he is precluded from using anaesthetics while he injects whatever it is that he injects. I say that is not so; he can inject whatever it is he injects under his licence alone; because it says that the animal must, during the whole of the experiment, be under the influence of some anaesthetic sufficiently powerful to prevent its feeling pain; and then it says that the animal must, if the pain is likely to continue after the effect of the anaesthetic has ceased, or if any serious injury has been inflicted upon the animal, be

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killed before it recovers. But in an inoculation of this kind neither has any serious injury been inflicted upon it nor is pain likely to continue after the effect of the anæsthetic has passed off. Therefore there is nothing whatever in the Act to prevent a man using his licence for an inoculation experiment.

10459. (*Chairman.*) Is not this rather on the question of the amendment of the Act, which I understand you are going to deal with afterwards. This is hardly a charge against the Home Office. There seems to be a point which you do not agree with as to the meaning of the Act?—The point I am taking is that in his evidence, Mr. Byrne would have the public believe that the Act of Parliament forces upon him a necessity for inflicting pain. That is what I object to.

10460. I do not see that?—He says, "It is curious that of the 60 contraventions quite a substantial number were contraventions indicating a keen desire on the part of the operator to provide greater security for the animal not suffering than the Act allows him to take." I say that is not so.

10461. Giving anæsthetics when the Act did not really compel him to do it?—No, giving anæsthetics when the Act says that he must not, is what Mr. Byrne says. I say that the Act does not say that he must not; I say that the Act does not put that compulsion upon him.

10462. I cannot see how that is a charge against the Home Office?—

10463. (*Dr. Gaskell.*) The contravention of the Act was, that they did not take out Certificate B as well; that is what the charge says?—I am speaking of the licence alone.

10464. (*Sir Mackenzie Chalmers.*) But you could not use the licence alone for inoculating an animal for the purpose of producing disease. If you aim at producing a disease, say cancer, under that you require Certificate A; the licence would not do it?—Why not?

10465. Because you are going to produce a serious lesion in the animal by producing cancer?—Not if the original operation is painless, a mere prick of a needle.

10466. You require Certificate A for that?—If you do not administer anæsthetics; but you may administer anæsthetics, surely.

10467. But you are bound to kill the animal before it recovers?—No.

10468. (*Chairman.*) Is not this a thing to be discussed, if discussed at all, on the question of amendment of the Act. If the Act is not clear about it you think it should be made clear. I do not see how you can found a charge against the Home Office for taking a particular view of the Act which, after all, is not an inhuman view?—My objection to it is that, as it seemed to me, Mr. Byrne's evidence was rather conceived in a spirit of railery against us.

10469. No, I remember his giving evidence upon that point?—Then I pass from that. Now in Hansard of the 30th May, 1902, there is an important statement by Mr. Ritchie—it must have been part of a speech: "With regard to vivisection, time did not admit of fully discussing the subject. As to the argument that animals suffered intense pain after recovering from the anæsthetic, he could assure the House that that was very easily overstated, because life was destroyed before return of consciousness in all cases, except those where special certificates authorising the keeping alive of the animal were granted. It might become necessary to appoint additional inspectors, but it would be absolutely impossible to ensure that an inspector should be present at all operations, for that would require an army of inspectors. No certificate was given except under the greatest precautions, and even where such certificates were granted, the animals had to be killed immediately the object of the experiment was attained." I wish the Commissioners to fully appreciate those words: "Immediately the object of the experiment was attained." "Before the certificate was issued the Home Secretary had to be convinced that every precaution would be taken that the operation would be conducted by a properly-trained person for an adequate scientific object. All possible means were taken to avoid causing animals unnecessary pain, the inspectors were most humane men, and so, generally, were the operators. He did not know that there had been any convictions under the Act." I must take those words: "And so generally were the operators," to be an admin-

sion by the Home Secretary that not in all cases were the operators humane men.

10470. I should think that was rather a forced construction of a Minister speaking in the House. I dare say he put in the word "generally" thinking, "I am not going to commit myself to the statement that there is no such thing as a hard-hearted operator"?—I just draw the attention of the Commission to that, that is all. I pass from that. And as regards the words "The animals had to be killed immediately the object of the experiment were attained," I have had considerable correspondence with the Home Office about the proceedings of Professor Starling with the brown dog, over which I had to pay damages in the Law Courts. I may point out that whatever Professor Starling did to the dog was not within the purview of the Law Courts on that occasion. The action was brought by Mr. Bayliss solely, and everything that happened before the dog reached Mr. Bayliss's hands was outside the purview of that action. But I have to point out (and I have pointed it out to the Home Office) that I have asked the Home Office repeatedly to tell me under what licence and certificate the first two operations by Professor Starling upon the brown dog were done. I have been wholly unable to get an answer from the Home Office to that very plain, simple question. I wish the Commission fully to understand the point. My point is this. Professor Starling, if he had Certificate B, performed two operations upon that dog, one in December, a long interval of time between, and then another operation in February, to ascertain whether inflammation had been set up in the intestinal, or, at all events, the internal parts of that dog, by the first operation. If he did that second operation under his Certificate B, it was his duty, according to the Home Secretary's own words and according to the Act of Parliament, to kill that animal "immediately the object of the experiment was attained." When he had opened that dog a second time, he had by that opening fulfilled the object of his experiment, which was to ascertain, in his own words in the witness-box, whether the condition of inflammation had been set up by the previous operation. Instead of immediately killing that dog, he handed it over to Mr. Bayliss, and left the building himself, and Mr. Bayliss performed a further major operation upon that dog, which took an hour. My contention is (and I have placed it before the Home Office with all the seriousness that I can) that in that case it was Professor Starling's duty to kill that dog the moment he had performed his second operation upon it, and that this handing of the dog round from one vivisector to another is utterly illegal and ought certainly to have been stopped by the Home Office if they knew of it. If they did not know of it, the evidence in the action brought against me apprised them of it. From the moment that that evidence was before the public, they knew that Professor Starling instead of killing that dog, handed it over to Mr. Bayliss. My contention is that there never was a case more clear of a breach of the Act. And if they say that the second operation performed by Professor Starling was not done under Certificate B, but done under his licence alone, then it was also his duty immediately to kill it on recovery from the anæsthesia. He had no business to leave it to anybody else. So that whatever view they have of it, Professor Starling must in my opinion, have broken the law.

10471. You have read Professor Starling's account of it, have you not?—I heard his account given in his sworn evidence in the witness-box.

10472. And also his account before us?—Yes, I believe he was examined.

10473. (*Sir Mackenzie Chalmers.*) Do you accept his account?—I do not know at the moment that I remember it. I am going much more upon the evidence given in the Law Courts and the subsequent action of the Home Office. The Home Office, at any rate in the long interval between that action against me and this Commission, did not know what Professor Starling was going to say to the Commission, and they did know what he said in the Law Courts, and upon what he said in the Law Courts they ought to have acted, in my opinion.

10474. (*Chairman.*) I am not sure whether I remember rightly myself—I have not referred to the evidence, and it was some time ago—but was it not a fact that the dog was under anæsthesia at the time that it was handed over to Dr. Bayliss?—That was a matter of dispute. There was sworn evidence on both sides.

But I am not dealing with the question of anæsthesia; I am dealing with the question of Professor Starling's duty to destroy that dog.

10475. I am not myself arguing the question whether it was a breach of the law or not; I am only trying to get at what the explanation was; I am not saying what the worth of his explanation was, but I understood his explanation to be that the animal was under anæsthetics; and instead of that dog being killed under anæsthetics, and a new dog being obtained and put under anæsthetics and operated upon by Mr. Bayliss, he allowed this dog to be operated upon. It had to be destroyed, and he thought that as it had to be destroyed without coming out of the anæsthesia, it was saving, as it were, another dog for Dr. Bayliss to make his experiment on that same dog. Whether that was a breach of the Act I am not saying—I dare say it was, but that was the explanation that he gave.

10476. (*Colonel Lockwood.*) Did not Mr. Akers Douglas give me the same answer about that very question in the House of Commons?—Yes, I think he did. My point is that the animal under Certificate B had a right to be killed the moment Professor Starling had performed that second operation.

10477. (*Chairman.*) I quite see the point?—Professor Starling handed it over to somebody else; he did not even stay to see whether the animal remained under anæsthesia. It is his duty to see; it is not his business to hand it over to anybody else. To pass dogs round from hand to hand and nobody is responsible: to wash his hands of it and give it to somebody else is not, in my opinion, the way in which this Act should be carried out. I also wish to point out that if it had not been for this action the veil would not have been lifted in this case; we should not have known that the animal was handed from one vivisector to another, and passed round with wounds clamped up in its side with iron pincers. And this may happen constantly. The inspectors have told me that they knew nothing about the case until the action was brought. I have been told by the Home Office that they knew nothing about it. Now I come to the latter part of this charge, which is, that the Home Office have constituted themselves the mere spokesmen of the vivisectors. The position of mere spokesmen of the vivisectors has never been so openly exposed as in their own Reports. In the Report issued for 1879 when Mr. Busk was the Inspector, this is what he said: "The number of experiments performed, and according to the returns I have received from each licensee, was about 481." First of all he says "about," which is a very ingenuous remark to make; he does not know the number of experiments, and even the "about" is simply based upon the returns which he has received from each licensee, so that one licensee may have performed a thousand experiments, and there is nothing to prevent his reporting it as one, two, five or six. It is a very remarkable thing that year after year a large number of licensees take the trouble to take out licences and certificates, and, according to their own account, never use them. I do not assert anything, but it is a matter of comment. If a man takes out a licence to drive a cab, you generally assume that he drives a cab; and if a man takes out a licence or certificate to enable him to vivisect animals without anæsthetics, it is somewhat strange if he does not use them. And he takes considerable trouble to get them, if they are to be renewed every year, according to one statement of the Home Secretary. In the Report for 1880 I find these words—still Mr. Busk: "In none of the other experiments under these certificates, as I am assured by the experimenters, was any appreciable suffering inflicted." That is the statement put forward, and that is a perfectly right and proper statement to put forward. He says: "As I am informed by the experimenters." If the Home Office always confined themselves to saying in their Reports that they were only assured by the experimenters that this and that did not happen it would be more fair to the public. But still, as I have said, they remain simply here the spokesmen of the vivisectors—that is all. In 1883 he says: "I have been assured by all the experimenters, that in this class of cases no appreciable pain was manifested." There again he is nothing but the spokesman of the experimenters. Then in 1883, further on, he says: "In none of these experiments, as I am informed by the experimenters, was any appreciable pain inflicted." In 1884: "No appreciable suffering would be caused if the provisions of the Act were faithfully carried

out, which I have every reason to believe was done." Well, down to the word "carried out" that is a very proper statement. "Which I have every reason to believe was done," is a statement that we will take for what it is worth. He was not there, and I do not see how he can say. He may have every reason to believe. I suppose that is a harmless statement; it does not carry much weight. This is the next Report, still 1884: "From the returns I have received from the various operators, it would appear that no pain was inflicted in these cases except in about 14 or 15 instances, in which disease was produced, but which was very trifling." There again he is merely their conduit pipe. Then the Report for 1885: "No appreciable suffering would be caused if the provisions of the Act are faithfully carried out, as there is not the least reason to doubt they were."

10478. You are not objecting to any of these statements that you have read?—I am only saying that throughout their Reports, and on all occasions, the Home Office constitute themselves the mere spokesmen of the vivisectors. They incorporate in their Reports just exactly what the vivisectors tell them. I say, and say it frankly, that in these Reports they say the vivisectors tell us they do not inflict any pain.

10479. That is rather a different thing from being mere spokesmen?—It comes to that. They do not criticise.

10480. When we speak of a man being the mere spokesman of somebody else, we mean that he is put forward to present that person's views in his interest. This is merely saying "I have merely to report information I have received, and this is the information I have received"?—I do not mean to convey any more than that, except that in their Reports, owing to their being wholly unable under the present Act to be present at vivisections that take place, they constitute themselves merely the spokesmen.

10481. We quite understand now what you mean by the word "spokesman." But we had better come to something about which you do complain of the Home Office?—I do not want to carry it any further than that. Now, in the "British Medical Journal" of the 18th June, 1898, Dr. Cecil Shaw, at page 1582, says: "My own work has consisted of two proceedings—first, the pathological examination of eyes removed on account of injury; and, secondly, an experimental research to determine what changes, if any, follow prolonged irritation of one eye, both with and without traumatism." He subsequently continues the narrative as follows: "In the experimental research my aim was to discover"—I am emphasising the "my," because he subsequently said that he never did any of these things himself; he says, "My own work," and he goes on subsequently "My aim was to discover whether prolonged irritation, external or internal, of a rabbit's eye would produce any visible organic changes in the ciliary region, either in the irritated eye or in the other. In the first experiment the eye was cocaineized, and a wound was made in the ciliary region, without antiseptic precautions, and the cornea irritated with powdered jequirity seeds for varying periods up to six months. The jequirity causes purulent conjunctivitis." (I do not understand all these words) "and abundant cocci were present in the pus, but though in some cases the ciliary region was again wounded while the eye was bathed in pus, no iritis was caused. In one case there was an extensive ulcer of the cornea, which ultimately healed without perforating. The non-irritated eyes were examined at intervals with the ophthalmoscope, but no departure from the normal appearance of the disc was seen in any case. Internal irritation was then tried, small shot being inserted into the eye; again without antiseptic precautions. In fact, soiled instruments were purposely used, to reproduce as far as possible the kind of wound which is most often found to cause sympathetic ophthalmia. When the incision was made in the sclerotic a few millimetres above the corneal margin, a little vitreous escaped on the shot being put in, but the wound healed rapidly, and in a few days no sign of it, or of external irritation, could be seen. When the wound was made in the ciliary region, more external irritation was seen. In one case the wound opened after several days, the shot was expelled, and all signs of irritation disappeared. In another, the shot was similarly expelled, but after some weeks, and irritation is still present at the end of five months, as shown by injection in the neighbourhood of the wound,

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but no signs of iritis are to be seen, either in the traumatised eye or in the other. This rabbit I have kept alive to observe how long the irritation lasts, and whether it will yet lead to any changes in the iris or ciliary body. After periods varying from three to four months, the rabbits were killed."

10482. Where was that performed?—In Dublin, I think.

10483. (Dr. Wilson.) In Belfast, I think.

10484. (Chairman.) In the United Kingdom, at any rate?—Yes.

10485. (Sir Mackenzie Chalmers.) That had nothing to do with the Home Office?—I have not said so yet.

10486. You were dealing, I thought, with your charges against the Home Office?—I identify —

10487. The Irish Office with the Home Office?—Oh, yes.

10488. Not for all purposes, I hope?—I look at this from the point of view of the animals, not of the officials.

10489. I understand?—On inquiry I found that the alleged author of these experiments had never held a licence under the Act, much less the certificate which would have been necessary for such experiments as those above described. It looked, therefore, very much as if there had been a breach of the law. Special inquiries were made on the subject, and the society's solicitor went to Belfast to continue those inquiries on the spot. It was found impossible, however, to fix the exact date of the experiments, which was necessary as a preliminary to bringing the matter before any court. That shows the difficulty we have about bringing law actions. The inquiries were, nevertheless, persisted in for some time, but unfortunately the proceedings of vivisection under the present law are carried on in such secrecy that it was found impossible to obtain the necessary evidence from any independent source. As a last but not very hopeful resource it was determined to lay the facts before the Irish Government. The correspondence which followed shows what happened.

10490. (Chairman.) When was this?—This was in 1898. I got a letter from J. B. Dougherty, dated from the Chief Secretary's Office, Dublin, and he encloses the correspondence, and here is the correspondence. It is a letter from Dr. Shaw to his very good friend Dr. Lorrain Smith, who had a licence, and a letter from Dr. Lorrain Smith to his very good friend Dr. Shaw, who had not. The letter from Dr. Lorrain Smith, who had a licence to his friend Dr. Shaw who had not, is as follows; both of them are dated the same day; they must have met in the street and exchanged them, I daresay: "Sir,—On receiving your note of yesterday's date, I referred the matter to Dr. Shaw, and I beg to enclose his reply. I regret that his paper is so worded as to be possibly misleading in regard to the vivisection experiments on inflammation of the eye which I allowed him to use. These experiments are performed by me" (this is the man with the licence) "in investigating the process of inflammation, and for them I hold the necessary certificate. I may further state that I reported the experiments in question to the Chief Secretary at the end of 1897 amongst other similar experiments on the process of inflammation. The experiments in which I as director of the laboratory aided Dr. Shaw were not on living animals." Now Dr. Shaw writes to his friend Dr. Lorrain Smith: "Dear Dr. Lorrain Smith,—I regret that any ambiguity in expression in my paper on sympathetic inflammation in the eye should have given rise to a misunderstanding. Your aid and advice were given in the whole course of the research, which included many bacteriological and other experiments, besides those involving vivisection. These latter, very few in number, were, I understand, part of the series of your own experiments on inflammation, of which you allowed me to make use. I had no responsibility beyond taking notes, and undertaking subsequent histological examination of the tissues.—Very sincerely yours, Cecil E. Shaw." So that Mr. Cecil Shaw, who in the papers said "my own work consisted of this, that, and the other," and "in the experimental research my aim was to discover"; and "this rabbit I have kept alive to observe," and so forth, when asked by the authority to explain how he did this says: "Oh, it was not I; it was my friend Mr. Lorrain Smith. I am sorry if there is any ambiguity of expression in my papers." I can see no ambiguity of expression. It is, "this rabbit I have kept alive," not "my friend Mr. Lorrain Smith."

10491. Is it now your suggestion that those two gentlemen were saying what is not true, and that Dr. Cecil Shaw did these experiments?—That is my suggestion. He said so himself in the paper which he published, and I prefer his statement written by himself before he was hauled over the coals to his refusal to say so afterwards.

10492. (Mr. Ram.) By the statement he made himself, do you mean what you have just read to us?—Yes, in his paper. "My own work consisted of two proceedings," and "in the experimental research my aim was to discover."

10493. I did not know whether there was anything else. I was not sure whether you alluded to that or any other paper?—No, I have nothing else to go upon but that paper.

10494. (Chairman.) Do you really think that that ambiguity — ?—I see none.

10495. Well, what appears to you to be an incorrect statement but to other persons apparently an ambiguity?—When you say other persons, the person who called it ambiguous is Mr. Cecil Shaw himself.

10496. When I heard the explanation it is quite capable, to my mind, of being an ambiguous expression. If there had not been any other evidence I should have thought he was speaking of his own work. I should not have known that there was another operator; but I can quite understand if there was another operator how Dr. Shaw should have written that; and that being so, it seemed to me that we require rather more evidence, if we are asked to say that these two gentlemen have concocted a lie, than a mere suggestion that those words are a distinct and express admission by Dr. Cecil Shaw that it was his hand that operated, and not Dr. Lorrain Smith's?—Very well, then, I will supply you with that extra evidence that you want. New light was thrown upon what I consider the somewhat discreditable proceedings in the "British Medical Journal" of 22nd July last, on page 220, of which you will read in the report of the Scientific Grants Committee for the past year the following: "SHAW, C.E., M.A., M.D., 14, College Square, East, Belfast.—For a research on sympathetic ophthalmia, necessitating the purchase of monkeys and dogs and apparatus, £12." I call that corroborative evidence that he, Dr. Cecil Shaw, is the person who did those experiments, because he was given money to do them, and to purchase monkeys, dogs, and apparatus for the purpose.

10497. I understand your point about it?—And, further down—I have not quite finished yet—the editor of the "British Medical Journal" himself does not attempt to conceal the truth as I hold it about Dr. Cecil Shaw, because he says on page 224: "Dr. Cecil Shaw has, owing to circumstances, had to abandon for the present the research for which he received the grant." Putting all that evidence together, I maintain that that is sufficient evidence for reasonable people that Dr. Cecil Shaw did those experiments himself, and that when the Home Office, represented by the Irish Office over there —

10498. (Sir Mackenzie Chalmers.) Oh, no, no, the Irish Office does not represent the Home Office?—Whatever it is—whatever is the Home Office over there in Ireland—

10499. (Chairman.) Supposing that Dr. Cecil Shaw had undertaken a research of this kind, and thought it was one that would yield useful results, supposing he had not got a licence, and got his friend, who had a licence and a laboratory, to do the manual work that was necessary, would not everything then be consistent? Would not this passage about the grant and everything else all be consistent?—No, I venture to think not. I think if a man gets a grant to do a thing himself—

10500. It does not say himself; it says his research. It does not say that his hand is to be the hand to operate upon those rabbits?—But he says himself, "My own work."

10501. We have gone through all that, and I have said what the effect is upon my mind?—I must beg leave to say that the effect on my mind is very different—that is all.

10502. So I understand.

10503. (Mr. Ram.) Your charge here is the illegality of any operation of this sort being performed by this particular man?—My charge is a little more than that. My charge is that when a person performs what is, in my opinion, a totally illegal act, and a knowledge of

that fact is brought by me to the officials who are his superior officers, instead of listening to me and saying to this man: "You have no business to do this," they always adopt his explanation, and throw it back at me, and say that is the explanation.

10504. (*Chairman.*) What they did was they accepted the statement of Dr. Cecil Shaw and Dr. Lorrain Smith?—Yes, at once. They had not at that moment, of course, before them the statement about the grant to Dr. Shaw himself for doing the very thing, because I had not got it to lay before them at that moment; otherwise it possibly might have led to a little difference. The answer of the Home Office was this: "With reference to previous correspondence relative to alleged violations of the Act 39 and 40 Victoria, chapter 77, by Dr. Cecil Shaw, of Belfast, I am directed by the Chief Secretary to transmit to you the accompanying copies of letters from Dr. Lorrain Smith and Dr. Shaw" (those are the two I have read). "I am to add that, having regard to the contents of these letters the Irish Government cannot carry the matter further." My complaint is that they immediately accept what I call the absurd explanation of these two vivisectors.

10505. (*Sir John McFadyen.*) Might we ask what it is suggested that the authority ought to have done under the circumstances?—They ought to have prosecuted Dr. Shaw for performing a very cruel experiment upon these rabbits without any licence at all.

10506. (*Chairman.*) The specific evidence against him would have been that statement, "My own work"?—They could get evidence that I, as an outsider, could not. They could have got the laboratory attendants and everybody in connection with that laboratory, and got to the bottom of it; instead of which they accept immediately, without any further talk, these two, as I call them, ridiculous letters of these two gentlemen.

10507. (*Dr. Gaskell.*) Is not the case of Ferrier and Yeo a very parallel case?—Yes.

10508. Was not that brought by you to the Law Courts?—It was before my time. I think it was brought by the Royal Society for the Prevention of Cruelty to Animals. I can find out all about it for you.

10509. Did not the Law Courts say that the evidence was distinctly that although Ferrier used the word "I," and spoke of them as his experiments, and got the money for the very purpose, the experiment was really done by Yeo?—I dare say.

10510. It was a parallel case?—I do not know.

10511. (*Mr. Ram.*) Was that before a jury?—I think it was before the magistrate; I do not think there was a jury upon it at all.

10512. (*Colonel Lockwood.*) Is that the end of your charges against the Home Office?—Oh, dear me, no.

10513. (*Sir Mackenzie Chalmers.*) We have been divagating to the Irish Office, and now we are coming back to the Home Office?—I did not really recognise much distinction. The general effect, I only wanted to say, of Dr. Cecil Shaw's case, in my mind, and I hope in the mind of yourself and others, is that—I do not say the Home Office in this case—the Irish Office did on that occasion certainly constitute themselves practically the representatives of the vivisectors—the spokesmen of the vivisectors.

10514. (*Chairman.*) Then what is your next charge?—I am here to charge the Home Office officials with having accepted the suggestion made to them by some nameless adviser, that to starve animals for days is not cruel, when they must perfectly well know that if any one of them starved their own horses (if they have any) the nearest policeman could successfully prosecute them for cruelty to animals under Martin's Act. That is the next charge I make. It is based upon this evidence. The "Journal of Physiology," volume xix., page 193, contains an account of great cruelty inflicted on animals by Dr. Noel Paton. On page 193 he tells us he starved a kitten for 56½ hours; on page 194 he tells us that he starved two pigeons for 96 hours; on page 199 we read: "Experiment 38. Three young rabbits of a brood, procured on June 7th, and starved to June 9th." I asked the Home Office whether the certificate under which these cruelties were perpetrated was Certificate A, and I asked what were the names of the professors who signed that certificate; and I further asked whether, in the opinion of the Home Secretary, starving animals for days together is correctly described as a "trivial operation," because

Certificate A, we have been told, is only issued for trivial operations. The answer was that they had communicated with Dr. Noel Paton, and that they had ascertained from that gentleman that the experiments were not performed under any certificate specified in the Act.

10515. Did Dr. Noel Paton say what the object of the experiment was?—I do not know what his object was. I take no more interest in the object than the poor kittens did.

10516. (*Sir Mackenzie Chalmers.*) You got the information from the "Journal of Physiology"?—Yes.

10517. Did not it state any object?—I dare say it did. I take no interest in the object.

10518. (*Colonel Lockwood.*) "On the Relationship of the Liver to Fat," by Noel Paton?—Yes; I am not a doctor; I take no interest in the relation of the liver to fat; I do take an interest in pain inflicted on animals. It is perfectly indifferent to me what his object was.

10519. (*Chairman.*) You are aware that we are also discussing the question of breaches of the Act?—Yes, but the Act does not say what the intention was.

10520. But it says "With a view"?—Yes, that I do not know.

10521. (*Mr. Ram.*) You said that you asked the Home Office three questions. What was the answer to the first?—The answer was that the gentleman said that the experiments were not performed under any certificate.

10522. Then the second one?—The second one falls with it; it was to ask who the people were who signed the certificate. That falls with it. The third was whether starving animals was considered a trivial operation.

10523. Was there any answer to that?—The Home Office Secretary does not commit himself to that; he would not answer it. I will give you the whole letter: "Sir,—With reference to your letter of the 9th ultimo, I am directed by the Secretary of State to acquaint you that he has made inquiry as to the experiments to which you refer as having been performed by Dr. Noel Paton, an account of which is contained in the 'Journal of Physiology' of 1895-6. He has ascertained from that gentleman that they were not performed under any certificate specified in the Act 39 and 40 Vict., cap. 77." That is the whole letter. They will not tell me whether they consider it a painful operation or not.

10524. (*Sir Mackenzie Chalmers.*) If the experiments were cruel they would come under the general law of cruelty to animals?—No, not if they are experiments.

10525. Why not? Surely if a man has no licence it comes under the general law of cruelty to animals?—Yes, of course, if he is not licensed.

10526. Here he had no licence, apparently?—I did not say that; I said he had no certificate. I think he had a licence. I did not make that point. I assumed, of course, that he was a licensee, and I feel sure that he was. If he was a licensee and was doing painful experiments, I say he comes under the Act. I think he is a licensee. If he is not, I should have been sure to ask the Home Secretary how it was that he did these experiments without a licence.

10527. (*Mr. Ram.*) Unless he had a licence there was no good in going to the Home Secretary?—Exactly; I feel sure that he had. I feel sure that I should have asked that.

10528. (*Sir Mackenzie Chalmers.*) Dr. Gaskell thinks he had a licence?—I deduce it from my letter. The point is, here is a licensee who does a peculiarly, in my opinion, painful experiment. If he does it under his licence alone, he is bound to see that the animal during the whole of the experiment is under the influence of an anæsthetic sufficiently powerful to prevent its feeling pain. You cannot keep an animal for 56 hours, I presume, under chloroform?

10529. If he does it outside his licence altogether he comes under the general law of cruelty to animals, surely?—I hardly think so.

10530. He did not purport to do it under his licence at all?—Until he informs you that he considers the experiment outside his licence the natural assumption is that it is inside his licence. It is an experiment

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that comes under the Act, and the Act deals with experiments on animals. He does not make the defence that he is doing anything else; it is not like a carter driving a horse with collar sores; it is an experiment. If this does not come under the Act, nothing does. I next asked the Home Office the following questions: "(1) Were the kittens that he starved for 56½ hours, the two pigeons that he starved for 96 hours, and the three young rabbits starved from the 7th to the 9th of June kept under the influence of anæsthesia during those hours and days mentioned respectively, and if so, what was the anæsthetic employed? (2) If these animals were not so kept under the influence of anæsthesia, and, consequently, if the law was broken, was a prosecution instituted by the Home Secretary against Dr. Paton; and if not, what course, if any, was adopted by the Home Secretary to punish Dr. Paton for breaking the law, and to secure his future obedience to it? (3) Whether Dr. Paton holds a licence and certificates at the present time, and if so, who signed his certificates and his recommendation for a licence?" I did ask the question, you see, in that letter. (*After referring.*) Yes, here he is; he is a licensee, and he had a licence at that time. The answer was: "Sir,—With reference to your further letter of the 5th inst., regarding certain experiments described in Volume XIX. of the "Journal of Physiology," I am directed by the Secretary of State to say that he is informed that Dr. Noel Paton, after careful consideration, did not deem these experiments to be within the scope of the Act 39 and 40 Vict., c. 77. The Secretary of State is clearly of opinion that experiments involving starvation to an extent calculated to cause pain would be within the Act; but the inquiries he has made point to the conclusion that the extent and degree of pain which animals suffer from deprivation of food is a matter of considerable doubt. In any case, Sir Matthew Ridley does not propose to institute proceedings in respect of experiments which were conducted so long ago as 1895-6 and not brought to his notice at the time. Dr. Paton holds under the Act a licence and certificates, particulars of which will be found in the Annual Return of Experiments upon Living Animals for the year 1899." My reply was: "Sir,—With reference to your letter of the 30th of July, 1900, in which you inform me that in Dr. Noel Paton's opinion his experiments were not within the scope of the Act 38 and 40 Vict., c. 77, what I desire respectfully to inquire is, not what Dr. Noel Paton's opinions may be of his own experiments, but whether, in the opinion of the Home Secretary, the starving of animals, described by Dr. Paton, were experiments upon living animals within the meaning of the Act." I cannot get an answer.

(*Mr. Ram.*) I thought he had answered that.

10531. (*Colonel Lockwood.*) He evaded it rather?—Yes, and he goes on evading it: "Sir,—With reference to your letter of the 27th ultimo, inquiring whether the Secretary of State considers the starving of animals described by Dr. Paton were 'experiments' within the meaning of the Act, 39 and 40 Vict., cap. 77, I am directed by Sir Matthew Ridley to inform you that, for the reasons given in the letter from this Department of the 30th ult., he decided not to cause proceedings to be taken against Dr. Paton. He therefore deems it unnecessary to express an opinion in the case of Dr. Paton whether or no his contention was correct, and whether or no what he did was an experiment upon living animals, such as to come within the provisions of the Vivisection Act."

10532. (*Mr. Ram.*) Would you read the first letter of the Secretary of State again in answer to your first letter on the subject?—"Sir,—With reference to your letter of the 9th ultimo, I am directed by the Secretary of State to acquaint you that he has made inquiry as to the experiments to which you refer as having been performed by Dr. Noel Paton, an account of which is contained in the 'Journal of Physiology' of 1895-6. He has ascertained from that gentleman that they were not performed under any certificate specified in the Act."

10533. Yes?—That is all, except "Your obedient servant, Henry Cunynghame."

10534. I thought there was some sentence that you read before that the Home Secretary was of opinion that these things did come within the Act?—No, that is the only letter. He rehearses what the other man said. Perhaps you are thinking of this: "I am directed by the Secretary of State to say that he is informed that Dr. Noel Paton, after careful considera-

tion, did not deem these experiments to be within the scope of the Act." "The Secretary of State is clearly of opinion that experiments involving starvation to an extent calculated to cause pain would be within the Act."

10535. There is a distinct answer. I thought he had given one?—Yes, but he did not tell me whether these were such experiments.

10536. That is a direct answer to the question you asked?—No; I wanted to know what these experiments were. I do not want a general statement that there may be starvation, no doubt.

10537. Surely the general contains the particular?—No; he says: "Starvation to an extent calculated to cause pain would come under the Act." That is obvious. I did not want him to tell me that. I wanted to know whether he considered that Dr. Noel Paton's experiments were experiments painful under the Act; and he would not tell me.

10538. He could not tell you that unless he knew what the feelings of the animals were?—I think if you starved a horse for 96 hours he would soon know. I think under the ordinary law there are a very large number of convictions every year through the Royal Society for the Prevention of Cruelty to Animals for the very offence of starving animals. I also want to point out this—there is another point which arises out of this: "In any case Sir Matthew Ridley does not propose to institute proceedings in respect of experiments which were conducted so long ago as 1895-6, and not brought to his notice at the time." The "Journal of Physiology" is a publication that certainly must be within the purview of the Inspector of the Home Office. It is the journal of the vivisectors; and to say that a paper in it was not brought to the Home Secretary's notice at the time, one of two things must have happened; either Dr. Noel Paton did not fulfil the instructions that we are told are given to all vivisectors under the Act, to forward to the Home Office papers that they publish describing any experiments that they perform—

10539. (*Sir John McFadyen.*) That they perform under the Act. His contention was that it was not under the Act?—But I think you will find that these experiments of Dr. Noel Paton are in a volume that must have gone to the Home Office—the "Journal of Physiology."

10540. (*Chairman.*) But if it was in 1895 how could the Home Secretary prosecute. It would be too late to prosecute summarily five years afterwards?—I am going to say that the "Journal of Physiology" ought to have reached him as soon as it was published.

10541. But taking his letter in 1900, he was quite right, was he not, in saying that it was too late to prosecute?—Very likely. What I am upon is this: It is almost the invariable rule that vivisectors, so far as I can see, never publish an account of their doings until a good six months after those doings have been completed, so as to escape any possible prosecution under the Act. The Act prevents any prosecution six months after the event, and we can only find out what happens in laboratories at secondhand, either from the publications of the vivisectionists, or from the Annual Report of the Home Office, which itself is generally only published three or four weeks before the expiration of the six months.

10542. I only asked the question, because from the Home Secretary's letter there might appear to be a discretion with him as to whether he should prosecute; but in fact the period for prosecution was up?—That is not the point. The point is that Dr. Noel Paton's assurance that he did it outside the Act is immediately accepted by the Home Office. It seems to me that a perfectly impartial person at the Home Office would have said, "These are painful experiments, they are done by a man outside the Act; he has done them outside the Act, and we will withdraw his licence from him or inflict some kind of censure upon him." Now I turn to Question 89 in the evidence of Mr. Byrne. I just want to show that this ought to have got to their notice—I think it ought, at any rate: "Now as to the records and reports of experiments which under statute a licensee is obliged to make, have you any observations to make as to the way in which that is complied with? Is that enforced by the Home Office?" and his answer is: "Decidedly it is enforced by the Home Office." Then the next question is: "Do you find that such records and reports are sent in regularly by the

licensees?" and then it goes on. I take all this evidence down to Question 94 practically to amount to an obligation placed upon vivisectors to send in any publications they make of their experiments. I think Dr. Noel Paton ought to have sent it in.

10543. (*Sir Mackenzie Chalmers.*) There is no obligation to send it in within a very short time. They ought to send it in soon?—I do not know what your regulations are, of course. You see, every day's delay puts a man further and further out of the possibility of prosecution under the Summary Jurisdiction Act.

10544. (*Mr. Ram.*) You are going to deal later on with the alteration of the present law which your Society purposes?—Yes.

10545. It may come up then, perhaps?—Yes.

10546. (*Chairman.*) Have you arrived now at any convenient stopping point in your evidence?—I will just finish this one charge, if I may?

10547. If you please?—Here is the answer to Question 95, which I think is important: "It is partly that and it is partly from the fact that if the supervision by the Home Office of an investigation going on in the country is to be anything like complete it will involve the examination of all the published investigations—investigations as to which anything is published," and so forth. That, I think, shows that this thing ought to have been sent in. And here, again, from Question 97 onwards: "The papers sent to the Home Office are usually not mere descriptions of experiments, but relations by a doctor of the steps

of an important investigation which he is carrying out, or the progress he has hitherto made," and so forth.

10548. Does not this relate rather to the question of amending the Act, as Mr. Ram says, rather than any charge?—Perhaps. The effect of the evidence from Question 95 down to 102 comes to this. I think the evidence amounts to an admission on the part of Mr. Byrne that the Home Office have no means whatever of finding out if and what suffering is inflicted upon animals. That is what it comes to. The Home Office cannot judge. Look at Question No. 101: "Why not? (A.) Because the report (which I call a record and report) sent to the Home Office is a mere list of the experiments which he has performed under various headings" (that is the thing he makes his report upon). "It is in no way a description of the experiments." So that by the official things sent in to them they cannot judge what suffering is involved at all. And, broadly, my contention there is that the Act is so administered as to put the considerations of science above those of humanity; whereas the intention of the Act was to put humanity first.

10549. Does that conclude your specific charges against the Home Office?—No, that has got down as far as Charge 5, and there are seven more.

10550. Might I suggest that you might look over those seven and see whether you cannot shorten them, because there are some of these five that I think were matters too small to have much effect upon one's mind, and if you look through your seven other charges you will probably find that you can shorten it?—Very well; I do not want to be too long.

*Hon. S.
Coleridge.*

19 June 1907.

TWENTY-FOURTH DAY

Wednesday, 26th June 1907.

PRESENT:

The Right Hon. the Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.

Sir W. S. CHURCH, Bart., K.C.B., M.D.

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.

Sir J. McFADYEAN, M.B.

Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.

Mr. W. H. GASKELL, M.D., F.R.S.

Mr. J. TOMKINSON, M.P.

Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

The Hon. STEPHEN COLERIDGE recalled; and further Examined.

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10551. (*Chairman*.) You are going to continue your charges against the Home Office?—Yes, my Lord. I have taken the opportunity, in deference to your Lordship's request, and I have endeavoured to shorten the material which I have to present to you for the remaining charges as much as I can. I have maintained the general order, but I have reduced it a good deal, so that it will not take very long. Just before I begin on them I should like to allude to one or two things which occurred on the last occasion. Your Lordship asked me first of all where the quotation from Dr. Johnson came from. I have looked it up, and I find it comes from the "Idler," No. 17, and there is another piece of the sentence before it which I should like to read; I had forgotten it, but I think it is very much *ad rem*. He says: "The *Idlers* that sport only with inanimate nature may claim some indulgence; if they are useless, they are still innocent; but there are others, whom I know not how to mention without more emotion than my love of quiet willingly admits. Among the inferior professors of medical knowledge is a race of wretches whose lives are only varied by varieties of cruelty; whose favourite amusement is to nail dogs to tables and open them alive; to try how long life may be continued in various degrees of mutilation, or with the excision or laceration of the vital parts; to examine whether burning irons are felt more acutely by the bone or tendon; and whether the more lasting agonies are produced by poison forced into the mouth, or injected into the veins. It is not without reluctance that I offend the sensibility of the tender mind with images like these. If such cruelties were not practised it were to be desired that they should not be conceived; but, since they are published every day with ostentation, let me be allowed once to mention them, since I mention them with abhorrence. *Mead* has invidiously remarked of *Woodward* that he gathered shells and stones, and would pass for a philosopher. With pretensions much less reasonable the anatomical novice tears out the living bowels of an animal and styles himself physician, prepares himself by familiar cruelty for that profession which he is to exercise upon the tender and the helpless, upon feeble bodies and broken minds, and by which he has opportunities to extend his arts of torture, and continue those experiments upon infancy and age, which he has hitherto tried upon cats and dogs." Then it goes on with what I read before.

10552. That leaves no doubt about Dr. Johnson's views in his day?—Yes, I thought you would like it. He is a man of very high character. Of course, that has all to do with vivisection without anæsthetics. I

wish to say now that I have brought here the letters from the eminent surgeons whom I quoted, and I find on looking at them that I quoted in full all the short ones, and that I did not quote altogether in full the longer ones; but I have them all here, and I will read them to the Commission if they like to hear them all.

10553. It seems a pity to read them all over again; if you will read the omitted passages that will be enough?—I will read those of which I only published part. What I was reading from, I may say, was printed matter. I was reading from a reprint of my letter to the Home Office, which I found only embodied part of them. I did not know whether it did or not until I got them.

10554. I observe it was indicated in the print that there were parts left out?—Yes; I gave the shorthand writer what I had been reading from, and he put in the dots, no doubt.

10555. Perhaps you had better leave it if you have not got it handy. You will be asked some questions about it afterwards in cross-examination?—Very well. Then there was a mention of the Ferrier-Yeo case, and its similarity to the Cecil Shaw case, which was mentioned by Dr. Gaskell. I have looked it up, and I find that the similarity is very slight. I accepted Dr. Gaskell's suggestion that it was a parallel case; I said "yes," but I find I was wrong. I have taken the opportunity, since the last meeting, to refresh my memory by reading a report of the trial of Professor Ferrier—it is reported in the "British Medical Journal" of 19th November, 1881, pages 836 to 842, which I have here if anybody likes to look at it. I find that the two cases rest on by no means the same footing.

10556. I do not think this is really material. It was an incidental observation really. Whether it is parallel or not is not very material?—Only the innuendo conveyed is of importance to me. I do not mean to use the word innuendo in any offensive way, but the suggestion conveyed was that inasmuch as the Ferrier-Yeo case was a hopeless failure, and ended in the acquittal of the defendants, therefore it was a parallel case, and therefore if an action had been brought against Mr. Shaw a similar result would have ensued—that was the suggestion made to me.

10557. It does not seem to me to be material in this case. Whether the Home Office ought to have prosecuted in Dr. Shaw's case or not must be really independent of what happened a good many years before in the case against another surgeon even if it were a

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parallel case. One does not mean when saying that a case is parallel to say that it is a *facsimile*. The Home Office must form their own judgment on the Shaw case, and they do not defend themselves on the ground that it was a decision in another case which hindered them from taking action?—I do not know what they did. If Dr. Gaskell will say that that was not his suggestion—

10558. I do not think it was meant in that way in the least?—It is very short; I only just want to point out the difference between the two cases. I wish to correct my own statement. I accepted Dr. Gaskell's suggestion to me that it was a parallel case. I find that it was not. It was before my time.

10559. It was only a suggestion; it was not an assertion that he had examined the case and found it to be parallel.

(Dr. Gaskell.) Not in the least.

(Witness.) I like to be quite accurate. If I accept a suggestion and find I am wrong I like to correct it. The Ferrier-Yeo case was a private prosecution brought by my Society, but before my time, and showed very clearly the difficulties under which a prosecution by a private society labours. My answer to Sir John McFadyen was that the Government officials should have prosecuted. The other difference is, that in the case of Professor Ferrier the charge rested entirely upon a newspaper report, a portion of which was dictated from memory by Dr. Roy, who subsequently stated in the witness box that the report was inaccurate. Professor Shaw, on the other hand, was the author of the paper, in which he described his own work: "My own work has consisted of," he said. "In the experimental research my aim was to discover. . . This rabbit I have kept alive. . ." and he never suggested that the "British Medical Journal" had inaccurately printed his own paper. Therefore on those grounds I say the two cases were quite dissimilar. There is another point that I could not find at the moment, viz., the question and answer in the House on the 26th June, 1903, which deals with the number of experiments actually witnessed by the Inspectors during the whole year. I have got it now, and should just like to read it:—"Dr. Shipman (Northampton): To ask the Secretary of State for the Home Department how many experiments on animals under licence alone, and under Certificate B respectively, have been witnessed by an Inspector at any time during the experiment, and during the whole duration of the experiment."

10560. (Sir Mackenzie Chalmers.) What year was that?—The 26th June, 1903. The answer, by Mr. Secretary Akers Douglas, was: "During 1902 eleven experiments under licence alone were witnessed by the Inspector, and one under Certificate B. In the case of one (under licence) the whole experiment was witnessed; and with regard to the others the Inspector usually remained a considerable time, and on some occasions made more than one visit during the progress of the experiment. In the case of the one under Certificate B the Inspector also visited the animal next day. Inasmuch as the Inspector habitually visits without notice he cannot be sure of arriving at the beginning of an experiment so as to be able to see the whole of it, nor even of finding any experiment in progress. It may be added that the Inspector at his visits always sees any animals on which experiments may have been made, and carefully notes their condition." I want to say as regards this Certificate B, as I am on it, that down from November, 1892, to October, 1899, the words of the certificate as issued by the Home Office were "such animals being during the whole of the experiments under the influence of some anæsthetic of sufficient power to prevent their feeling pain." It is "during the whole of the experiment" down to 1899. In October, 1899, the words were changed to these: "Such animals being, during the whole of the initial operation of such experiments, under the influence of some anæsthetic of sufficient power to prevent their feeling pain." I became aware of this very vital change in the wording of the certificate, which seemed to me, and still seems, to admit of a great deal more pain being inflicted upon animals than the certificate in its former form; and I, therefore, wrote and asked the Home Secretary this question: "Whether conditions are invariably added to this certificate" (that is, Certificate B) "enforcing the employment of anæsthetics ensuring insensibility to pain from the commencement of the first operation until the conclusion of all

manipulations of the animal, and until the last wound is closed aseptically?" That was my question. The object of that question is obvious. There are experiments, as I presume you are all aware, in which the animal is opened and various operations of an initial kind are done to it, after which it may be kept for hours in a condition of disembowelment during the period when observation of its condition takes place. What I wanted to know was whether during that period of observation the animal was lying disembowelled without any anæsthetic, under this certificate; therefore I asked that question, because after all the manipulation of the animal, if it was kept under anæsthetics during all that period of observation and until the last wound was closed again aseptically, it seemed to me that the certificate did preclude a great deal of torture which it otherwise would not. The answer is most illuminating. The answer was "Whenever the experiment would involve a series of cutting operations (which is very infrequent), conditions are added to the licence enforcing the employment of anæsthetics ensuring insensibility to pain during all operative procedures." That is the answer; and that answer to that plain question leaves me under the conviction, which I am sure the Commissioners ought to share, that that certificate admits of a very large amount of extreme pain and awful suffering, because under that certificate the animal may be kept disembowelled alive without anæsthetics as long as actual cutting operations are not being performed upon it.

10561. (Chairman.) Do you propose to show us any case in which that has happened?—My lord, I am dealing with the law.

10562. I know?—I am not on laboratories.

10563. You are a little too combative, I think?—I do not wish to be so.

10564. I merely asked the question in order to ascertain exactly how you apply that proposition, because you spoke of keeping the animal under anæsthetics first, while it continued lying on the table with its bowels open, and then in another place you added the words "until the wound has been aseptically closed," which is a long time afterwards, and possibly includes a time in which the suffering would be comparatively small. Those are two very different propositions. I wanted to know whether you were going to illustrate it by any particular case, because I quite see the force of it if it is not too widely applied?—I have not got the volume here now I am afraid; I thought I brought it.

10565. I daresay you will come to some other point when you can give an illustration?—Then I pass away from that for the moment. I wish to say a word now on the feeding of kittens with cow's milk, which was brought up on the last occasion. I daresay you will remember that my objection to that question was that in my opinion it was conceived in a spirit of railery against those whom I represent. I perfectly unexpectedly found myself face to face with the suggestion here that the object of that experiment was to create disease in the animal by giving it diseased milk. I am right, am I not?

10566. (Sir Mackenzie Chalmers.) No. The point is that the man who wanted to perform the experiment wrote up to know whether it was necessary to take out a licence or not, because the object of his experiment was to try the effect of substituting the milk of another animal for mother's milk. He did not know what the effect would be, whether the mere substitution of different milk for the mother's milk would produce disease or not?—Whether weaning a kitten would do so! All kittens are weaned, surely, from their mother's and given cow's milk.

10567. I think they are generally suckled by their mother?—But there comes a time when they are weaned, like children are given the bottle.

10568. This was at an early stage to see what the effect of substituting other milk for cat's milk would be after the very earliest stage, and he was advised, at any rate, that it might produce deleterious effects on the kitten, and it would be safer for him to have a licence, just the same as with those fish feeding experiments. As you know, a licence is required in cases where fish are kept in a pool where there is effluent sewage, where if the water becomes contaminated the fish get ill and die. A licence has been granted for that, as you know.

10569. (Chairman.) I did not consider that the question was asked in a spirit of railery. The case had

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really come before the Home Office, on which they had to advise?—My point only is that as no suggestion of disease was made on both the former occasions when this cat was alluded to, I naturally was unaware that that was the object of his experiment. If the object of the experiment very likely included suffering to the kittens, it was, of course, a very proper case for having a certificate, and there is nothing more to be said about it.

10570. (*Sir Mackenzie Chalmers.*) There is just, perhaps, one point that I might clear up here. The witnesses for the anti-vivisection societies have taken up two lines. One line is this: that they object to experiments causing pain. Another line taken up has been that we have no right to exploit an animal, whether it causes pain or not, for the purpose of advancing human knowledge. Both those lines have been taken up before us?—I see. Of course, I cannot be responsible for other anti-vivisection societies.

10571. And this point, of course, was in my mind when I asked the question—that a great many experiments licensed by the Home Office, which appear as experiments, are absolutely painless in their results?—Then I pass from that. There was a question asked me by Sir William Collins: “Are the words of Certificate A mentioned in the question,” referring to a question asked in the House of Commons—that is, whether Certificate B was mentioned as among the certificates dispensing with anaesthetics. I said “Yes, they were.” I could not find it at the moment. I have got it now. Here it is: “Table 1. List of licensees who performed experiments under their licence upon animals. Experiments on cats and dogs without anaesthetics, E always coupled with A or B.” I say that that is a general statement by the Home Office that Certificate B is for an experiment without anaesthetics.

10572. (*Sir William Collins.*) That is rather an answer to question 10328. Is it your suggestion that cases under Certificate B were classed among those dispensing with anaesthetics?—Yes, I have found it. They were classed under Experiments dispensing with anaesthetics up to 1897, at any rate. I have it here in the Table for 1897.

10573. (*Chairman.*) You have gone through the first five charges, and you said that there were seven, which I suggested might possibly be shortened?—Yes. I go on with my Charge VI. In this charge I charge the Home Office officials with having suppressed in the annual Parliamentary Return the names of those who take upon themselves the very grave responsibility of signing the certificates exempting licensees wholly or in part from the obligation to employ anaesthesia in their vivisections, although in the report of the former Royal Commission we find this clear direction: “We recommend that his (the Home Secretary’s) advisers should be from time to time elected and nominated by himself; their names should be made known to the profession and the public.” Now, my Lord, the Act of 1876 having provided that licences and certificates exempting licensees from the use of anaesthetics must be signed by persons holding certain official positions, if the Parliamentary Reports were drawn up in an open, straightforward manner they should give the names of these persons, so that anyone could ascertain who was responsible for exempting each individual vivisector from the obligation to use anaesthetics in his experiments, but it is quite impossible to discover from the report who assumes this very grave responsibility.

10574. What are the precise words of the Section as to what is to be reported?—Section 11: “Application for a licence under this Act.”

10575. That names the persons who are entitled to give a certificate?—Yes.

10576. But is there anything about the reporting in the Statute; or is that only in the Order?—No, it is all “may,” I think, all the way through; it is not “must.”

10577. (*Sir Mackenzie Chalmers.*) “An application for a licence and certificate under this Act must be signed by one or more of the following persons”—that is to say, the President of the Royal College of Surgeons, and others?—Yes, but what his Lordship asked me was whether it must be put in the report.

10578. (*Chairman.*) You are complaining that in the report they do not report those facts?—Yes, that

they do not give us the names of the persons who signed the certificates.

10579. Is that based upon what they ought to do for the convenience of the public, or upon what they ought to do if they are to obey the words of the Order?—It is what they ought to do if, as I say, there was no desire on the part of the Home Office to be partial or to screen the vivisectors from criticism.

10580. You do not put it upon any express words, but you put it upon the general duty which arises from the Order?—I also all through my evidence wish to rely very largely upon the recommendations made by the last Royal Commission, which sat before the passing of the present Act, and I say that that Report clearly contemplated the publication of the names of those who take upon themselves this grave responsibility. I pointed out this strange concealment of their names to several of those who may be assumed to be the persons concerned, and I asked them whether it was their desire that their individual names should thus be hidden under the cloak of an official title. Some of them, I think, have been manly enough to tell me that so far as they are concerned they have no objection whatever to the publication of their names; and others have told me that they consider it an insult to suggest that they wish to hide their names—which may or may not indicate that they would not object to their names being published. But whether any of them object or not, I am here to protest against the suppression of their names by the Home Office officials in a public document purporting to inform the public who sign these dreadful certificates. By the system of concealing their names under the description of their official positions, the Home Office officials themselves seem unable to know who the individuals really are who sign these certificates. In the Parliamentary report, dated the 8th June, 1899, I find Professor Schäfer’s certificate (I take Professor Schäfer because he has been here), is stated to have been signed by the “Professor of Physiology, University College, London.” That is the cloak under which the name is concealed. According to the Medical Directory for 1899, in that same year, the Professor of Physiology of University College was Professor Schäfer himself. The Home Secretary, therefore, was asked, by my request, in Parliament, “Whether the Home Office authorises or allows certificates to be signed by the operator himself on his own behalf?” and the reply was that such a thing “Would be directly contrary to the proviso in Section 11 of the Act.” That answer is in Hansard of 24th June, 1899; I have it here if anybody wants to see it. I therefore wrote both to the Home Office and to Professor Schäfer himself to elucidate the truth, inquiring who the person was who is described in the Parliamentary report as “the Professor of Physiology, University College, London.” The Home Office said (I have got the letter here), that this signatory was Sir John Burdon-Sanderson, who had resigned the Professorship of Physiology at University College, London, many years before. Professor Schäfer said—this is what he said to me—“I have not signed any certificates on my own behalf. The certificates I hold are signed by Lord Lister and Professor Halliburton only.”

10581. (*Sir Mackenzie Chalmers.*) Is it licence or certificate?—“I have not signed any certificates on my own behalf” (he says. “The certificates I hold are signed by Lord Lister and Professor Halliburton only.”

10582. Professor Burdon Sanderson, I imagine, signed the licence?—I do not know, I am talking about certificates now.

10583. (*Chairman.*) Was the answer of the Home Office an answer about certificates or a licence. You said that the Home Office referred to Sir John Burdon-Sanderson?—“Whether the Home Office authorises or allows certificates to be signed by the operator himself on his own behalf”—that was the question. I framed the question, so I know what it was.

10584. And the answer was what?—The answer was that it would be directly contrary to the Act if he did it. I then proceed, as I say, to ask Professor Schäfer himself, and he said that he did not do it. He said, “The certificates I hold are signed by Lord Lister and Professor Halliburton only,” whereas the Home Office said it was signed by Sir John Burdon Sanderson.

10585. That is what I wanted to know. What does the Home Office say was signed by Sir John Burdon

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Sanderson. What is the word they use in their letter?—We will get the letter.

10586. (*Sir Mackenzie Chalmers.*) May I read the letter, which your assistant has been kind enough to hand to me? "With reference to your communication of the 15th ultimo, enclosing a copy of Mr. Coleridge's letter of the 14th December last (which the Secretary of State regrets has not received a reply), I am instructed to say that in no case is an applicant for a licence under the Act 39 and 40 Vict., cap. 77, allowed to sign his own certificates, and Professor Schäfer has not done so. His licence was originally given on the recommendation of Dr. Burdon-Sanderson, by whom the certificate was signed, and who was at the time Professor of Physiology at University College, London, which post Professor Schäfer now holds. I am to add that the laboratories of the Mason University College, Birmingham, are not registered for the performance of experiments under the Act." That is neither here nor there?—I say that there is no inaccuracy in my statement which I make here. The Home Office said that Sir John Burdon-Sanderson signed the certificate.

10587. (*Chairman.*) Signed the licence?—Signed the licence and the certificate. I am not on the licence.

10588. I quite understand that; that is the reason I asked the question?—

10589. (*Dr. Gaskell.*) But not that certificate?—Yes.

10590. He signed the certificate at the time he signed the licence—that is years before?—But we have been already told that certificates expire every 12 months.

10591. (*Sir Mackenzie Chalmers.*) Perhaps it would be convenient once for all to state the practice. The practice is this: The licence has to be renewed every year, as you know. At the end of the year it comes up for consideration. It is originally signed by the people mentioned in the Act?—Is that so? They only recommend for a licence.

10592. Yes, they recommend for a licence. It does not go back to them. Every year the licence has to be renewed before the 28th of February. If the licence is not renewed the certificates fall with it. If the licence is renewed the certificates are also considered, and if they are not exhausted, that is to say, if the number of animals and experiments authorised by the certificate have not been exhausted, then it comes up for consideration whether that certificate should or should not be continued; it does not go back to the original people who gave the certificate, but comes up before the Home Office inspector at the Home Office for continuation. A licence, of course, may be renewed and the certificates not continued, or fresh certificates may be obtained?—

10593. The certificate necessarily falls if the licence falls; but they may or may not be continued if they are not exhausted?—But all that does not explain how it comes about that Professor Schäfer thinks the certificate was signed by one person, and the Home Office thinks that it was signed by another.

10594. (*Chairman.*) It is confusing, I agree, as appears from the different letters; but what I gather is that Sir John Burdon-Sanderson at the beginning recommended the licence and also recommended the certificate?—Signed the certificate.

10595. And also signed the certificate?—Yes.

10596. And that being started is referred to as being his signing the certificate in that letter?—Yes.

10597. But that subsequently the persons who are responsible for Professor Schäfer's certificate at the time in question are Lord Lister and Professor Halliburton. That is what I should gather from it.

10598. (*Dr. Gaskell.*) Might I just ask what the date was when Sir John Burdon-Sanderson signed that licence and certificate?—I have not got it. You must ask the Home Office. That is my point, that nobody can tell.

10599. All I meant was that if Sir John Burdon-Sanderson signed a certificate for a certain experiment it is very unlikely indeed that Professor Schäfer could be doing that same experiment many years after. He would probably have to get a new certificate?—The point, if I may say so, that I am making is my desire, and the public desire, the humane public, to know who signs certificates authorising vivisectors to do without anaesthetics, and when we come down to any particular case we find it almost impossible to find who that

person is who takes that grave responsibility, and the confusion is accentuated by the Home Office saying that it is one person and the vivisector himself saying that it is another. That is my point. In this case there is no mistake about it. The Home Office said it was one person who vouched for Professor Schäfer's certificate in a particular year, and Professor Schäfer says it was two other gentlemen. This, therefore, I say, is a most informing example of the results arising from the use made by the Home Office officials in the Parliamentary Return of titular professorships as cloaks to hide the identity of the vivisectors who sign each other's certificates. Professor Schäfer's certificate is stated in the Report of 1899 to have been signed by "the Professor of Physiology, University College, London." The Medical Directory for that year told us that this person is Professor Schäfer himself. The Home Office assert that the person is Sir John Burdon-Sanderson, and Professor Schäfer says the person is not himself but Professor Halliburton. The task of reconciling these statements is quite beyond me. I say that the impossibility of tracing the identity of the persons responsible for signing certificates has been rendered absolute by the statement of the Home Secretary in the House, that all certificates expire on the 31st December. I understand now that they do not. My position about that is very clear. I say that here are these persons who sign these certificates. Many of them have told me themselves that they have no objection to their names being published. I have protested again and again to the Home Office against their names being concealed in this manner, and I protest again here against their names being thus concealed. If they have nothing to be ashamed of why are not their names published? It is a very grave responsibility that they take upon themselves in giving their brother vivisectors leave to vivisect without anaesthetics.

10600. (*Chairman.*) You attack these gentlemen very strongly?—I am not attacking these gentlemen; I am attacking the Home Office.

10601. You said, why do they wish their names to be kept out just now?—I do not say that they do. I did not mean that. I say, Why are their names kept out? Some, I say, have been manly enough to say that they do not wish to have their names kept back.

10602. I thought what you read certainly was a charge that these gentlemen themselves were extremely anxious to cloak up?—No, on the contrary, I have read what I wrote.

10603. I mean what you read just now?—I give in detail what they said to me in answer to my letter. Some said that they had no objection; others said it would be an insult to suggest that they wish to hide their names. Nevertheless, their names are hidden for them by the Home Office, and against that I protest. I pass on to the next charge. I am here to charge the Home Office officials with shielding the names of such licensees as they know to have broken the law, although the former report contains these words:—"Abuse of the power conferred by the licence must, of course, render the holder liable to its withdrawal, but this will involve great disgrace"—a phrase, in my opinion, which clearly indicates that the framers of that report contemplated the publication of offenders names, for no great disgrace can possibly appertain to a secret remonstrance or a secret withdrawal of a licence. Under Section 11 of the Act, 39 and 40 Vict. c. 77, a person who performs experiments calculated to give pain without a licence in contravention of the Act is "guilty of an offence against this Act," and shall be liable to a penalty not exceeding £50 for a first offence, and for a second to a penalty not exceeding £100, or to imprisonment not exceeding three months. Anybody who commits an offence against any other Act of Parliament which creates a criminal offence, and who confesses himself guilty of the offence, would in the ordinary course be brought before the Courts and subjected to sentence. The Home Office officials, however, have never instituted proceedings against any vivisector for committing admitted offences against the Act, not even in a case described by themselves as a "case of deliberate violation of the Act after warning."

10604. (*Colonel Lockwood.*) What was that case?—That you will find at Question 38. I do not know to this day what the case was; the case has never been revealed to us, not even before the Commissioners. Mr. Byrne said: "This power has been exercised on four occasions," and at the end of that answer you will see "He had been previously warned as to the necessity

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10605. (*Sir Mackenzie Chalmers.*) Would you just give the facts first. A licensee who did not hold certificates B and E performed the operation of gastric fistula on a cat under anaesthetics, the animal being allowed to recover. That was the case?—Yes, a breach of the Act—a clear breach of the law.

10606. And his licence was revoked?—Yes, his licence was revoked. But that fact—the fact of his licence being revoked, and the licensee's name were never mentioned even in the annual report. And, further, instead of prosecuting and punishing him, he was quickly reinstated as before, this very man, though we still do not know his name.

10607. (*Chairman.*) And you say that his name ought to have been mentioned in the report?—Certainly.

10608. And he ought to have been prosecuted?—Certainly.

10609. (*Colonel Lockwood.*) There is a footnote, you see, to that evidence?—He was not only not prosecuted, he was reinstated; after just a decent interval of a month or two he was put back where he was, as if nothing had happened. "Mr. Byrne subsequently wrote that a few months later the gentleman in question was allowed a fresh licence and Certificate A for inoculations, after special inquiry, on his representation that the revocation of this licence had stopped a very valuable investigation." We are not told what the "very valuable investigation" was. To this day the public cannot discover the name of that offender. Proceeding a little further, I tried to see if I could get it from the Home Office by writing to them. In the House of Commons, I may say—this is in Hansard of the 26th June, 1903, page 629—the Secretary of State said: "Wherever the conditions of the licence were violated the severest notice was taken of it, and, if necessary, the licence was revoked." I wrote and asked "what were the instances of violation of the conditions of the licence to which you alluded in your speech above quoted, of which the severest notice was taken?" I wanted to know who they were. The reply was: "The Secretary of State does not think that any useful purpose would be served by the publication of the names of these experimenters." I just wrote that letter to test it, to see whether they would continue to shelter the names of the peccant vivisectors, and they did.

10610. (*Sir Mackenzie Chalmers.*) There is one preliminary point that I should like to clear up. If you write to the Home Office it is only a matter of courtesy that they answer you?—Yes. I do not complain of any lack of courtesy on any occasion from the Home Office.

10611. The only place in which the Home Secretary is answerable is in Parliament?—Yes.

10612. As a matter of courtesy, they answer your letters, but if the Home Office undertook to give explanations to every one of the 35,000,000 people in England who write to them about any of their business it would be a little more than they could do?—But without undue egotism I may say that I do not regard myself as writing to the Home Office on any of these occasions in my own individual capacity. I have never written to them in any other capacity than as honorary secretary of a great and important society. I quite admit that in my own individual capacity I would have no claim.

10613. I do not know of any difference between a great and important society and any single individual who asks a question?—I only say for myself that I should not write except as the representative of what I consider more important persons than myself.

10614. (*Chairman.*) Does that conclude that charge?—Yes. I come now to my eighth charge. I am here to charge the Home Office officials with preparing for the Home Secretary evasive and insufficient replies in the House of Commons to plain questions on the administration of the Act, and with making evasive and insufficient replies themselves in official correspondence with my Society, and with leaving perfectly proper questions unanswered altogether. I accept Sir Mackenzie Chalmers' dictum that they are not obliged to answer any question at all,

of course; but at the same time I think that on a question of the administration of the Act they would naturally answer reasonable questions, and I think they generally do.

10615. (*Sir Mackenzie Chalmers.*) You can take it for granted that they will always make inquiry. They would not necessarily tell you the result of that inquiry?—That is rather my complaint. Mr. Paulton, on the 24th July, 1899, asked "Whether Dr. Poore, while holding the office of Inspector, had signed certificates in his capacity of Professor of Medical Jurisprudence, University College, London"; the answer of Sir Matthew White-Ridley was "The answer is in the negative." I am assuming, of course, that these answers are prepared by the permanent officials. Colonel Lockwood asked: "Does Professor Poore sign the certificates?" to which Sir Matthew White-Ridley replied: "I am informed that it is not the case that he does so." Then Mr. Paulton asked: "Then how is it that the statement appears in the Report of the Inspector? Will the right honourable gentleman further inquire as to its accuracy?" and Sir Matthew White-Ridley replied: "Yes. I was only aware five minutes ago that the Report seemed to indicate this, and, of course, I have not had time to inquire." Now, the certificates signed by Dr. Poore were still in force while he was Inspector, and the official who prepared Sir Matthew White-Ridley's answer must have known it; they must have known that these certificates signed by Dr. Poore were in force at the time the question was asked.

10616. (*Chairman.*) Would you read the question again?—The question is "Whether Dr. Poore, while holding the office of Inspector, had signed certificates in his capacity of Professor of Medical Jurisprudence, University College, London"; and the answer is in the negative. I quite agree that it is perfectly accurate, but I venture to say that it is extremely evasive. The Home Office officials knew perfectly well that these certificates had been signed by Dr. Poore before he was Inspector, but in my opinion it was a strange thing that they should not have added to the answer "Yes, they were signed by Dr. Poore, but not while he held the office of Inspector."

10617. (*Sir John McFadyean.*) Might I ask why that question was not put in a form which would have made evasion impossible? Why was not the question, whether the certificate which was signed by Dr. Poore before he took office was still in force?—Because we did not know it. According to the documents before us, it appeared that he had signed certificates while he was Inspector, because we had at that time the statement of the Home Office that certificates expired on the 31st December. Until this explanation of Sir Mackenzie we had always believed that that was so. Therefore it seemed impossible that Dr. Poore had not signed them while he was Inspector, because there was his name on them, and apparently he was the person. It transpired that he had signed them many years before, and instead of expiring on the 31st December they have been kept alive nominally, Sir Mackenzie Chalmers now tells us.

10618. (*Chairman.*) You say that the replies of the Home Office are evasive. What had the Home Office to conceal in that case? What were they evading? It seems to have been perfectly regular?—No, they did not tell the public the truth.

10619. Evasive, you say, and, apparently, from the way you put it, wilfully evasive answers?—Yes, wilfully evasive.

10620. Therefore they were going to evade something?—Yes.

10621. What was it they were trying to evade, because if they had said all that you say they should have said, nothing could have been said against them?—Yes, I think the public would have been very much surprised and indignant at discovering that a vivisector held a certificate exempting him from the use of anaesthetics, signed by the very man who was to inspect him to see what was done.

10622. No, signed by a man before he was Inspector at all?—But still responsible. The certificate was alive.

10623. The certificate being there, it was alive by virtue of the Act of Parliament?—Yes, quite so, and in my opinion a very improper thing.

10624. You may amend the Act, but I cannot see what the Home Office have got to evade?—The Home Office have themselves said that the person who is Inspector ought not, under the Act, to sign certificates.

10625. And Dr. Poore did not do that?—But the certificate was kept in force after he was Inspector.

10626. That is a totally different thing?—No, I do not see it. I think he was a most improper signatory.

10627. When you say that you do not see that it is a different thing you startle me?—I do not. If it is kept alive every year, it is the same thing as his signing it every year again. That is my opinion, and I think it ought to be the opinion of everybody.

10628. (*Sir John McFadyean.*) The Government have not admitted your view of the question, have they?—What view?

10629. The view you have just been putting forward, that it is improper to allow a certificate to continue because it was signed by the Inspector before he took office?—The Government have said that it is not proper for a man to sign his own certificate.

10630. That is not the question. They have not admitted your view?—Oh, dear, no.

10631. Therefore it hardly seems fair to charge them with evasion; because the charge of evasion seems to imply that your view was necessarily the right one?—I say my view is the view that the general public would take; I am quite convinced of that. That is my charge, and I am quite convinced that the general public would take the view that it is very improper for the Inspector to sign the certificates of the very vivisectors he had to inspect. I am quite convinced of that, and I should have thought the Commission would have been so, too.

10632. (*Chairman.*) Your conviction does not necessarily govern ours?—No, my Lord. I invite you to share it; I can do no more.

10633. (*Sir Mackenzie Chalmers.*) Mr. Coleridge's point, as I understand it, is this: that when a gentleman who has signed certificates or signed recommendations for a licence becomes an Inspector, all licences and certificates signed by him ought, *ipso facto*, to expire?—You have incorporated the licences into it. I have not said anything about the licence. I am entirely upon the question of certificates exempting men from using anaesthetics. With that alteration I accept that statement as my view.

10634. (*Sir William Collins.*) Is your point that Dr. Poore, as a matter of fact, was inspecting work that was being done as the result of certificates given under his own hand?—That is my point, and I think that is very improper. Then, this is not a very important matter, but I should just like to allude to it. I called the attention of the Home Office to Clause 9 of the Act, which provides the machinery whereby results may be collected and subsequently incorporated in the yearly report; and I said that my committee hoped "that the public may, in the next report, be told what results in the way of benefit to mankind and the healing art have been achieved by all the torture that is continually being inflicted upon animals in the ever increasing laboratories of this country?" I asked that very plain question. We are always being told that such wonderful results are being achieved by vivisection, and I asked the Home Office whether, under Clause 9, they would not let the public into the secret and let us have some of the results published in the report.

10635. (*Chairman.*) When was that?—That I wrote on the 8th of December, 1898. The answer was that it would have attention, but nothing further ever happened; the results have never been published in any report. It is not a very important matter, but in passing I should just like to point it out. Now, on the 15th July, 1904, in Hansard I find Dr. Shipman asked the Secretary of State for the Home Department "whether he can state in how many cases the licensed vivisector himself has been the only authority present at the operation whose duty it is to see that anaesthetics of sufficient power to prevent the animal feeling pain are administered?" The answer by Mr. Secretary Akers Douglas was: "I have no information enabling me to answer this question." Surely the Home Office know at how many experiments the Inspector had been present, and consequently, at how many the vivisector himself was obviously,

the only authority alluded to in this question; and therefore I think that that is beyond all question an inadequate answer. On the 22nd July, 1904, Dr. Shipman again asked the Secretary of State for the Home Department "How many dogs were vivisected in University College during the year 1903"—a very plain question. The answer is: "As I informed the honourable Member on the 4th of May last, I have no material from which I can answer this question." No material! Therefore if he does not know how many dogs are vivisected in a particular laboratory, he must know nothing at all about what goes on there. The first thing he must know, if he knows anything about it, is the number of dogs vivisected there. It is no doubt a perfectly true reply, but it seems to me that he was in a position to get, and ought to have got the information before replying. He could have found out by inquiring. I presume they keep records there—they ought to do so—of how many dogs they use.

10636. (*Sir Mackenzie Chalmers.*) There would be no return under the licence, would there? As long as the animal is put under anaesthesia and killed before it recovers from the anaesthesia, it would come in under the licence. There is nothing in the licence to limit the number of animals that can be experimented on in that way?—That I do not know. You issue the licences. I have no idea what powers are given to the vivisectors.

10637. That is the Act itself?—I do not know.

10638. (*Mr. Tomkinson.*) Did that question apply to the number of dogs vivisected during that year?—During 1903, the year before. I asked for the year before. Now, on the 15th of July, 1904, Dr. Shipman asked the Secretary of State for the Home Department "How many experiments under the Act 39 and 40 Vict., cap. 77 have been performed at each place on the register during 1903, and whether he can ascertain how many separate experiments were performed on the same animal during the same period." Here, again, the answer is that there is no material upon which he can give any information. Of course, if he had answered this question he would have known about the brown dog having several wounds in it; but he did not. We know that he did not know anything about that brown dog. I maintain that he ought to have known. I think the Home Office ought to know what happens to every dog that is vivisected. If they have not the machinery at their own command to know, they ought long ago to have asked Parliament to give them that machinery in my opinion, if they care at all to protect the animals from unfair treatment. Then I asked this—and I must say I do think this is a case of evasion: On the 6th October, 1904, I wrote, "I desire very respectfully to inquire whether you yourself, or whether your predecessors in the office of Secretary of State have required from the licensees under the Act any guarantees of their humanity, and if so, what is and has been the nature of these guarantees?" The reply was: "I am directed by the Secretary of State to say that he has no doubt that you are acquainted with the duties imposed upon him" (that is, the Home Secretary) "by the Vivisection Act, and that he has every reason to believe that these duties are duly and faithfully performed by the Home Office." I think that letter is interesting, because it answers a great deal of what Sir Mackenzie Chalmers said about the devolution, and what I wish to say about the devolution of responsibility. Here, over the signature of Henry Cunyngame, we have a statement of the duties that are imposed upon the Home Secretary by the Vivisection Act, and that they are duly and faithfully performed, not by him, but by the Home Office. So that there in that letter the Home Office duly takes upon itself the responsibility for fulfilling the duties put upon the Home Secretary by this Act.

10639. (*Sir Mackenzie Chalmers.*) Does Mr. Coleridge expect the Home Secretary to perform the functions of Inspector?—Of course not. That is what I say. That is my contention. That the Home Office officials are the real persons against whom I bring my indictment. I call that a clear evasion. Because, of course, I know the duties imposed upon him, and I know perfectly well that the Act does not impose the duty in its provisions, but the whole tenour of the Act does, in my opinion. I then wrote: "A large discretion was left by that Act in the hands of the Home Secretary personally, a discretion which I take leave to believe was intended by Parliament so to be exercised as to provide guarantees of the personal humanity of each

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individual licensee. The question that I, therefore, desire most respectfully to put to you is: do you require of each vivisector a guarantee of his personal humanity before granting him a licence or passing his certificates? I put this question" (I say) "as representing a very large number of serious persons to whom your answer is of grave importance in view of future legislation in the matter." I was then drafting my Bill. Again I received a second evasion. "I have laid before the Secretary of State your further letter of the 27th ultimo, and he directed me to inform you in reply . . . that if any instance of inhumanity on the part of a licensee is brought to his notice, he will be prepared to act promptly in the matter." That is the answer which I say is an evasion. I replied: "I have received the letter of the 12th of November, written by your direction, informing me that if any instance of inhumanity on the part of a licensee is brought to your notice you will act promptly in the matter. In view, however, of the fact that you have yourself stated that during 1902 only one experiment upon a living animal out of a total of 14,906 was witnessed throughout by either of your Inspectors, the contingency of which you now speak may be regarded as remote. But I beg leave to point out that I did not ask whether you would act promptly in that remote contingency, and I fear I have failed to make my inquiry intelligible. I will, therefore, very respectfully repeat it as plainly as I can." I then repeat the question: "Do you require of each person licensed under the Act 39 and 40 Vict. c. 77 any guarantee of his personal humanity before granting him a licence or passing his certificates?"—a very plain question. And at last the Home Secretary, or rather the Home Office, takes refuge in silence. "I am directed by the Secretary of State to say that he has nothing to add to his previous letter." They will not answer the question—a masterly silence. I suggest that if he did require the guarantees of vivisectors of their humanity he would have said so, and certainly would not have evaded the question and refused to answer it. I therefore must come to the conclusion that he does not require such guarantees; but though he does not require them he does not like to admit it in a letter that may be published. Therefore I say that in my opinion the Home Office evince more solicitude for the vivisectors and for the Home Office than for the animals under their charge. I will not go into this story of the brown dog and Certificate B, except to say (I do not want to go into it all again) that in my opinion as the two operations performed by Dr. Starling upon that brown dog were admittedly done under Certificate B, and as the words of the certificate tell him that he is to destroy the dog immediately the object of the experiment has been obtained—and it was obtained by the second operation—I think he did an illegal act in handing it over to Mr. Bayliss. It was his business to see it killed, and in my opinion the Act gives the animal a vested right to be destroyed painlessly as soon as the object has been attained of the experiment under Certificate B.

10640. (Chairman.) Does that conclude that charge?—Yes. I pass to Charge IX., which is a very simple one; it is self-evident from the yearly Returns. I am here to charge the Home Office officials with putting forward annually a Parliamentary Return in which it is asserted, on the official authority of the Government Department, that not a single experiment in thousands inflicted on animals in Great Britain with its permission can be specified as entailing any pain at all, when all the while this plausible assertion is based upon no better evidence than the bare assertion of the vivisectors themselves, who are not expected to report themselves as cruel men.

10641. (Sir Mackenzie Chalmers.) The form of the Report has been altered since then?—Yes, down to a certain date the form of the return was "Return showing number of experiments performed on living animals during the year 1903 under licences granted under the Act 39 and 40 Vict. cap. 77, distinguishing painless from painful experiments." If this paper is supposed to distinguish between painful and painless experiments and you can find inside it no single experiment specified as painful, that is a statement to the public that there are none painful.

10642. When was that form abandoned?—Quite recently, I think.

10643. (Mr. Tomkinson.) That is an annual return?—Yes.

10644. It has not to be moved for?—I think somebody asks for it. Colonel Lockwood would know that.

10645. (Colonel Lockwood.) I think it is laid before the House in the ordinary course; nobody asks for it.

(Chairman.) It is laid on the table by the Department?

(Sir Mackenzie Chalmers.) Yes.

(Witness.) There generally is a question asked, because I suppose somebody wants to know.

(Colonel Lockwood.) Yes, somebody is keen about it.

(Sir Mackenzie Chalmers.) It is only a question about the date.

(Witness.) Charge X. is the next one. I charge the Home Office officials with placing a certain vivisector year after year beyond the reach of the safeguard erected by the Act to protect animals from illegal treatment by licensees, by giving him permission to vivisect in private places, thereby placing him beyond the possibility of legal inspection, because Clause 10 of the Act of 1876 gives the Inspector no right of entry into or inspection of any unregistered place. I am alluding here to Sir Victor Horsley, and there is no denying it. Mr. Byrne, in answer to Question No. 73, says "There were a few cases in which, on account of scientific eminence, or the high position of certain licensees, they were allowed to perform their experiments in unregistered places." High position! High position the former Royal Commission informed us was no guarantee of humanity. They said that persons in very high positions it could not be denied had shown gross inhumanity. Having discovered myself that Sir Victor Horsley could perform experiments where he liked, I wrote to the Home Secretary on the 25th of July, 1905, and I said: "On page 27 there is a note" (I was then alluding to the yearly report) "which runs thus: 'Sir Victor Horsley could also perform experiments at such places as might be necessary for the purpose of his experiments.' You (the Home Secretary) have, therefore, authorised this vivisector to perform his experiments anywhere he likes, and have thereby placed him beyond the possibility of legal inspection, for in Clause 10 of the Act the Inspectors' right to inspect at all is limited to registered places. May I ask whether you have not in this case deprived the animals vivisected by Sir V. Horsley of every safeguard provided by the Act, and whether by so doing you have not shown yourself unmindful of the sacred interests of those pitiful creatures committed to your charge by the law, who are the living victims of experiments? Whatever be your answer to these questions, I take leave to enter my solemn protest, in the name of many thousands of thoughtful and humane people, against this safeguarding of a vivisector from all inspection. The law constitutes you the protector of vivisected animals, and you have made yourself the guardian of the vivisector!" That protest I sent to the Home Office on the 24th of July, 1905, and no notice whatever was taken of it.

10646. (Chairman.) You sent it to the Home Secretary?—I sent it straight to the Home Secretary.

10647. That was Mr. Akers Douglas?—Yes, I think it was some new Home Secretary I addressed, because I hoped to get more from the new Home Secretary than the last. I expect it was soon after a reappointment. In spite of that protest of mine, which is very plain and clear, Sir Victor Horsley's permission to vivisect where he likes remained.

10648. Was it a specific permission given to him in writing, or was it a licence or certificate of any kind. How was it done?—I do not know. That is a question Sir Mackenzie must answer. It is simply a note at the foot of the return. He has had it for years I believe. I had not noticed it.

10649. I understand that it was probably endorsed on his licence?—I really do not know.

10650. (Sir Mackenzie Chalmers.) You do not know as a fact whether he made use of it or not. He asked for it for a special purpose?—I do not know. I am not attacking Sir Victor Horsley. I am attacking the Home Office for giving him permission. Whether he uses it or not is a matter of indifference to me on this charge. I am charging the Home Office with permitting this gentleman to vivisect outside the Act and beyond the reach of inspection.

10651. (Sir John MacFadyen.) Is it your charge that under no circumstances should it be permitted to

be done?—Certainly. No vivisector should be placed beyond the reach of inspection, because the whole Act of 1876 definitely in its provisions implies that every vivisector is to be inspected, otherwise it would not have provided inspectors. And if vivisectors are to be inspected, why is one to be given *carte blanche*.

10652. But might the vivisector not signify his entire readiness to be inspected in an unregistered place? Would that make any difference to your position?—It certainly would. You clearly understand that I am not making any attack upon Sir Victor Horsley.

10653. (*Chairman.*) Is there any special power in the Act given to the Home Office to permit this?—Yes, no doubt it is legal; it is all "may" in the Act. I do not think there is anything compulsory.

10654. For cases done on farms, for instance?—Yes, I am not suggesting that it is outside the Act at all. But what is illuminating is, that though this leave was given to Sir Victor Horsley for many years, and was maintained for many years, and was maintained in spite of my protest in July, 1905, I find that it was withdrawn on the 13th of February, 1906, just a month before the announcement of the Royal Commission in the newspapers, just at the time when we may assume that the Home Office were perfectly well aware that there was going to be a Royal Commission; and it is a very remarkable thing that contemporaneously with the knowledge in the Home Office that there would be a Royal Commission, this leave was withdrawn from Sir Victor Horsley to vivisect where he liked and beyond the reach of inspection.

10655. (*Sir John McFadyen.*) But have they withdrawn all similar leave to other vivisectors?—I should think probably not. There are some, I think, who have to do with inspecting grouse on a moor. I should have no objection to that.

10656. It was a question of the motive which inspired the Government—your reason for attributing this motive to the Government. If they had been in such a fright, would they not have withdrawn the whole of these permissions?—No, I should think not. If it be so—you are putting it into my mouth (I do not know whether they have or have not withdrawn them all)—if they have not withdrawn the others and have withdrawn the one to Sir Victor Horsley, the assumption is that the one to Sir Victor Horsley is less defensible than the others, in the public eye. That is the conclusion I should come to. That finishes charge X. Now I have dealt with Charge XI. under another heading, but I wish to enforce this charge: that although the officials of the Home Office must be aware that the Report of the Royal Commission in 1876 contained this pregnant warning on page xvii: "It is not to be doubted that inhumanity may be found in persons of very high position as physiologists." They apparently have made no inquiries, public or private, into the vital question of the character for humanity of the licensees to whom they have delivered over the animals to be vivisected, and that although the Act of 1876 placed upon the Home Secretary the tremendous responsibility of personally protecting animals from inhumanity. I have already cited this correspondence as showing the evasive replies given by the Home Office to proper questions. I now cite it to show that we must conclude that as a fact no guarantees of humanity are required by the Home Office. I think it is quite clear that that is so, and in addition to the Home Office correspondence, which ends with a frank refusal to continue the correspondence without giving me that information, we have the following question put to Mr. Thane at Question No. 400: "Do you inquire at all into his"—that is, the experimenter's—"reputation for humanity or the reverse or anything of that kind?" To which he replied: "No, I do not inquire about humanity." On the point of the question of the humanity of vivisectors, I wish to draw the attention of the Commission to the evidence given before the former Royal Commission by Dr. Klein.

10657. (*Chairman.*) Have you finished your charges against the Home Office?—No, it is part of this charge—I cover it under this charge. There is one more charge after this, and only one more. It begins at Question 3538: "(Q.) What is your own practice with regard to the use of anæsthetics in experiments that are other wise painful? (A.) Except for teaching purposes, for demonstration, I never use anæsthetics where it is not necessary for convenience. If I demonstrate, I use anæsthetics. If I do experiments for my in-

quiries in pathological research, except for convenience sake, as, for instance, on dogs and cats, I do not use them. On frogs and the lower animals I never use them. (Q.) When you say that you only use them for convenience sake, do you mean that you have no regard at all to the sufferings of the animals? (A.) No regard at all. (Q.) You are prepared to establish that as a principle which you approve? (A.) I think that with regard to an experimenter, a man who conducts special research, and performs an experiment, he has no time, so to speak, for thinking what will the animal feel or suffer. His only purpose is to perform the experiment, to learn as much from it as possible, and to do it as quickly as possible. (Q.) Then for your own purposes you disregard entirely the question of the suffering of the animal in performing a painful experiment? (A.) I do. (Q.) Why do you regard it then when it is for a demonstration? (A.) Because I know that there is a great deal of feeling against it in this country, and when it is not necessary, one should not perhaps act against the opinion or the belief of certain individuals of the auditorium. One must take regard of the feelings and opinions of those people before whom one does the experiment. (Q.) Then am I wrong in attributing to you that you separate yourself entirely from the feeling which you observe to prevail in this country in regard to humanity to animals? (A.) I separate myself as an investigator from myself as a teacher. (Q.) But in regard to your proceedings as an investigator, you are prepared to acknowledge that you hold as entirely indifferent the sufferings of the animal which is subjected to your investigation? (A.) Yes." Now, further down, at Question 3641, you find this: "(Q.) When you take hold of an animal for this purpose, what is done with it; do you bind it up while you are making this injection? (A.) If it is a large vigorous animal, as a dog, we do bind it and fasten it. A cat we generally must chloroform. (Q.) Why do you not chloroform a dog? (A.) We chloroform a cat because we are afraid of being scratched. (Q.) Why not a dog? (A.) If it is a small dog there is no fear of being bitten by the dog." That is the evidence which was laid before the former Royal Commission by Dr. Klein. It was laid before the Commission on a certain Thursday, the 28th of October, 1875. On October the 30th of the same year, three days later, Professor Huxley, who was a member of that Commission, wrote to the great Darwin, and this is what he wrote—

10658. What is this on? Are we to have the Commission of 1876 brought up?—I am going to bring it right up to date. Dr. Klein has a licence at this moment.

10659. That may be; but we are on the question whether the law requires amending?—This is a charge against the Home Office of continuing a licence to a man whose inhumanity is patent.

10660. Then let us deal with that?—I want to show what Professor Huxley thought of this man.

10661. What Professor Huxley thought of Dr. Klein cannot matter?—But he was on the Commission.

10662. That may be. It is thirty years ago, and the law has been entirely altered since. The law now prevents these things. By all means state that Dr. Klein has a certificate still, and that you think that is an improper thing, as I understand; but we really cannot go through the whole of this?—It is not very long; it is a very short letter from Professor Huxley.

10663. The evidence runs very long. I think at any rate there is more recent matter and more material matter which takes up so much time that I think we can dispense with the history of what happened before this Act, which is, of course, the Act we are discussing?—My Lord, I maintain very respectfully that it is very much *ad rem* what a Commissioner thought of Dr. Klein at the last Commission, in consideration of the fact that Dr. Klein has for years had a licence and certificate from the Home Office, and has one now, on the point that they are perfectly regardless of the humanity of the vivisectors, and take no interest in it.

10664. You have read Dr. Klein's own statement. I object to time being taken up by having cited to us what this, that, or the other person, whether a member of the Commission or not, thought of what Dr. Klein said. We have Dr. Klein's evidence, and the only point that is material so far as I understand, is that you complain of Dr. Klein having a certificate?—Well, my Lord, I think this has been already dealt with before this Commission.

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10667. (*Chairman.*) That may be?—Then I think it is material, my Lord.

10668. But the alteration of the evidence was before the other Commission?—Yes, but you have had it dealt with here.

10669. You have cited what he said about his practice. If you wish to qualify that in any way or to increase the force of it by a statement which he himself made subsequently, by all means do it?—You see, my Lord, the point has been made here that Dr. Klein altered his evidence, and it is on that ground that I wish to lay this letter before the Commission, as showing what Professor Huxley, one of the Commissioners at the time, thought of that evidence.

10670. But that Commission is not this Commission, and there is no end to that sort of evidence of what other people thought about Dr. Klein. We have Dr. Klein's statement, which you say stamps him as being an inhuman vivisectioner, and you say that he ought not to have had a licence given him?—I say that he ought not to have one now.

10671. But when it comes to citing other people's opinions to prove that Dr. Klein was an inhuman person, there is no end to it?—Well, my Lord, I submit that I am entitled to read this letter. I request to be allowed to read it. If I am refused I pass on.

10672. I think we ought not to have it. It is admitting a great deal of matter which is not relevant?—Then I pass from it.

10673. You say that Dr. Klein has had a certificate which he ought not to have had?—He has it now so far as I know. He had it in the last yearly report, and he has had it ever since 1884, I think. Then I wish to show, if I may, by citing a short passage out of an article by C. S. Myers, who is a man of great distinction at Gonville and Caius College, Cambridge, a first class man in both parts of the Natural Science Tripos, and the author of various articles in scientific journals, that the opinion of Dr. Klein, in which he said that he had no regard for the feelings of animals, did not expire in the year 1876, but is held by persons of distinction to this day. I do not know whether I may read it for that purpose. It is an article from a man of great scientific eminence, who writes quite recently to say that he endorses Professor Klein's opinion.

10674. I cannot see how that is relevant?—Very well. This is only to bring it up to date, so that it shall not be said that it is thirty years old. It is an article up to date endorsing those views that the sufferings of animals are of no account.

10675. There are a number of people over Europe, as we know, who hold those views?—He is an Englishman and a Professor at Cambridge at the present day. I come to my last charge. I am here to charge the Home Office officials with having placed themselves in improper private confidential relations with a private society composed of supporters of vivisection entitled to no more consideration than the National Society composed of opponents of vivisection. Mr. Byrne was asked at Question 27: "The Association for the Advancement of Medicine by Research is a body consisting of a great number of eminent surgeons, I suppose?" and he replies, "Yes, I believe it includes, nominally, at any rate, a large number of gentlemen of great distinction in this matter of investigation"—I suppose by investigation he means vivisection—"but no doubt the Commission will ascertain its precise composition from the body itself." Then we have had the assurance on several occasions, and we had it from Mr. Akers Douglas in 1903 that: "No licence or certificate was granted except on the recommendation of an eminent medical man, such as the President of the College of Physicians or the President of the College of Surgeons, or without reference to the Society for the Propagation of Scientific Research"; that is what he called it, it is a better name for it, almost, I think, than its real one. Then we have Mr. Byrne, who tells us that it is a purely voluntary organisation. He

is asked at Question No. 171: "It has no statutory recognition? (A.) That is so. (Q.) Can you tell us the name of its president or officers? (A.) Not at the present moment. (Q.) In every case, does the Home Office receive advice in regard to applicants for licences and certificates from that Association? (A.) Yes." So that it is a voluntary organisation of the promoters of vivisection, and it is consulted by the Home Office on the competence of vivisectioners; and I do not know why, as we are also a voluntary organisation, we should not be consulted as to their humanity. But we are not. Desiring to find out, when I saw this evidence, what this Association really was, I could not find its address anywhere. I wrote in the air, and posted my letter "To the Secretary of the Association for the Advancement of Medicine by Research, London." The Post Office always find anybody, and they found him, I suppose. The letter I wrote was as follows: "Dear Sir,—In view of the enquiry now being made by the Royal Commission, I shall be much obliged if you will let me have a list of the members of your Association and the last yearly report, if you have one."

10676. (*Colonel Lockwood.*) When did you write that?—On the 24th of December, 1906, as soon as I saw this evidence. This evidence was given in October, and in December I wrote and asked for their last yearly report. I got a reply from the Association for the Advancement of Medicine by Research from 135, Harley Street, W., on the 29th of December, 1906, and it is in this form: "In reply to the Hon. Stephen Coleridge's letter of December 24th, the honorary secretary of the Association for the Advancement of Medicine by Research writes to say that he is unable to comply with the request contained in the letter." You see this gentleman did not even sign his name. I could not even tell who he was, but I looked up 135, Harley Street in the Directory, and it was the house, I found, of Mr. Charles Edward Beevor, who, I presume, therefore—I deduced—is the honorary secretary of the Association for the Advancement of Medicine by Research. But he gave me no information—he would not give me the yearly report or tell me who anybody was.

10677. (*Chairman.*) He is a doctor, I suppose?—I do not know. I suppose he is. I therefore prosecuted my further researches to find out what this Association was. I looked up old files of the "British Medical Journal," and I found in the number for the 1st of April, 1882, on page 476, that Sir William Jenner, at a meeting, concluded by proposing the following resolution: "That, with the view of bringing the legitimate influence of the medical profession more effectively to bear on the promotion of those exact researches in physiology, pathology, and therapeutics, which are essential to sound progress in the healing art, an association be formed, to be called 'The Association for the Advancement of Medicine by Research.'" Mr. Spottiswoode, then president of the Royal Society, moved also, at that same meeting apparently, "That the Association consist of representative members of the medical profession, and of other persons desirous of promoting the above objects." That is how it was formed. Then I find that on the 8th of April, 1882, a few days later, Mr. Hallows, writing in the "British Medical Journal," on page 157, alludes to "The formation of a society to resist the opponents of scientific research," which, he says, "will be most welcome to the members of the British Medical Association." Therefore, he calls it a Society to Resist the Opponents of Scientific Research—that means us, I suppose, which is a misnomer, because we do not object to scientific research at all. Then in the "British Medical Journal" of the 22nd of April, on page 599, which is a fortnight later (I am doing this seriatim), I find a letter from the treasurer, who happened to be a Mr. Samuel Wilkes.

10678. What is this on?—I am showing you what the Association is, to which every application for a licence is referred.

10679. Could you not come to something which does show us what the Association is? All this history has very little to do with what the Society is now. This is the same Society that advises the Home Office?—Yes, this is the Society.

10680. But all this history of how you came to know what it is is immaterial?—I am only telling you what it is.

10681. I think you are telling us a great deal more than that?—This is a letter from a member of the provisional committee of it when it was first started, which contains these words, "The working physiologists"—that, of course, means the vivisectors—"of the three kingdoms have expressly stated that they do not desire (at least for the present) to attempt to abolish the Act, of which we are all ashamed, but to secure its being harmlessly administered. To speak with authority to public opinion, and to bring effectual pressure upon officials, needs other means than those which are suited to the arena of controversy."

10682. This is from a gentleman who is a member of the Association?—He is a member of the committee.

10683. In a letter written to the papers?—Yes; and he says that the object of this Association is to bring effectual pressure to bear upon officials—meaning the Home Office. Then the first report of the Council, dated the 29th of April, 1882, says that the first duty of the Association is "to promote those exact researches in physiology, pathology, and therapeutics which are essential to sound progress in the art of healing, and to remove any hindrances which obstruct these researches." Sub-committees were appointed to report (1) on the present hindrances to research due to the working of the Vivisection Act; (2) to report on the best ways in which exact researches in medical science (i.e., vivisection) can be encouraged"; and on the 13th of May we find a £500 prize offered for an essay on "The usefulness of vivisection as a method of scientific research, the conditions to be approved by the Association."

10684. Is not this rather beating the air? We all know that this Association is one that was formed for the advancement of research by vivisection, amongst other methods, and is very strongly in favour of it?—Yes.

10685. There is no doubt about that?—But I do not think that the Commissioners knew before that one of its objects was "to bring effectual pressure to bear upon officials."

10686. No, I am not aware of that. Is that a resolution?—That is what one of the provisional committee writes to the public press to say is one of their objects—"to bring effectual pressure to bear upon officials."

10687. In a large society, what one person who is a member of it writes to the press is not its best interpreter?—No, but further on you will see in the evidence that has been given before you that any licensed vivisector, on payment of 10s., may become a member of it, so that I think I am right in saying that this Association is an Association formed for bringing pressure to bear upon the officials of the Home Office, in order, if possible, to have the Act "harmlessly administered."

10688. Is there a resolution of the Society, or is there any printed rule of the Society saying that that is one of their objects?—My Lord, I have written to them in vain to get it, and I am thrown back upon this.

10689. I do not say that it is your fault that you cannot produce that rule if it exists, but certainly if that is the suggestion we shall have somebody from the Society (we have had one witness), and we will ask the question from him to ascertain it. But I think it is really taking up time unnecessarily to try and convince us that these gentlemen are all strongly in favour of vivisection—quite as strongly as you are against it?—Very well, if that be accepted, and that their object is to bring pressure to bear upon the Home Office—

10690. I do not accept that. I understand that somebody has written to the Press to say that that is his view.

(Sir Mackenzie Chalmers.) The pressure has not yet been felt.

(Witness.) Pardon me, I cannot accept that, if I may say so. I wish to point out in answer to that *obiter dictum*, that it is a fact proved before the Commission that the applications for licences are laid before this Association for the promotion of vivisection before they come to the Inspectors. Therefore the Inspectors receive these applications under prejudice, as they have already been before this Association of Vivisectors, before they reach the Inspector, who has to report upon them, and therefore it is put upon the Inspector.

10691. (Chairman.) I believe that is so; there is no dispute about that, is there?—Very well, my Lord, I

think that answers Sir Mackenzie when he says that no pressure is brought to bear. I call that most distinctly pressure brought to bear upon the Inspector. Having finished my charges, I wish just to say in conclusion that I hope I have shown the Commission that the Home Office have repudiated the duty of protecting animals from unjustifiable suffering; that they have constituted themselves the consistent injudicial defenders of the vivisectors, protecting them from the punishment which it is their duty themselves to see inflicted upon them; that they have appointed biased Inspectors; that they have given licences to men who have shamelessly and publicly proclaimed their personal inhumanity! that they have improperly allowed themselves, in our opinion, to be guided for years by a private society of vivisectors, formed for the very purpose of bringing pressure to bear upon them, if I may say so, to neglect that which they should do for the protection of animals; and I invite this Commission to acknowledge that I have laid before them ample evidence to show that the Home Office officials have for 30 years betrayed the solemn trust placed upon them by Parliament.

10692. The next point is the ethical ground upon which your Society objects to vivisection. I hope that you will place that before us very shortly; we have had it at extreme length already?—Perhaps the simplest way would be to submit myself to cross-examination on that statement.

10693. You say in a sentence that you think what is done is cruel?—Yes, I think that we have a right to kill animals, and that the duty is laid upon us in killing them to do so with as little accompanying pain as possible.

10694. Then you have in the course of your evidence already dealt with some of the statements made by previous witnesses. I do not know whether you have finished that part of your case?—I have some more statements of witnesses to deal with, but perhaps I had better keep that. I will do exactly what you like. I have the materials to go on with, but I will subject myself now to cross-examination on these charges or do anything you wish.

10695. You do at the end of your *précis* refer to certain alterations of the law?—Yes, I have my Bill.

10696. I think we had better deal with that, that is very much more practical, of course, than anything else?—I think perhaps it would simplify the discussion before the Commission if I were shortly to give you the alterations in the law which this Bill really would carry out.

10697. If you please?—I put them under 11 headings. The first alteration would be (1) the animal during the experiment to be under an anæsthetic of the nature of a respirable drug or gas.

10698. With regard to the present Act, is this draft which you have put before us by way of amendment to the Act, taking the Act as it is, and amending it?—Yes.

10699. It is not entirely a new Act repealing the present Act?—No, it is an amending Act. The second heading is (2) the animal to be killed before recovering from the anæsthetic. That is really re-instating the Act without Certificate B. The third, and this is really the important thing, is (3) the Inspector to be present throughout each experiment—each physiological experiment, of course, I mean; I do not mean experiments in the nature of inoculation. Provided that in the case of an "experiment not calculated to give pain from the act or operation by means of which the experiment is initiated." (By that I mean an inoculation experiment.) These three provisions shall not apply, but in lieu thereof (1) directly the animal feels pain it must be placed and kept under an anæsthetic, as above mentioned, or killed. (2) The Inspector shall frequently make surprise visits to all places where such experiments are taking place. The idea here is that the Inspector shall be present at all serious physiological operation experiments, but he is not necessarily present at inoculation experiments, which may, of course, last for weeks or months; but his duty will be to go at frequent intervals and inspect the condition of the animals which have been inoculated, and if they are in serious pain it will be his duty to order them to be killed.

10700. I thought you said that these three heads that you have given were none of them to apply to what you call inoculation experiments?—That is so, but in lieu of them there is this further direction that the

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Inspector is often to make visits, and have them killed if they are in severe suffering. Then the next condition is (4) the total abolition of the use of curare, and after that the next one is (5) no application of matter to the eye of an animal for the purpose of observation through the conjunctival membrane. (6) Abolition of certificates, and experiments depending thereon, of course.

10701. This is all confined, I presume, to experiments for the purpose of acquiring knowledge?—Yes. The rest of the Act stands.

10702. (*Sir Mackenzie Chalmers.*) You have dealt with lectures upon another point. You would prohibit all lecture experiments?—Yes, I am coming to that.

10703. (*Chairman.*) By providing that no application should be permitted to the eye, you did not mean that no oculist was to apply any application?—No, I mean for the purposes of experiment. Then 7. Licences to be granted only for one experiment or for one series of not more than six connected and consecutive experiments and must specify the time and place at which such experiments are to be performed and the nature of such experiments. (8) The licensee must make within seven days of completion of experiment a detailed chronological report in writing to the Secretary of State of the description, course, and result of the experiment, and is liable to penalties of £20 and £50 for first and subsequent failures to comply with these requirements.

10704. Within how many days?—Within seven days. Of course, I should have no objection to that period being lengthened, if it is found impracticable. I must put in some number of days. The next one is (9). That three copies of every licence granted by, and three copies of every report made to the Secretary of State shall be deposited respectively in the Library of the House of Lords, the Library of the House of Commons, and such other place accessible to the public as the Secretary of State may determine. (10) In the appointment of inspectors and licensees special regard is to be paid to the applicant's reputation for humanity, and (11) prosecutions may be instituted at any time within two years of the offence without the assent of the Secretary of State, the prosecutor, however, being bound to deposit the sum of fifty pounds as security for costs.

10705. (*Colonel Lockwood.*) To prevent any vexatious indictment?—Yes. Those are broadly the main provisions of the Bill.

10706. (*Chairman.*) Under No. 6, Abolition of certificates and experiments depending thereon, you mean the abolition of all certificates?—I meant Certificate B really. Certificate A remains.

10707. Do I correctly understand that that is what you put before as the minimum which your Society desires?—Yes, that is the Bill which I drafted which my Society supports, and which has been before Parliament with some small alterations for several Sessions.

10708. (*Colonel Lockwood.*) Are you able to differentiate at all between the ethical and practical views held by various people in this matter, or do you look upon the two as indivisible?—I look upon this question solely from the point of view of the animal. I do not deal with, and I do not address myself to the question of the utility of the practice of vivisection, if that is what you mean.

10709. What I mean is this. Do you hold the opinion that, if it was proved that certain operations lead to discoveries to alleviate human suffering, you would not object to them; or do you object to them altogether because you believe them to be immoral?—Not at all. My objection to vivisection begins and is centred in the question of pain. If an animal be placed under complete anaesthesia and destroyed before it recovers consciousness, personally I have no objection to that vivisection at all; and anything that might be discovered thereby would be to the benefit of humanity and welcomed by myself.

10710. We have had, as you know, various witnesses before us who refused to deal with what I call the practical side of the question, and dealt entirely with the ethical side. I will not go so far as to say that you dissociate yourself from them, but that is not the opinion that you hold?—I ought to say that I am not against the total abolition of vivisection, because in my opinion the total abolition of vivisection would be the most certain way of preventing any possible torture to animals and saving them from all possible

pain; but as it is the pain that I object to, if the pain be really and truly eliminated from vivisection I personally have no objection to it.

10711. In the beginning of your remarks you said that you regretted that the Press were not admitted here because much that the public have a right to know (I think that is about it) would not be known by them?—Yes.

10712. Would you tell me and the Commission exactly what was in your mind when you said that in view of publishing the evidence?—First of all I should have liked to have heard the debate which was conducted among the Commissioners on the question of the introduction of the Press and the permission of myself and others to be represented by counsel. I think that would have been very illuminating to the public.

10713. You mean the preliminary talk of the Commission?—Certainly. On what ground it was that the Commissioners determined to refuse my application to appear by counsel.

10714. You alluded in the early part of your evidence to Queen Victoria's opinion being against vivisection?—I have Her Majesty's letter here. It was sent to the secretary of the London Society who kindly let me see it.

10715. (*Chairman.*) Is that from Her late Majesty?—It is from Sir Henry Ponsonby on her behalf. It is as follows:—"Windsor Castle, May 8th, 1889.—Sir,—I duly laid all the documents forwarded by you before the Queen, who was much interested in the report of 1888. The object of your Society is to effect the total abolition of vivisection, by which, of course, is meant the removal of laws which sanction it. It would be impossible for the Queen to take any part in petitioning Parliament or herself to pass new laws, and therefore, while heartily sympathising with your efforts, Her Majesty is compelled to decline the office of patron.—I am, Sir, your obedient servant, HENRY PONSONBY. The Secretary, the London Anti-Vivisection Society, 32, Sackville Street, Piccadilly, W."

10716. (*Colonel Lockwood.*) In the beginning of your evidence you alluded to the gross inhumanity of the medical profession, I think?—No, I never said that.

10717. I wanted to make quite sure. I made a note at the time, being very much struck by it?—I know what you are thinking of. I was very careful of what I said.

10718. Here I have it at Question 10262. "We are prepared to show that under the existing law there is no limit to the agony that may legally be inflicted upon animals, and we abominate a Statute that authorises gross inhumanity"?—Yes.

10719. Could you elucidate that a little for my information?—Yes; that is a plain statement of fact. under the present Act there is no limit. If a man has Certificate A, there is no limit to the agony he can inflict. I do not say that it does happen. I say that under the present law it can happen. I am not saying for a moment that at the present time provisions are not added to that Certificate which may prevent it under the law. But the Home Secretary might be a vivisector.

10720. When we were talking about Dr. Bayliss's operations not having been performed under an anaesthetic, would not the animal have cried out if it had not been under an anaesthetic?—I never said that. What I said was that "The anaesthetics during these prolonged and fearful mutilations can legally be applied by an automatic pump in another room."

10721. Would it not have been the case that if the pump had not acted properly and the dog was not insensible, there would have been some evidence of pain on its part?—Yes, I have no doubt, but then there was evidence that it did struggle, and the vivisector said it was St. Vitus's dance. They admitted the struggle, but said it was St. Vitus's dance. I do not know what they call it, but it was St. Vitus's dance.

10722. (*Mr. Wilson.*) Chorea?—Yes, chorea.

10723. (*Colonel Lockwood.*) At Question 10,264 you go into the question of fact, that the inspectors appointed by the Home Secretary have no testimonial as to their humanity?—I do not think I said the inspectors, did I?

10724. "Nor the inspectors appointed by the Home Secretary, testify to the personal character or humanity of the applicant"?—Yes, of the other people. I said nothing about themselves.

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10725. Yes, it is the applicants. How are you going to meet that in your Bill?—I say that in the appointment of inspectors special regard is to be paid to the applicants' reputation for humanity. I say that certainly men like Dr. Klein would not get a licence under my Bill. His reputation is the other way.

10726. I mean how could you possibly arrive at getting a humane character for anybody excepting from an interested party?—I think it might be done.

10727. It seems to me very difficult?—I think inquiries might be instituted about a man's personal character, his character for humanity.

10728. You think by your Bill you could work it?—I do specify it. I say "a certificate recommending that the application be granted on account of the applicant's reputation for humanity, which must be signed by two or more of the following persons. That is to say, a Justice of the Peace, a minister of any religious denomination in the United Kingdom." That is as near as you can get.

10729. That you think would be sufficient safeguard?—Yes.

10730. And guide to humanity?—As much as one can expect.

10731. And you think that would ease the public mind as to the humanity of the person who got a licence?—Yes, always provided that the Inspector was present at every vivisection. That is what I look upon as really comforting the public mind.

10732-3. The presence of the Inspector invariably?—The presence of the Inspector invariably at serious cutting operations.

10734. In answer to Question 10265 you say, "It will be observed that my Society has been forced to the conclusion that, by placing the interests of the animals in the hands of the Home Secretary the framers of the Act of 1876, unwittingly, no doubt, made a fatal mistake. The Home Secretary has practically delegated much of his responsibility to the Home Office officials, who have, in their turn, delegated their responsibility," and so on?—Yes.

10735. You will acknowledge, I suppose, that the Home Secretary himself, beyond being, of course, the authorised responsible minister for these things, must delegate his work to somebody?—Yes.

10736. To whom would you delegate it?—I say that under the safeguards of my Bill, if it is passed, first of all any person may prosecute without leave of the Home Secretary, provided he is a serious person, and puts up £50 to cover the costs. That, in my opinion, takes the protection of the animals very largely out of the sole care of the Home Office, and allows such a society as mine, or any serious society, to do what they can to safeguard animals in the event of any breach of the law. But at present, I say it deliberately, I have no reason to suppose that any illegal act in the way of torturing an animal or anything of that kind has ever taken place hitherto in the presence of a Government inspector. I therefore rely (I am not a fanatic), upon the presence of a Government Inspector to protect the animal from illegal treatment.

10737. Do not you think that if this power was placed in the hands of practically anybody to prosecute, it would lead to a certain amount of carelessness on the part of the real official of the Home Office, the Home Secretary, because he would say, "Well, after all, anybody in the street, or in a society can take up this matter; I need not be extra careful on the subject"?—I do not think that. I think we can go by the analogy for instance of the children's society, of which I sit on the Council. In practice it is the society that institutes prosecutions; but therefore because the society institutes prosecutions I do not think (I have never observed) that it makes the police any more remiss in their readiness to assist or carry out the law.

10738. In answer to a previous question you said that you were not against experiments, provided they were not cruel to the animals?—That is so, as long as there is no torture of animals I do not object to it.

10739. And you do not dispute the fact that there have been discoveries made by these experiments which have been of use to human beings?—That is a question for experts.

10740. You would not like to express an opinion?—No, neither one way nor the other. What is my opinion against the aggregate opinion of the scientific men of this Commission? I do not set it up for a

moment. Perhaps I might add, to elucidate that position of mine, that with regard to the results of vivisection, I see no objection to human beings benefiting by the results, provided that those results are not continually produced by fresh infliction of pain, and serious pain.

10741. You have probably read over Professor Starling's evidence?—Yes, I have read it through.

10742. You will have noticed, of course, that, as he would be expected to do, he traverses the various statements made by Mrs. Cook and various other people who hold anti-vivisection views. Have you any remarks to make upon the statements which he makes as traversing those statements?—No.

10743. In one part I remember he said, "That is absolutely false." For instance, he said of one of the witness's statement at Question 3672, which refers to some of Mrs. Cook's statements about cruelty to animals, "these scalped animals with mutilated nerves were allowed to live and then they were again anaesthetised. This is entirely false." Of course, I had read this statement of Mrs. Cook's before, and I confess to having been considerably horrified; but then, when I come to a skilled witness who says "This is absolutely false," it leaves rather an unpleasant impression upon my mind that I have been misled. Have you formed any opinion about it?—I have formed the opinion that as neither Mrs. Cook nor Professor Starling were present when these experiments were performed, they are both equally at liberty to form their own conclusions from what they have read.

10744. But you see Mrs. Cook's account was published to the public, and this evidence, of course, is published to the public, but the public will not necessarily hear of it?—I should like to go back if I am to be asked this, to see what this was. I do not know what Mrs. Cook was talking about at the moment.

10745. If you would just look over it?—I do not know what it is. If this quotation, "for three or four weeks these scalped animals with mutilated nerves were allowed to live," is a fact, it sounds rather horrifying. That is a question of fact. I do not know what Mrs. Cook is quoting from. I would rather she dealt with it herself.

10745. If you would just look over it?—I do not wish to ask you questions about which you have no personal knowledge?—No, I have no knowledge of it.

10747. (*Colonel Lockwood.*) Professor Starling says at Question 3772, that his view of experiments on living animals is "That science is advanced by experiment; that physiology is a science of living things, and that physiology will therefore be advanced by experiments on living things." Would you give me your answer to that?—I presume that, other things not being considered, physiology would be advanced and might be advanced at any rate by experiments on living animals, but whether physiology thus discovered is of any service to mankind is another question.

10748. (*Chairman.*) That is a point upon which I understand you do not wish to express an opinion?—That is so.

10749. (*Sir William Collins.*) I do not know that I have many questions to ask you in reference to your statement in regard to the Home Office. Do I correctly understand that your Society approves of the draft Bill, which you have laid before the Commission?—Yes.

10750. Then their legislative project is rather in the direction of amending than repealing the existing Act?—Yes.

10751. And though you might perhaps as a counsel of perfection desire abolition, I think your Society and you would be content with this amending Bill?—We think that to bring in a Bill for the total abolition of all vivisection in the present condition of public opinion would be a waste of time, and that meanwhile we think much good might be done by such a Bill as this to protect animals more than they are protected at present. And we should like to see how this Bill works if it is passed, if it ever becomes law.

10752. From a hasty perusal of your Bill, I do not see any provision in it for certificates such as those obtained under the present Act?—We leave Certificate A alone, but not certificate B. Certificate B is practically abolished in the sense that you must not allow the animal to recover. You may vivisect it as long as it is kept under anaesthetics and destroyed before it recovers consciousness, under my Bill.

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10753. Do you make any distinction as to what animals should be used under your Bill?—No.

10754. Provided that the conditions of your Bill are carried out, you do not object to the use of dogs, cats, and monkeys?—I do not want to make any distinction between vertebrate animals.

10755. Have you formed any opinion as to the number of inspectors who would be required if your Bill became law?—Yes, if the Bill became law my hope would be that, instead of persons now vivisectioning all over the country, when they had a serious object which would be recognised by the Home Office as a serious object, it would be worth their while to come to some central place, there to perform the series of operations under the eye of the Inspector which they wished to do; and I should look to a large reduction in the number of registered places, thereby saving the time and the travelling of the Inspectors. I see no reason for a very large increase in the number of Inspectors, provided that their whole time is occupied in inspecting, and provided that the places where they inspect are not too far distant from one another.

10756. Is there anything in the Bill which limits the number of registered places?—No.

10757. I do not think you told me the number of Inspectors that you contemplate?—No. I leave that to the exigencies of the case. At present the two Inspectors do not give their entire time, and inasmuch as they are not required to be present at every vivisection, they spend a good deal of their time in travelling about and seeing nothing, whereas under my Bill their services would never be wasted; wherever they were there would be a vivisection going on for them to inspect, so that an enormous saving of time would be made under my Bill.

10758. You set a good deal of store on inspection?—Yes, I do absolutely. Inspection is the whole thing in my Bill. In my view the present Act contemplates the necessity of inspection, but does not provide that it shall be efficient.

10759. On the ethical ground I understand that you direct yourself against the infliction of suffering, but you do not object to the utilisation of animals for scientific purposes, provided that it is painlessly conducted?—Quite so.

10760. Is it in your opinion immoral to use knowledge which has been obtained as the result of painful experiments on animals?—No, no more than it would be illegal or immoral for me to enjoy the results of a fortune accumulated in the slave trade by my great-grandfather. I need hardly say that he did not accumulate any such fortune, but if he had I should feel myself perfectly justified in enjoying that fortune. Although I should condemn the slave trade, and should certainly condemn its continuance now, or its resuscitation in any shape or form, I do not think it would necessarily morally preclude me from enjoying the results of the fortune. Nor do I think if any valuable invention has been discovered by painful vivisection of animals we are thereby precluded from using it.

10761. I understand that you do not contest the position that useful knowledge has been acquired by painful experiments upon animals?—That I neither admit nor deny, not being an expert.

10762. I do not know whether you are contemplating giving further evidence or whether you have dealt with that heading in your evidence with regard to statements by previous witnesses, dealing with the value of vivisection, both in physiology and in therapeutics?—I rather reserved myself for cross-examination upon that. Of course, though I assert nothing as to the usefulness of vivisection, as an ordinary layman it is open to me (as a layman) to criticise, if I can, any statement that is brought forward asserting great benefits to mankind in the way of lowering the death-rate from any particular disease. If the vivisectioners were to claim that they had lowered the death-rate from any particular disease, that is a statement that, as a layman, I am as qualified to criticise as the greatest expert; and therefore in any case of that kind I should certainly not preclude myself, but simply for what it is worth I should examine the death-rates at Somerset House and think myself not precluded, though I am not an expert, from bringing them before the public.

10763. There are many previous witnesses who have called our attention, for instance, to the value of antitoxin as a mode of treatment for diphtheria, indicating that it has been discovered as the result of experi-

ments on living animals, and that the serum is now obtained by a process involving the use of living animals. Have you any remark to make upon that evidence?—I have looked into the death rates from diphtheria before and after the introduction of antitoxin.

(After a short adjournment.)

10764. (*Sir William Collins.*) I think I was asking you whether you desired to say anything with regard to the claims which have been made by previous witnesses, as to the beneficial effects upon the diphtheria mortality arising from discoveries due to vivisection?—I have made a table here from the death rates per 1,000,000 persons living, and I think the fairest and best way to do it is to group it in quinquennial periods so as to avoid the slight inequalities of various years. I begin with 1876, and I come down to 1905. In the first quinquennial period, from 1876 to 1880, the average mortality per 1,000,000 persons living from diphtheria was 121; in the next quinquennial period it was 156; in the next 170; and in the next 253. That quinquennial period reaches down to the year 1895, and the "Encyclopædia Britannica" tells us that the antitoxin treatment began in the latter part of 1894; so that it falls just at the end of that quinquennial period. In the two quinquennial periods subsequent to the introduction of antitoxin in the first the death rate is 272, and in the second it is 204. That comes down to 1905. So that both the averages, taking the last two quinquennial periods covering ten years since the introduction of the antitoxin, are considerably higher than in the period from 1876; higher than the three periods during which the antitoxin was not used. I do not set any great store by that, except that there is the fact, when they claim to have cured this disease by this particular remedy, that the death per 1,000,000 persons living, appears to have gone up.

10765. May I revert once again to your draft Bill, and may I ask you how you deal with cases under that Bill, should it become law, in which animals have been inoculated with various morbid products with a view to ascertain the pathological effect?—Under my Bill if an animal is inoculated with a substance that creates a painful disease in it, it will be the duty of the Inspector to go and see that animal frequently, and if it be in a severe condition of suffering it will be his duty to order it to be killed painlessly.

10766. Of course in some cases, I suppose the painful symptoms might be expected to supervene somewhat rapidly after an inoculation?—As to that I presume that as the Inspector will probably be a man of some experience in these matters, and as he will know what the inoculation is from information sent in by the vivisectioner, he will know whether the painful period is likely to supervene early or late; therefore it will be within his power to protect that animal from unnecessary suffering.

10767. Do you think, under the provisions of your Bill, and with the inspection it provides, vivisection could be carried on without inflicting suffering upon animals?—I should hope so, yes. That is my hope.

10768. (*Sir Mackenzie Chalmers.*) I think you told us that you have never seen an experiment yourself?—No.

10769. And you never wish to see one?—No.

10770. And you have not any intention of seeing one?—No.

10771. Have you ever seen any animal after an experiment in a physiological laboratory?—No, I am wholly unacquainted with the thing from personal knowledge.

10772. Your knowledge is derived from reading?—Absolutely, and from what I have been told.

10773. Have you had communications with licensees, and discussed the question with them?—I know licensees; I have met them and talked to them.

10774. Do you accept their statements?—That is rather a large question.

10775. I will not press it?—It depends upon the man.

10776. I take it that your position is, that it is not what has been called the exploitation of animals that you object to, but the infliction of pain?—Yes, broadly that is it.

10777. Does the question of the amount of pain enter into your consideration?—Yes.

10778. Let us take one or two instances. You know that at present we are losing a million lives a year in India from plague?—I did not know that.

10779. And at home and in India certain experiments have been carried on to test the mode of transmission of plague—experiments on rats and guinea-pigs?—Yes.

10780. Do you think that these are justifiable, or not?—It depends entirely upon the amount of pain involved.

10781. The amount of pain involved in these experiments is to inoculate an animal with plague, which sometimes is a painful and sometimes not a very painful disease. Do you think it is justifiable?—Under my Bill it would be the duty of the Inspector to have that animal destroyed as soon as it manifested signs of severe suffering.

10782. You would not prohibit the experiments?—No, they would be permissible under my Bill.

10783. Take the case of the cancer research which is now going on. You agree that if any further information as to the nature or causation of cancer can be obtained, it is most essential?—Yes, but they have been some years at their researches now, and they have placed an almost inconceivable number of animals under test and under experimentation, and so far they have had no result whatever for the benefit of mankind; if the death rate per million persons living is to be any test at all.

10784. Do you think those experiments and the continuation of those experiments unjustifiable?—I should think it is about time that some period should be put to the numbers and the quantity of suffering that is inflicted for that purpose.

10785. Do you think that the induction of cancer in mice, which are chiefly used, is a painful experiment?—That is a matter for experts. If it be painful, I say it should be dropped as soon as the animal shows signs of severe suffering; it should be destroyed. There are cancers I believe in the human family which are not painful and some that are. I presume it is probable that it is the same with animals. I do not know.

10786. Then take the case of vaccination. The calf from which the vaccine is prepared suffers no doubt a considerable amount of inconvenience if not severe pain. Do you think that is justifiable or not?—I am an anti-vaccinator on the ground of the liberty of the subject quite apart from any other question.

10787. I am not dealing with compulsory vaccination?—I thought you meant that.

10788. I am dealing with vaccination which people are at liberty to adopt or not. We must have vaccine if people are to be voluntarily vaccinated?—Yes.

10789. Would that come outside your Bill or not?—That would not come under the purview of my Bill. That is a trade product you may call it. It is not experimentation for the purpose of scientific research; and my Bill only covers experimentation for the purposes of research.

10790. Any improvement in vaccine I suppose would be a matter of experiment?—Yes. Vaccine has been going some time. I presume it is a well-known product, is it not?

10791. Then to take the antitoxin for diphtheria, you were going to say something about that. Do you object to the preparation of antitoxin for diphtheria?—That I presume is also a trade product. It would not come under the purview of my Bill.

10792. I am not quite sure, but has not the antitoxin to be standardised by means of fresh experiments?—I do not know. I thought it was like vaccine, a thing that is now ascertained and is produced like a trade product. I thought you could buy it by the bottle.

10793. A good many experiments, as you know, are authorised for the purpose of standardising some of these preparations which cannot be standardised by chemical means?—If the Home Office issue licences for the purpose merely of what you may call standardising that is not an experiment, nor a new discovery; I consider that in issuing that licence under the present law there is no question of a new discovery there; therefore, I think such a licence is absolutely outside the Act, and ought not to be granted.

10794. If we are not justified in causing pain to animals for the purpose of alleviating human suffering and the advancement of knowledge, are we justified in

killing and maiming animals in sport?—I am not a sportsman myself; personally, I have never shot a bird. *The Hon. S. Coleridge.*

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10795. I was not accusing you of being a sportsman; I was asking you whether, if the law deals with one, it ought not to deal with the other?—It does not seem to me to come within the purview of my Bill, but I am ready to give my own opinion, if it is of any interest.

10796. I want to know the view that your Society takes on this question?—My Society has no opinion on that matter; it is simply a Society for the prevention of vivisection, an anti-vivisection society. It has nothing to do with anything else. I should be absolutely going outside my rights and committing a breach of trust as regards its many subscribers if I devoted any of its funds for agitation against anything else.

10797. But may we have your private opinion; you are a very important member of your Society, and I should like to have your private opinion?—My private opinion is that you have no right to inflict unnecessary pain on animals in killing them, and therefore if any sport inflicted unnecessary pain on animals in killing them, I object to it myself personally. I should not do it myself. I do not bring any accusations against others who do, but I should object to it myself, and, so far do I object to it that I have done my best strenuously to support the Spurious Sports Bill now before the House of Commons.

10798. Take, for instance, rabbit shooting. No doubt a rabbit can be killed in a painless way, but in a good day's rabbit shooting a good many rabbits get away maimed, and die in their holes?—I never went out for a day's rabbit shooting, and I do not know.

10799. I was wondering whether it is a consistent attitude that you take up?—My consistency is perfect, because I do not shoot. I have never even hooked a fish. Nobody can say anything against my consistency. I can speak for myself; you asked me only to speak for myself.

10800. Is it a consistent attitude for your Society to take up, only to deal with one form of pain to animals, leaving probably infinitely worse forms of unnecessary pain untouched?—There is the Royal Society for the Prevention of Cruelty to Animals, whose purview is the whole cruelty performed in the world in all forms to animals of all sorts. My Society has taken up one particular form of cruelty to animals, and devoted themselves to it alone, because in their opinion the Royal Society for the Prevention of Cruelty to Animals has neglected it.

10801. You specialise?—Yes, and we think we can do it better than the Royal Society for the Prevention of Cruelty to Animals, even if they did do it, because we have devoted out entire attention to it.

10802. Colonel Lockwood called your attention to your evidence. Your Society publishes detailed accounts, does it not?—What of?

10803. Of its income and expenditure?—Oh, dear, yes.

10804. Do you know whether the other societies that you referred to published detailed accounts or not?—I think I have most of them here. I asked them all to send me their annual reports. I think they are here, if you like to see them.

10805. (*Dr. Wilson.*) You gave the annual incomes?—Yes; there they are, such as I have got (*handing in the same*).

10806. (*Sir Mackenzie Chalmers.*) I was not quite sure of the object of your statement in comparing the income of your Society with that of others?—My object was purely egotistical, to persuade the Commission, if I could, that mine was the most important Society in the anti-vivisection world, and to claim greater attention for my evidence than for that of small obscure societies which have hardly any address and no income.

10807. The point which occurred to me is this: What we have to judge of is not the income of the Society, but the validity of your arguments?—Yes, quite so. I hope mine have been more valid than those proposed by other societies.

10808. The smallest society might send a very important witness?—Yes; but every other society except mine, so far as I know, confines its attention solely to advocating the total abolition of any vivisection whatever, and they all differ from my Society, and somewhat

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acrimoniously on occasion, because we do not confine ourselves to that utopian policy, but are anxious and ready to advocate anything we can for the amelioration of the condition of vivisected animals. And I think it is a very valid argument in favour of our policy that the vast majority of anti-vivisectionists of England support us, and not them. That is why I brought those figures before the Commission.

10809. It will be convenient perhaps on that point to take some of the points in your Bill. Taking Section, Sub-section (3)?—I must apologise for the language "prior to." I do not like it; but that is prepared by a draftsman.

10810. "The animal must, prior to the commencement, and thenceforth during the whole course of the experiment, be under the influence of a general anæsthetic of the nature of a respirable drug or gas, such as chloroform or ether, or alcohol, chloroform, and ether combined, or sufficient power to prevent the animal feeling pain." That would abolish the present certificate B, would it not?—Yes.

10811. Because an experiment under certificate B consists first of the operative procedure, and then allowing the animal to recover from the anæsthesia?—Yes.

10812. That you would prohibit altogether?—That is my desire.

10813. The effect of that would be to prohibit Professor Starling's work on the digestive juices?—If you say so, I accept your statement.

10814. What he told us was that in what he considered important work on the digestive juices, the animal was put under chloroform, was operated upon, and a drainage tube was put in, and after the animal recovered, at stated intervals the digestive juice was drawn off. Those experiments would be absolutely prohibited?—It takes a long time to do that, does it?

10815. Weeks perhaps?—Then certainly I would prohibit it.

10816. Although he said that the animal was suffering really no appreciable pain?—I do not feel myself bound to take his statement on that point.

10817. The practical result of your Sub-section (3) would be to reproduce the existing licence and abolish Certificate B?—Yes. Certificate A I retain to a certain extent. Certificate B would be abolished.

10818. In fact, you draw no distinction between an experiment and an initial operation?—No, I think not. Our view, of course, is that it is under Certificate B that the animal has the greatest risk of suffering under the present law.

10819. Then by Sub-section (5) you would prohibit all demonstration experiments?—Yes, I should, on another ground besides on the ground of the animal, though I do not particularly object to it on the ground of the animal—that is to say, the animal is under complete anæsthesia, and destroyed before recovering consciousness. I think, on the whole, those conditions are more likely to be carried out in the presence of a lot of people than in the solitude of a laboratory. But I do think, from the evidence before this Commission, and from the evidence brought before us, that it does not tend to edification. I do not think it is right that young people should witness these things if they can be prevented. I do not think it is good for or tends to the edification of youth to witness these operations upon animals.

10820. Do you draw any distinction between witnessing operations of that kind on an animal and an ordinary surgical operation?—Yes, I draw a very large and grave distinction. In my opinion, when students go into the theatre of a hospital, what they are witnessing there is a beneficent operation undertaken with gravity by the surgeon for the benefit of the patient who lies there, and a totally different atmosphere of feeling must be present generally from that which obtains in a laboratory, where the simple thing is to find out whether the spleen does this and the blood pressure does that, and has no relation to the benefit of the animal of any sort.

10821. In fact, you disagree with the surgeons who have been here and given evidence on this question?—I decline to put myself up against anybody else. That is my opinion, and the reason I hold it. It is a reason which I hold with a great number of the persons whom I represent.

10822. Experiments should not be performed, you say, for the purpose of obtaining manipulative skill. That is the existing law?—Yes, I think it is.

10823. "The substance known as curare shall not be used or administered for the purpose of or during the experiment"?—Yes.

10824. Curare, if used now can only be used, you know, when a general anæsthetic is given?—It cannot be used, of course, under the present Act as an anæsthetic.

10825. We have been told that for certain investigations curare is essential; but you say that those investigations must stop?—My objection, of course, to curare is one which you must appreciate; that the most careful inspector cannot tell, if the animal has curare in a certain quantity injected into it, from any manifestations on the part of the animal when the real anæsthesia passes off. That is the sole reason I object to it. It is for the protection of the animal. It is impossible to protect an animal from the extremest torture if curare is permitted to be used at all.

10826. That is to say, the animal would make no manifestation; but it is quite possible to keep up the anæsthetic certainly and surely while the animal is under curare?—No, not certainly and surely. No one can tell whether an animal is suffering if it is under curare, except by manifestations which it makes itself of sensation.

10827. Not by knowing the amount of anæsthetic that is given?—You cannot know the amount of anæsthetic that is given from a bottle in another room. There may be a mistake, and every animal differs from every other animal. One animal will take more anæsthetic than another, and I should have thought that you yourself would have recognised the extreme difficulty in protecting an animal from pain if it has had curare as well as another anæsthetic. I feel sure that you must.

10828. I cannot give evidence, but I put to you this question. If a person who is skilled in giving anæsthetics gives the anæsthetic properly, surely there is no danger then?—What does the most skilled anæsthetist in the world do when he is administering anæsthesia to a patient? He does not merely watch a bottle and see how much he administers; he watches the patient for signs of life, and sensation, and it is by the signs of life and sensation in the patient that he is guided. I am sure that any anæsthetist would tell you that.

10829. Is not that for another purpose? Is not he afraid of the patient dying?—Very likely, but he is also afraid of the patient suffering, I hope.

10830. Perhaps some medical witness will deal with that. Coming to your Bill again—this refers to Certificate A: "Directly the animal begins to feel pain it must be placed, and thenceforth while it suffers or is likely to suffer, must be kept under the influence of a general anæsthetic of the nature of a respirable drug or gas such as chloroform or ether, or alcohol chloroform and ether combined, of sufficient power to prevent the animal feeling pain"?—Yes.

10831. "Or if the animal is not so placed and kept under the influence of such anæsthetic as aforesaid, it must be forthwith killed"?—Yes, it practically comes to its being killed.

10832. That would prevent experiments which have the effect of producing any painful disease where it may be necessary for the experiment to let the disease run a certain course?—Of course, if it is a painful disease, yes.

10833. For instance, plague?—Yes, if it is a painful disease. I believe there are some forms of cancer, as I say, which are not painful—that people have it and suddenly die, never knowing that they had it. I have a cousin who died of it and never knew it.

10834. It would be rather difficult with an animal, would it not, to tell whether cancer is painful or not. Animals show very little sign of pain in case of chronic diseases?—I did not know that; I should have thought that they did. That is for the inspector to decide. If he says that an animal is not suffering at all, I am ready to take his word for it.

10835. You would stop any experiment?—Take such a thing as tetanus—that is what my eye is upon. If you inject tetanus into an animal it gets dreadful sufferings. I think that such an animal should either be placed under anæsthetics when it begins to manifest severe signs of suffering, or be killed.

10836. It struck me that your clause goes a great deal further than the actual cases you had in view?—I hope it will stop all severe suffering. That is the object of my Bill, and that is the object of those whom I represent, to stop suffering if we can—a laudable object, I should have thought.

10837. This will stop not only acute suffering but any suffering?—You cannot have a measure of pain. There is no such thing as a pain gauge after all. I have made it as wide as I can because I want to protect animals as much as I can. You see why I have done it—to protect animals as much as possible.

10838. Not being concerned, of course, with scientific results?—In no way whatever. If science can be advanced contemporaneously with painless vivisection I am glad to hear it; but I do not see why science should be advanced through suffering any more than any art. You would not allow any art to be advanced by suffering, or any other pursuit of any kind. You would not allow astronomy to be advanced by suffering. Why should you allow physiology to be advanced by suffering?

10839. I only wanted to get your view. There is no question of proportion. If any suffering is caused you would stop even what skilled people say is a most useful investigation?—I am not an unreasonable man. When I say suffering I mean real severe suffering.

10840. Would discomfort incidental to certain diseases come within your definition?—If the discomfort amounts to what is really serious, so that the animal is utterly wretched, I say that the animal ought to be killed.

10841. I think your clause goes beyond that?—I consider that the poor dog at Khartoum in that picture reduced to a hideous condition of emaciation, is cruelty. I think that animal ought to have been destroyed.

10842. I have not seen it?—There was a photograph of it, and therefore it must be true—at least I suppose so.

10843. Your third clause says: "There shall not be applied to the eye of an animal by way of experiment any matter or substance for the purpose of absorption through the conjunctival membrane." That would prevent, of course, the injection of cocaine?—It is not meant for that, of course.

10844. I am only taking the words?—I have seen books and pictures of wretched creatures' eyes in a hideous condition, and no doubt you have vivisectors who do use the eye, I believe, as the place for cultivating these dreadful germs, which produce a terrific state of suffering.

10845. I do not know whether your object was to prevent cocaine being used as an anæsthetic?—No, that is not the object of the clause; it is to protect the eye from being used as the nursing ground for bacteria.

10846. (Chairman.) You do not say there "any matter or substance likely to cause pain"; you say "any matter or substance"?—I daresay that is an error on my part; that is my intention.

10847. (Sir Mackenzie Chalmers.) I see what you mean. Then in Clause 8 you mention the persons who are to sign these certificates?—That is the old Act.

10848. Have you put in any obligation there that the names of the persons should be disclosed?—No, but I will now you mention it. I think that is most important. I quite agree with that.

10849. Under the existing law there is no such obligation?—I am not quite certain about that. I think the name certainly must be on the certificate.

10850. Yes, on the certificate?—That is all the clause says here. There is no obligation for you to publish a yearly report at all in the Home Office, so far as I know.

10851. Would it not be rather inconvenient for these people, who may be continually shifting and taking each other's place, to require their names to be published?—No, I think that anybody who signs a certificate ought not to be ashamed of having his name published in your report. Nobody looks at it except humanitarians.

10852. What is the object of having the names published?—I think that a man would think twice, or even perhaps three times, before he signed such a serious matter as a certificate exempting a man from using anæsthetics, if he knew that his name would be known to everybody as having done it. That would

not preclude him, if he thought that the applicant was a proper person for a certificate; but I think it would make him a great deal more careful if he knew that his name would be published. I do not think that anybody would have been found year after year to sign a certificate for Dr. Klein if he knew that his name would be published for the world to see.

10853. On that, Dr. Klein has done nearly the whole of his work for the Local Government Board, has he not?—I do not know. If you say so I accept the statement.

10854. And under the instructions of the medical officer for the time being of the Local Government Board?—I suppose he is not the only person who could do that work.

10855. You are asking me a question that I cannot answer?—I think that some one else would have been better than a man who had proclaimed himself in public as inhumane, as he has.

10856. Are you aware that at the time when he gave that evidence he was, at least he said he was, somewhat imperfectly acquainted with English?—Yes.

10857. And that he asked to correct his evidence?—I know that, and I know also that the Royal Commission declined absolutely to have anything to do with his corrections.

10858. They published both his evidence and the corrections?—Yes. If you like to have this out I will give it you. Anything more condemnatory of his whole conduct cannot be conceived, in my opinion, than the whole thing. I have it here. Here it is, Dr. Klein's letter to the Secretary: "Dear Sir,—I return you my evidence corrected. I may mention to you that when under *vivâ voce* examination, the fact of my being a foreigner made me often not able to appreciate all the purport of the questions which were asked of me, and that therefore my answers were not always such as I would have desired to give if I had quite understood the questions. In revising the evidence I have endeavoured to remove the effect of any such misapprehensions.—I am, etc., E. KLEIN." Mr. Baker, the Secretary, replied: "Sir,—I beg to acknowledge the receipt of your letter of the 16th instant, together with the proof of your evidence, and to reply that, as your corrections appear to me to have exceeded the latitude usually allowed to witnesses, I must lay it before the Commission, and take their directions as to the course I am to pursue. I am, etc., NATH. BAKER (Secretary). P.S.—I must ask you to return me the proof I originally sent you, which you damaged, for, as I told you when I sent you the duplicate, I am answerable for all the copies that are supplied to me." Then Mr. Baker writes again: "In my letter of the 12th inst., in which I forwarded you a second proof I told you that I must have the original proof back. On the 17th instant I again asked you to return it to me. I regret that I must now formally request you to return it to me at once." Dr. Klein now replies: "Dear Sir,—I am very sorry to have given you so much annoyance, as it appears from your letter. As I have stated on the occasion of my asking for a second proof of my evidence, the first proof has been accidentally damaged so much that it was quite useless. I therefore destroyed it, and am not in a position, consequently, to comply with your request. I beg again to apologise if I should have put you to any inconvenience.—Believe me, etc., E. KLEIN." To which he gets this answer: "Sir.—Your letter of the 16th November has been considered by the Commission, with the proposed alterations of your evidence. These appear to the Commission to be so much at variance with the letter and the spirit of the answers you gave at your examination, that the substitution cannot be permitted. The Commissioners direct me to say that they observe with regret that after my letter of 12th November you should have destroyed the proof I originally sent you, and requested you to return. If you desire it, the Commissioners will include in the appendix your letter and the proposed alteration, with the correspondence which has passed with reference thereto." And they did it. In these circumstances, as I have been asked to produce that correspondence, I once more ask to be allowed to read Professor Huxley's letter to Mr. Darwin.

(Chairman.) I do not think that reading what you have now read makes any difference on the question of Professor Huxley's letter.

(Witness.) I only ask to read it, and it is refused.

10859. (Sir Mackenzie Chalmers.) We have not had Dr. Klein's corrections. I do not know what he wished

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to say?—He watered it all down of course, but I should have thought that as the Commission declined to receive them it would not be material. But you can have them.

10860. (*Chairman.*) I do not understand from these letters that the Commission declined to receive his corrections; they said that they would publish them as his statement of what he wished his evidence to be altered to?—Yes, but they declined to accept them.

10861. No, they declined to strike out the evidence?—They declined to accept his corrections.

10862. As corrections?—Yes.

10863. They published both the evidence and the corrections?—Yes.

10864. (*Sir Mackenzie Chalmers.*) They published both and left the public to judge?—Yes. The Secretary said of the corrections: "These appear to the Commission to be so much at variance with the letter and the spirit of the answers you gave at your examination, that the substitution cannot be permitted."

10865. (*Chairman.*) The substitution?—Yes.

10866. (*Sir Mackenzie Chalmers.*) At any rate Dr. Klein himself wished to withdraw a good deal of his evidence?—No doubt he did.

10867. And, rightly or wrongly, he put it on the ground that he was a foreigner, and not very conversant with English at the time?—That is all quite true.

10868. Do you know whether it is a fact that he had done any operations except inoculations?—When?

10869. At the time when he gave his evidence?—I do not know in the least. How could anybody know?

10870. Was he not examined on it?—That must be a fact that no one can know.

10871. At any rate, since the Act, do you know whether he has done anything under his licence for the Local Government Board except inoculations?—I have not the slightest idea what he has done. He has the run of the laboratory at the Brown Institute, and as the Inspector is not there, I do not suppose that anybody knows what he has done. He has a licence.

10872. (*Chairman.*) Do you say that he is in the habit of conducting experiments at the Brown Institution?—I say that he is licensed to perform experiments there. I think he is still licensed.

10873. (*Sir William Church.*) But Lord Selby meant, would you please say when he ceased his connection with the Brown Institution?—That I will look out; I do not know; I am only speaking from my own memory, but it is easily looked up.

10874. (*Chairman.*) I do not know anything about him myself. Sir William Church tells me that it is a good many years ago, possibly 20, he does not know how long exactly, since Dr. Klein ceased to have any connection with the Brown Institution?—I do not think that is so. Here you are—"1904. E. Klein, Brown Institution, Wandsworth Road, London."

10875. (*Sir William Church.*) That was when he was acting under the Local Government Board?—I can only say that he is licensed to perform experiments at the Brown Institution. There is nothing in this Report about the Local Government Board; I can only go by what is published by the Home Office.

10876. I beg your pardon if I misunderstood you, I understood you to say that he was connected with the Brown Institution itself?—No.

10877. He was performing certain experiments there, but they were being done for the Government?—I do not know what they were done for. My sole statement is that Dr. Klein is licensed to perform experiments at the Brown Institution; that is all I know, that is all I state, and all that I ever meant to state.

10878. (*Sir Mackenzie Chalmers.*) You do not think it is very material what experiments he is authorised to perform?—I think it is very material what experiments he is authorised to perform. I may say in passing that I do not find his name in any medical directory. I suppose he is a foreigner, because he does not seem to have taken any degree in England. I am told he is a Fellow of the Royal Society. I did not know whether I was right to call him Dr. or Mr., that is all.

10879. Turning to your evidence in answer to Question 10262, Colonel Lockwood referred you to the sentence in which you say that you are "prepared to

show that under the existing law there is no limit to the agony that may legally be inflicted upon animals," and that you "abominate a statute that authorises gross inhumanity"?—Yes.

10880. You know that the Council of the Royal Society have given evidence here by their President, who, on their behalf, pressed upon this Commission that the existing law and practice should be maintained?—You mean Lord Rayleigh?

10881. Yes. Do you charge him with inhumanity?—That does not arise out of my statement.

10882. I am asking you now?—I thought you were appending it to my statement.

10883. No, I ask you the question. The Royal Society support a statute which you say has this effect. Do you charge them with inhumanity?—Let me see what Lord Rayleigh did say. Will you tell me what it is you refer to, and I will look it up? I have the very greatest respect for Lord Rayleigh.

10884. You will find it in the second volume of our evidence, on page 53: "So much has already been gained from the application of experiments on animals, both for the progress of physiology and for the alleviation of human suffering, and so much more may be confidently expected in the future, that the President and Council trust that nothing will be done that would hamper the legitimate employment of the method"?—What has that got to do with my statement that the present law admits of any amount of torture? I do not want to be hypercritical. You seem to raise it on that statement of mine. I say it is not *ad rem*. Of course, if Lord Rayleigh says that a great deal has been gained from the application of experiments on animals, that is his opinion, and you are right to give it the greatest weight here, of course. I am not here to deny that.

10885. You are here to suggest an alteration in the existing law, which the Royal Society deprecate?—In any alteration of the existing law I trust that nothing will be done to hamper the legitimate employment of the method. Nor does my Bill do so. The legitimate employment of the method is not hampered by my Bill, but I consider that torturing animals is an illegitimate method—and I should hope that everybody would agree with me.

10886. (*Chairman.*) Do you say that the Royal Society suggest any alteration in the existing law?—No.

10887. That is all Sir Mackenzie Chalmers asked?—I do not say that they do. They have not seen my Bill; they have not had my Bill before them; they do not know what the suggestions are; they do not say anything about the law as a matter of fact; they simply say that they trust that nothing will be done that would hamper the legitimate employment of the method. There is no word about the law there or any alteration of the law. I find nothing of the sort to support that suggestion. On the contrary, in the very next sentence they say, "While precautions should undoubtedly be taken against improper use of experiments on living animals, it is not the province of the Society to suggest what safeguards should be adopted." As to that I suggest the safeguards which should be adopted to carry out Lord Rayleigh's intentions.

10888. (*Sir Mackenzie Chalmers.*) Then they go on: "It is, however, the bounden duty of the President and Council to urge that those safeguards should be so framed as not unnecessarily to interfere with that advancement of knowledge to promote which the society exists"?—I entirely endorse that.

10889. "Such restrictions would not only cripple or arrest the growth in this country of an important branch of biological science, but in so doing would reduce the efficiency of both physician and surgeon to mitigate or cure disease"?—I endorse every word of that. I entirely agree with Lord Rayleigh, but I maintain—and I expect that he would agree with me—that if the sole hamperings that I suggest are to prevent animals being tortured, provided that science is left free, those hamperings should be applied at once. I do not imagine that he would be so inhumane as not to agree to that, and if you ask me whether Lord Rayleigh is humane or not, I see nothing in this statement to suggest that he is not as humane as myself.

10890. Coming to the Royal College of Physicians, you have read Sir Douglas Powell's evidence, I suppose?—I have read it through.

10891. Is there any point that you wish to criticise on that?—Really I do not know that I have anything.

10892. I thought you said that there were various points in the evidence which you wished to criticise?—Yes, I have a list of them. I have got them in one packet here. I am ready to deal with them at any moment, if I am invited to do so. (*After referring to notes.*) Yes, I think I have something with regard to Sir Douglas Powell. At question 5,606 he was asked: "You do not believe that any cruelty, as we should understand it, is practised?" To which he replied: "I should say emphatically that it is not." Then "Have you ever seen any experiment in which the animal was allowed to recover from the operation without being killed?" (A.) Not in recent times, not since the Act, certainly." I say that any statement, therefore, on the part of Sir Douglas Powell as to whether cruelty takes place under this Act is wholly out of court. He cannot speak of what happens when he is not there any more than I can; and I can, with equal emphasis, assure the Commission that cruelty is practised on equally good grounds. We were neither of us there.

10893. Are you aware that he said that he always considered carefully before granting a certificate as President of the College?—Yes, I daresay. If you say so, I accept it. He only considers the physiological fitness and the scientific eminence of the applicant; he does not pretend, nor does anybody, so far as I have seen throughout the evidence, that he takes into consideration the humanity of the applicant.

10894. Would it not be rather an impossible task to get evidence of humanity or inhumanity?—I should think not; at any rate, the effort might be made.

10895. I have heard of certificates of honesty, but I have never heard of certificates of humanity; I do not know on what evidence any such certificate would be founded?—I have suggested in the Bill how it should be done.

10896. You have suggested two clergymen or two justices?—No, one justice of the peace and one minister of any religious denomination.

10897. How could they know whether a man is humane or not; how can anyone know?—I presume that a man has friends who know something about him.

10898. (*Chairman.*) Is it essential in your Bill that he should have a minister of religion to speak for him?—The certificate must be signed by "Two or more of the following persons." Two justices of the peace will do perfectly well without any minister of religion.

10899. (*Sir Mackenzie Chalmers.*) You draw in your evidence a distinction between the Home Secretary and the Home Office?—You have done so yourself in one of your letters to me, which I read this morning.

10900. The Home Secretary speaks for his Office, of course?—Yes. I draw the distinction which everybody I suppose, with common sense, would draw, that the Home Secretary himself as the head of the Office passes away with the Government, and the permanent officials who are there always must of necessity take into their care such an Act as this, and I have no doubt that they do so.

10901. You know that the permanent officials are nearly as fleeting as the Home Secretary?—Oh, no.

10902. You will find it is so?—I hope not.

10903. For instance, in this way. Vivisection is in one branch of the Office?—Yes.

10904. A man is continually transferred from one branch to another, as a man gets promotion from one branch to another. The only permanent person is the Under-Secretary, for the time that he is there?—Oh, yes.

10905. Anyhow, what I want to come to is this. Many of the cases that you have referred to must necessarily have come before the Home Secretary himself. He has to answer the questions in Parliament?—Yes, but I assume, and I have no doubt it is the fact, that those answers are almost invariably prepared for him by the permanent officials of the Office; he does not write them of his own initiative. If you are prepared to tell me that he does—

10906. I cannot give evidence, but I can put the question in this way. The draft is prepared for the Home Secretary, of course, of the answer to be given

in Parliament; that draft he considers himself, alters himself, asks for further information himself, as he wishes and pleases?—No doubt.

10907. And he ultimately is the person responsible?—Yes; that is a technical matter, which I must leave to you and your chief. It is for you to say who is responsible. I bring my indictment against the Home Secretary—and his Office, if you prefer it; only I think myself, and I have always thought, that it is the Home Office officials who really are the persons who frame these answers. The Home Secretary cannot be a master of all the details of an office of that kind.

10908. Certainly not?—Naturally.

10909. But if you take the people who have been Home Secretaries since the Act of 1876, the whole series of them—Lord Cross, Sir William Harcourt, Sir Matthew White-Ridley, Mr. Akers Douglas, Mr. Asquith, Mr. Gladstone—do you really think that they have not exercised independent control?—I am afraid I must answer that question very likely in some cases in the affirmative. I think it was under Sir William Harcourt that the habit grew up of consulting this Association of Vivisectioners before a certificate was granted.

10910. That was instituted by Sir William Harcourt himself?—Yes. I dare say in big questions of policy of that sort the Home Secretary is the person whose word is taken, but in all matters of detail and all questions of routine, it is not so.

10911. Do you think that with all the attention that has been drawn to these matters in Parliament, each Home Secretary has not considered and laid down his own line?—No, I think they are immensely influenced by the officers behind them. I think it would be extremely difficult for the Home Secretary to take an entirely new line inside the four corners of the Vivisection Act against the settled habit, if I may say it, of the permanent officials, because settled habit it has been, as I have shown, I think. I think it is almost incredible that the same settled habits should be manifested under so many different heads, unless it were that the permanent officials were almost supreme in that matter.

10912. Have not the permanent officials, as I asked you before, altered almost as fast as the Home Secretary?—I have had a great number of letters signed by the same person, I know, through many years.

10913. You object to Sir William Harcourt's proceedings in making a reference to the Society for the advancement of Medicine by Research?—Most decidedly.

10914. Each licence and certificate goes to them. Are you aware what happens when they come back? Are you aware that certificates are continually altered and modified, and sometimes refused?—My point is that, so far no evidence has been offered here before this Commission to show that the Home Secretary has ever gone contrary to this association till the Commission was sitting.

10915. And, of course, there is no evidence, equally, that he has followed those recommendations?—The next thing I want to ask you about is this. You referred to Dr. Schäfer's drowning experiments. You are aware that his object was to verify the researches of a particular method of restoring the drowned?—I am not aware of it. I accept your statement. The object does not concern me.

10915a. It was a research carried out by him, I think, for the Royal Chirurgical Society, who wished to know what method ought to be recommended, there being four or five different methods of resuscitating the drowned. That, you will allow, is an important matter, as to what is the best method of restoring drowned persons?—Oh, dear, yes.

10916. Are you aware that Professor Schäfer performed 36 experiments?—I am not in the least aware of that. I will accept your statement.

10917. Thirty-four of those were under anæsthetics?—You say so.

10918. Does that affect your opinion of his experiments. You say that his experiments were regarded with particular detestation?—I did not say that. I said your permission to him was regarded with particular detestation.

10919. Does it make any difference in your opinion that 34 of those were under anæsthetics?—It may be that Professor Schäfer was much more humane than

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The Hon. S. Coleridge. the Home Office. The Home Office may have given him permission to do things which he did not do. My complaint is that the Home Office permitted him to do them, and they gave him a certificate that he might do them. I do not know whether he did them or not. My complaint is that he was permitted to do them.

10920. Thirty-four of those experiments were licensed under anaesthetics. He then applied (will you take it from me) for eight experiments without anaesthetics?—You say so; I know nothing about it.

10921. But he only performed two—will you take that from me? He considered those experiments unnecessary to control the conditions under which the anaesthetic experiments were performed?—The *gravamen* of my charge is that the Home Office gave Professor Schäfer leave to drown and re-drown the same dog without anaesthetics. That is the *gravamen* of my charge. What he did is not known to me nor to anybody but himself.

10922. Will you take from me what he did and what he wanted to do: that the two animals that were not under anaesthetics were not revived, but were simply drowned straight away?—What is your question?

10923. Will you accept his version of what actually happened: that the two animals which were not put under anaesthetics were drowned, and not allowed to recover?—You challenge me to make a charge of cruelty against Professor Schäfer. That is not my intention. I do not know Professor Schäfer.

10924. I am asking you about the actual experiments which were performed under the permission of the Home Office?—If your statement is correct it only proves to me that Professor Schäfer was more humane than those who gave him permission. You gave him permission to do something far more cruel than he actually did, if your statement is correct.

10925. There was a question asked about it in the House?—Yes, there was; that is what I am basing my answer upon.

10926. Will you read the question and answer?—I am afraid I have not got it here at the moment. (The question was postponed.) You do not traverse that statement of mine, that permission was given by the Home Office to drown and re-drown dogs?

10927. To test the means of resuscitation?—There was nothing in the certificate to prevent his drowning and re-drowning the same dog. I had that question asked in the House (I am sorry I cannot find it you now) and the answer was that Professor Schäfer had informed the Home Secretary that he had not done so.

10928. I do not think the certificate would ordinarily be in those terms. However, we can get the original certificate?—Yes. I am perfectly convinced that the permission given him was to drown and resuscitate dogs without anaesthesia.

(Chairman.) It must depend upon the document.

10929. (Sir Mackenzie Chalmers.) Anyhow, whether that is so or not, for the purpose of saving innumerable human lives, would not that, in your opinion, be justifiable?—I have not heard that one single person has been resuscitated in the country by these experiments yet. I have read the account, and so far as I can see the sole thing was, that you must put the person on his face so that the gravitation might act on the tongue and it would come out of the mouth; whereas the other way it would fall back and choke him.

10930. (Chairman.) That is not a full explanation of what he stated?—I read the account.

10931. I remember that he said more than that. One particular thing that he impressed was that a single person could do it with very few movements, whereas if you carry it out with the present methods it requires two persons if you are really to do it effectively, and it is a much longer process to set about.

10932. (Dr. Gaskell.) And the amount of air exchange was greater, and that any unskilled person can do it. You can describe by a photograph the exact method, whereas you can only teach the Sylvester method by means of a skilled teacher?—I can produce the question and answer in the case of course. I have not seen the certificate. We can get it to-morrow.

10933. (Sir Mackenzie Chalmers.) You think, anyhow, that even with the prospect of saving innumerable human lives such an experiment, if performed, was not justifiable?—My position is a very clear one on that. I say that the loftiest motive professed by per-

sons who inflict pain and suffering upon animals does not justify their inflicting pain on animals. If so, as I said before in my opening statement, you might steal to devote the money to good purposes. An act is either right or wrong, and it is quite apart from the motive of those who perform that act.

10934. Do you object to drowning dogs as a means to getting rid of superfluous dogs?—I prefer the lethal chamber at Battersea.

10935. But if a dog is drowned right out it suffers no more if it is drowned in the course of an experiment?—I have always been told that the proper way to kill a dog is to shoot it—that that is the most merciful way, and I presume it is the way in which most people do it.

10936. In Edinburgh, at all events, when Professor Schäfer performed his experiments, and until recently, the mode of getting rid of superfluous dogs was by drowning, was it not?—I do not know. You know it is the resuscitating the dog and then drowning it again, which was what I said was the *gravamen*.

10937. That as a matter of fact we were told was not done except under anaesthesia?—That may be.

10938. Now then we come to some experiments which certainly read as very horrible experiments, by Dr. Crile. You know nothing about those experiments except from Dr. Crile's own published report?—That is so.

10939. Would you accept his own explanation?—Certainly not; of course not. I read his book, and see what he says, and I accept that; but subsequent explanations when he is called upon to explain himself by the Home Office, if he was, I certainly do not accept.

10940. He was called upon, and perhaps I may read his explanation as given by himself?—I do not accept it for a moment.

10941. Whether you accept it or not, perhaps you will kindly listen to what he says?—Certainly.

10942. Dr. Poore wrote to him. The first letter miscarried, because he was in some out-of-the-way part of America. Then Dr. Poore wrote again, and he replied.

(Chairman.) What is the date?

10943. (Sir Mackenzie Chalmers.) There is no date of the year here. "Dear Doctor,—Replying to your favour of June 19th" (we can get the date from the previous letter) "I beg to say that in the entire series of experiments no animal suffered pain. The reference to the corneal reflex was correctly made, but the animal was not conscious. In the remark to Protocol XVI., page 31, 'At one time the anaesthetic was overlooked' simply meant that the animal had recovered far enough from the state of surgical anaesthesia to exhibit a corneal reflex, and begin a slight voluntary movement. By surgical anaesthesia I mean the administration of the anaesthetic to the same degree of anaesthesia as in the human subject in the performance of surgical operations. I wish to emphasise that in no instance did any animal suffer pain. In nearly every experiment I had a special anaesthetizer. As I am engaged in an active surgical practice, and have been for years, I feel myself quite competent to state that the animals I have subjected to vivisection have not suffered any more than the patients upon whom I have performed operations. And the latter, I know, have not suffered at all. The experiments were all performed as nearly as possible under the same conditions as operations on patients. I shall at once send a book to the Secretary of State. The delay in my reply was due to my being abroad—just returning yesterday. Thanking you for writing me, and if there is anything more that I can do, please let me know. Very respectfully yours, GEORGE W. CRILE, to G. V. Poore, M.D., 32, Wimpole Street, London"?—It is rather strange that he quotes that one statement that "the anaesthetic was overlooked" in the letter, and then goes on to say "The animal was coming out of anaesthesia, and the corneal reflex appeared." He did not read his own experiment, because the next words in the experiment are "The dog became profoundly under its influence," which, of course, meant that the dog had got too much, not too little, "Causing a very great fall in blood pressure of 40 millimetres in 85 seconds." So that in the book the overlooking was that it got too much, and in the letter the overlooking is that it got too little. You can look at the book for yourself. The first thing he

says in his letter defending himself shows that he is not accurate. So that he answered you without looking at his own book.

10944. Possibly?—In such a hurry was he to defend himself, in my opinion. There was no allegation against him in that experiment about anaesthesia, because it was there stated that the animal became profoundly under its influence.

10945. His letter refers to the whole experiment?—No, he quotes that particular one in that letter. That is the very one he picks out.

10946. "I wish to emphasise that in no instance did any animal suffer any pain." That refers to the whole?—Yes, quite so.

10947. Are you aware that Sir Victor Horsley saw a good many of these experiments?—I do not know, I am sure.

10948. Are you aware that he was of opinion that no pain was caused to the animal? We shall have his evidence—he is coming here?—No doubt he will say that.

10949. Are you aware that Dr. Goodbody also saw these experiments?—No, my whole position is that the Inspector should be there. If the Inspector had been there I should have been prepared to take the Inspector's word that these animals suffered nothing. The Inspector was not there, and I decline to accept the bare statement of the performer, particularly when this man himself, in his letter of excuse, clearly shows that he is not accurate.

10950. You accept his statement in the first instance, and you will not accept it in the second?—I will not accept Dr. Crile's statement at all.

10951. Will you accept Sir Victor Horsley's statement as a spectator?—Certainly not. I do not accept a person's account of his conduct in an experiment when his conduct is impugned, for one moment. I say that there should have been an inspector there. Parliament in its wisdom did not trust these men. If it had it would not have told you to appoint an inspector there to watch them. As the Inspector was not there I say I do not trust them. Parliament did not trust them; why should I? Parliament was not satisfied with their uncorroborated word as to what happened in their laboratories. Why should I be?

10952. We may have inspection, but still we may ask a person of character, when he saw the experiment, what his opinion of it was. You will not accept that?—Certainly not, because I think that all these experimenters have the greatest contempt for the Act of Parliament. They would deny a breach of this Act just as I should deny a breach of the Motor-Car Act. I drive a motor-car, and when I go beyond the speed limit, and a policeman asks me, I say, "No, I am not going beyond the speed limit." Nothing would keep me from going beyond the speed limit except the presence of a policeman in the car; and nothing will keep an experimenter within the four corners of the Act except an inspector in the laboratory.

10953. Surely if you were asked about the speed limit, and gave your word that you had not exceeded it, you would not expect to be disbelieved?—No, I do not say so. I said last year that, of course, I did, and I exceed it every time.

10954. (Chairman.) You are apparently not very ethical about motor-cars. If you apply your principles as regards motoring to the physiologists, you have very little to say against them?—What I have to say is that they regard the Vivisection Act of 1876 with the same contempt that I regard the Motor-Car Act as regards the speed limit. Every motorist who has a motor-car, of course, breaks the Act.

10955. What you said was that if you were going more than 20 miles an hour, and were stopped for it, you said that you were going less than 20 miles an hour?—No; on the contrary, I was asked, and frankly answered that I was going beyond 20 miles an hour. I did not mean that. I always frankly admit it. When I am summoned, and get to the law court, I always admit it.

10956. Then you must not be quite so hard on a physiologist as to say that men are not to be believed on their oath?—I did not mean that; I meant that they regard the Act in the same sense as we motorists regard the Motor-Car Act. We do not treat it with very great respect as regards the speed limit.

10957. (Sir William Church.) Some of us?—Some of us.

10958. (Mr. Ram.) You think that these people will tell a lie about this because they have a contempt for the Act?—I did not say that. The Hon. S. Coleridge.

10959. That is what I gathered. You despise the Motor-Car Act, which says that you may not go more than 20 miles an hour. Therefore on the face of your evidence you would not mind saying what was untrue with regard to it. You say that these men have a contempt for the Act, and therefore they say what is untrue?—No, I say that they will break the Act with the same facility as I break the Motor-Car Act, for the same sense of any lack of morality about it. 26 June 1907.

10960. If one of these physiologists says, "I performed an operation, and I say the animal did not suffer," is that any reason why they should not be believed?—I do not think it is unprejudiced evidence. What do you expect them to answer? What would you expect Dr. Crile to say—"I, of course, torture the animals"?

10961. (Sir John MacFadyen.) The same as you do when you are asked about the speed limit?—The difference there is very great. Nobody regards anybody a pin the worse for exceeding the speed limit. Nobody, so far as I know or ever heard of, would hold it up against anybody as a moral fault to exceed the speed limit, but merely a technical breach of the Act; but I think that the public would certainly regard it as a moral offence if Dr. Crile were to admit that he tortured these animals.

10962. (Sir Mackenzie Chalmers.) We shall have Sir Victor Horsley's evidence, for what it is worth, as you say?—Yes.

10963. Do you suggest that the Home Office could have made any further inquiries than they did from persons who were present and from Dr. Crile himself?—I do not say that. I say that the present Act of Parliament is most defective, because they cannot get at present any information except from the person whose conduct is impugned. Therefore I consider that all the evidence that they get must necessarily be prejudiced. That is why I want the Inspector to be there always.

10964. But you agree, do you not, that the Home Office have to carry out an Act of Parliament as they find it, whether they like it or whether they do not?—Yes, I suppose that is so.

10965. Some Acts they may like, and some Acts they may not like, but they equally have to administer them according to the words of the Act?—Yes.

10966. And you know that as a matter of legal construction, not even expressions used in Parliament can be referred to, to gather the intention of an Act. You cannot refer to the debate in Parliament as throwing any light on the meaning of words used in an Act?—Is that so?

10967. It has been laid down pretty often?—I suppose that is so.

10968. Still less can you refer to the Report of a Commission, which may or may not have had anything to do with the foundation of the Act?—No, certainly there is a difference between a speech made in the House for or against an Act and the report of a Royal Commission upon which an Act of Parliament is based; at least I should have thought so.

10969. You have a Royal Commission, and then the Minister who prepares the Bill takes or does not take its recommendations, and in construing the Act you only have to look at the words of the Act itself which is the final expression of the Legislature?—I think that extraneous considerations are allowed. For instance, I have here Maxwell's "Interpretation of Statute," the fourth edition, page 30, "The liberal construction of an Act of Parliament) has, in general, but *prima facie* preference. To arrive at the real meaning it is always necessary to get an exact conception of the aim, scope and object of the whole Act; to consider, according to Lord Coke (Haydon's Case, 3 rep. 7b; 10 rep. 73a):—(1.) What was the law before the Act was passed. (2.) What was the mischief or defect for which the law had not provided. (3.) What remedy Parliament has appointed. (4.) The reason of the remedy. According to another authority (*per* Lord Justice Turner, in *Hawkins v. Gathercole*, 6, De G. M. G. 1) the true meaning is to be found, not merely from the words of the Act, but from the cause and necessity of its being made, which are to be ascertained not only from a comparison of its several parts, but also from extraneous circumstances." Therefore I think you may call

The Hon. S. Coleridge. the Report of a Royal Commission an extraneous circumstance which ought to be taken into consideration.

26 June 1907 10970. (*Chairman.*) You may think that is good enough for us poor Commissioners, but in a Court of Law I think you would find some difficulty in making that assertion?—It is Lord Justice Turner.

10971. He never said that you can interpret a statute by the Report of a Commission?—No, he does not say so, and I do not say so. He says that extraneous circumstances may be taken into account.

10972. (*Sir Mackenzie Chalmers.*) Is not that when the language of the Act is ambiguous, and not when the meaning of the Act is clear?—"We have therefore to consider not merely the words of this Act of Parliament, but the intent of the Legislature, to be collected from the cause and necessity of the Act being made, from a comparison of its several parts and from foreign (meaning extraneous) circumstances so far as they can justly be considered to throw light upon the subject." Those are Lord Justice Turner's exact words. I think that the Royal Commission does throw light upon the subject, so far as I can see. It is not very important.

10973. Further on you draw a distinction between the Home Secretary and his Department, which you have already mentioned. Of course, you agree that the Department is under this difficulty: that it never can reply. No officer of the Department who is attacked can ever reply?—I do not attack any individual in the Office, of course, and I have never done so. I have never done such a thing at all.

10974. I am not referring to the present time; I am rather referring to your objection to Dr. Poore. When Dr. Poore ceased to be Inspector, he made a speech of which you disapprove?—Yes.

10975. At the time when he was Inspector you and your Society had attacked him, had you not, as Inspector. You had raised various points about his inspection and his administration of the Act?—Very likely, about his reports.

10976. Then when he became a free man he retaliated?—I do not think that is so.

10977. You do not accept that way of putting it?—I think that his making that speech, so soon after he was relieved from the trammels of office, showed that he must have had a bias while under the trammels of office; and besides, I fail to see why criticism by my Society upon the Inspector should produce any personal animosity of that kind, or why it should find expression in such violent language.

10978. As a matter of fact, what he was doing was hitting back when he was a free man, was it not?—I wonder if he was ever criticised by the vivisectionists.

10979. Then after Dr. Crile's experiment I think you wrote some letters to various surgeons?—I did.

10980. How many did you write altogether, do you know? We have got answers, I think, from seven?—I wrote to a great number. The vast majority did not answer me at all. I know that many more did not answer me than did. I really forget how many.

10981. Seven are all the answers that you got, I understand?—One from Sir Victor Horsley's secretary to say that he was away, and one from Professor Schäfer, and the rest from surgeons.

10982. You wrote a good many?—Yes.

10983. And you got answers from seven?—I wrote many more than seven, certainly.

10984. Then there was some subsequent correspondence from some of them, was there not?—Mr. Timothy Holmes and Mr. Owen?—I dare say there was.

10985. That subsequent correspondence we have not had?—I will get it.

10986. For instance, in the "Times" of March 15th, 1902, from Dr. Owen?—Yes, there was one letter to the "Times," I remember now.

10987. Shall I read it?—If you read his letter, I hope you will read my reply, if you have it.

10988. Yes. I have your reply too. This is from the "Times," of March 15th, 1902: "Anti-Vivisection Methods.—To the Editor of the 'Times.'—Sir,—When Mr. Stephen Coleridge asked me some time ago if it was true that at St. Mary's Hospital operations were performed on patients under incomplete anaesthesia, I at once sent him a reply in the negative. For, knowing his methods, I was afraid lest he was dressing up some horrible bogey, wherewith to shock a generous but emotional public, at the time when we were raising money to finish our much-needed Clarence wing. And standing as it did, my reply was perfectly correct. But when I find Mr. Coleridge using it in support of his attack upon men of science, who, in a humane and lawful manner, are doing a necessary piece of distasteful work, of which Mr. Coleridge and I, and his and mine, and the whole community, may reap incalculable benefit, I must beg you to allow me to qualify it. For closing a cleft in a child's palate, and in certain grave operations on the tongue and throat, to have the patient in complete anaesthesia would be to invite disaster. But why should it be necessary to spread out these hideous things before the public gaze?—I am yours truly, EDMUND OWEN." To which you reply on March 17th, 1902: "Sir,—With regard to Mr. Edmund Owen's letter in the 'Times' of to-day, I asked him a plain question, and he gave me a plain answer. Now he says he wants to qualify that answer, and in qualifying it he states that he made it originally in the form he did for fear I should stop subscriptions to some ward they were building at St. Mary's Hospital! Mr. Owen's qualification of his original statement is that incomplete anaesthesia is employed in certain operations on the tongue and throat. The experiment of Dr. Crile I cited, and which took place under incomplete anaesthesia in Mr. Victor Horsley's laboratory, consisted in repeatedly crushing a fox terrier's paw. Mr. Victor Horsley and his friends are not likely to succeed in this controversy if they continue to enter the arena without previously consulting each other as to what they shall say.—Your obedient servant (Signed), STEPHEN COLERIDGE." Then you also had a correspondence with Mr. Timothy Holmes?—If you have got it there, by all means, if you will, read my reply.

10989. This is from the "Times" of March 17th, 1902: "Anti-Vivisection Methods.—To the Editor of the 'Times.'—Sir,—I see in a letter from the Hon. S. Coleridge in your paper to-day that he quotes from one which I wrote to him some years since. Mr. Coleridge is a complete stranger to me; but he has sometimes written to me about hospital matters, and the sentence which he quotes was in reply to a letter in which he seemed to be labouring under the strange delusion that hospital surgeons (for what purpose I know not, but I supposed him to fancy that it was for purposes of experiment) were in the habit of performing operations while their patients were still conscious of pain. Now, I was a hospital surgeon for more than a quarter of a century, and have performed hundreds and witnessed thousands of operations. I never heard of a case in which the patient was operated upon while conscious of pain, nor can I conceive why the surgeon should so act, and so I told Mr. Coleridge. I have, as I say, no knowledge of this gentleman, and have no right to speculate on his motives for the attitude he assumes towards our hospitals, but it seems to me a mistaken and a mischievous one. The researches carried on in our hospitals (miscalled 'experiments' by persons of Mr. Coleridge's views) have resulted in an immense saving of human life and human suffering, and anything that withdraws from our hospitals the public confidence and support appears to me a crime against humanity.—I am, Sir, yours faithfully, TIMOTHY HOLMES." I am afraid I have not got your reply with me.

10990. (*Chairman.*) Perhaps if we adjourn now you will be able to bring it next time?—Yes.

10991. (*Sir Mackenzie Chalmers.*) And will you bring any of these letters that you have got which we have not had?—Yes.

TWENTY-FIFTH DAY.

Wednesday, 3rd July 1907.

PRESENT :

The Right Hon. The Viscount SELBY (*Chairman*).
Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. McFADYEAN, M.B.

Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*.)

The Hon. STEPHEN COLERIDGE recalled; and further Examined.

10991A. (*Chairman*.) I think when he finished the other day you had been asked about some correspondence between Mr. Timothy Holmes and yourself?—Yes.

10992. And you said you would probably be able to bring some letters to-day?—Yes, I have them all here.

10993. (*Sir Mackenzie Chalmers*.) I think I had read the letter of Mr. Timothy Holmes dated the 17th of March, 1902?—Perhaps you would like me to give you the results of my investigation at my office as to this correspondence. I have a very large number of letters. I have the whole correspondence here now; every letter that has passed between me and all these people. I have looked through all my books and letter books, so far as I can, and I have here a list of the eminent surgeons to whom I wrote, so far as I can make out:—E. Owen, T. Holmes, E. Nettleship, T. Bryant, T. W. Nunn, T. Bond, Professor Schäfer, Wm. Allingham, Jonathan Huthinson, Lord Lister, Sir H. Thompson, Mansell Moullen, Sir Victor Horsley, T. H. Openshaw, J. A. Kingdon, Noble Smith, Herbert Snow, M. F. Rees, G. D. Pidcock, J. May, T. J. Walker, G. W. Gosling, T. Walker, T. R. Jessop, J. A. Dearden, and S. F. Barber. I wrote to those myself. Of those Nettleship, Nunn, Allingham, Moullen, Sir Victor Horsley, Kingdon, Noble Smith, Rees, Pidcock, Gosling and T. Walker made me no reply. T. Holmes' reply was marked "private." I also find that I asked Lady Grove to write for me to Sir Frederick Treves, because she happened to know him; and I asked Lord Harberton to write to Walter Tyrrell, because he knew him; and I have the replies forwarded to me from those two gentlemen. I have all the replies now.

10994. All I was concerned with was, that some of the people you wrote to afterwards protested, rightly or wrongly, against the use you had made of their letters?—I do not think that is quite the way to put it. I have the correspondence here.

10995. I read Mr. Timothy Holmes' letter of the 17th March, 1902. Is there any reply to that that you wish to put in. I propose afterwards to read a letter of the 27th of March?—That I think is marked "private."

10996. I am only referring to a letter in the "Times"?—That cannot be marked "private."

10997. That is all I am concerned with. Do you wish to put in any reply to that letter of the 17th March?—Yes, I do. I wish to read my reply:—"To the Editor of the 'Times.' Sir,—Mr. Timothy Holmes

is entirely inaccurate in stating that my letter to him suggested that hospital surgeons operated on their patients under incomplete anaesthesia, and I ask him either to withdraw this inaccurate statement or to publish my letter, which will disprove it. Although a very august authority had informed me that hospital patients were operated upon under incomplete anaesthesia—i.e., before the corneal reflexes were abolished—I did not believe it, and wrote to Mr. Timothy Holmes, hoping that he would authorise me to deny it, which he did." The rest of the letter is on quite another subject; I do not know whether you want it introduced. That is all I say about Mr. Timothy Holmes.

10998. (*Chairman*.) You are speaking about vivisection, but not about this case?—It is on quite another subject. I am ready to go into it. I am answering two letters, you see, in my letter, at the same time.

10999. (*Sir Mackenzie Chalmers*.) Then a third letter came from Mr. Timothy Holmes?—Yes, on the 21st of March he writes to the "Times": "Sir,—I enclose a copy of the letter of Mr. S. Coleridge which he asks me to publish, and will leave your readers to judge whether I was inaccurate in saying that it suggests that hospital surgeons operate on their patients under incomplete anaesthesia; and then he enclosed my letter to him, which is as follows: "Dear Sir,—I have been recently informed on very high authority that surgeons now frequently perform severe operations on man under 'incomplete anaesthesia,' that is, while the eyelid still blinks when the eyeball is touched. As you may possibly be aware, I vehemently believe that animals are tortured in laboratories attached to our great hospitals, but I hesitate to accept this information charging our medical men with conveying the inhumanity of the laboratory into the hospital wards and operating theatre. I should be much obliged to you if you would tell me whether your experience leads you to endorse or deny this statement. Very faithfully yours.—Stephen Coleridge." My comment on that letter is that my letter in no way suggests for a moment that I believe it; on the contrary, I say that I do not.

11000. Then Mr. Timothy Holmes writes the following letter in the "Times" of March, 27th, 1902: "Sir,—I judge by your leading article to-day that you think you have given sufficient space already to this topic; but I hope you will find room for these few lines in reply (final as I hope) to Mr. S.

*The Hon.
S. Coleridge.
3 July 1907.*

The Hon. S. Coleridge. Coleridge"?—I have not seen that; but I am quite ready to hear it.

3 July 1907. 11001. "Mr. Coleridge (see his letter, March 20th) charged me with being entirely inaccurate in stating that his letter—a copy of which you published on March 21st—suggested that hospital surgeons operated on their patients under incomplete anaesthesia." As a matter of fact, what I had said (see my letter of March 17th) was that he appeared to have laboured 'under the strange delusion that hospital surgeons were in the habit of performing operations while their patients were still conscious of pain.' And if this was not the meaning of his letter, what could it mean? It is quite true that he says he hesitates to accept the information given him on very high authority to this effect; but as he also says that he 'vehemently believes' that animals are tortured in laboratories attached to our great hospitals, and talks of 'our medical men conveying the inhumanity of the laboratory into the hospital wards and operating theatre,' it is clear that he did believe it. Else why write to me—a perfect stranger—and, as it now turns out, to many others equally strangers to him, to settle a question that any surgeon of his acquaintance could have answered at once? It is quite evident that he confounds 'incomplete anaesthesia' with sensibility to pain, and that he had discovered the mare's nest which I alluded to. However, quite enough has been said about Mr. Coleridge and his opinions. I fully agree with the writer of your leader, that it is easy to attach too much importance to them; and, as far as the progress of physiology and medical science is concerned, I daresay they are negligible quantities. But at the present time our hospitals are seriously hampered for funds, and the misrepresentations of the anti-vivisectionists tend to frustrate our efforts to secure what is wanted. Only the other day a gentleman thus replied to the appeal from St. George's Hospital, which you, Sir, have so kindly supported: 'It would be impossible for me to give to any hospital of which I know that part of its income is used, not for the relief of the poor, but for a purpose which I consider immoral and detestable—the vivisection of animals.' I answered this gentleman," and so on?—I think if that letter is read and considered by this Commission, I am entitled to defend my entire conduct as regards the diversion of hospital funds to schools licensed for vivisection.

11002. (*Chairman.*) I do not see any object in going into it while we are talking about Mr. Timothy Holmes, unless you consider that you are attacked in any way?—I am quite prepared to defend every action I have taken.

11003. (*Sir Mackenzie Chalmers.*) I only read it for the purpose of showing that Mr. Timothy Holmes protested against the use made of his letters?—My answer to that, of course, is that I did not answer that letter; it did not need an answer. It is self-evident that he is wrong. I had never in my letters suggested anything of the kind that he said I did. On the contrary, I said in my letter distinctly the opposite, and asked him to confirm my opposite view.

11004. At any rate, he differed in effect as to that?—There can be no difference as to what English means. My letter meant one thing; he said it meant another.

11005. (*Chairman.*) I cannot accept that general proposition, I am afraid, upon a subject in which lawyers find great difference?—I fall back on Bishop Butler, who said "Words mean what they do mean, and not other things."

11006. He did not solve the question what they do mean?—No.

11007. (*Sir Mackenzie Chalmers.*) Probably you make an exception to your rule in favour of Acts of Parliament?—Well, that is rather sweeping.

11008. Then you read part of the Home Office letter to us last time. As the foundation of your letter to these surgeons, you read part of the Home Office letter of the 2nd September, 1899. I would rather have the whole letter on the notes?—My letter to the Home Office, do you mean?

11009. No; a letter from the Home Office to you. I would rather have the whole letter on the notes?—Certainly.

11010. You read a portion of it?—Did I? What is the date of it?

11011. Whitehall, 2nd September, 1899. It is in reference to Dr. Crile; you read a portion of it. Mr. Cunynghame wrote to you—"Sir, With refer-

ence to the sixteen experiments which were performed in this country by Dr. Crile, and to which you call the attention of the Secretary of State in your letter of the 18th May last, I am instructed to say that before these experiments were sanctioned the Secretary of State was advised that they would be wholly free from pain, and that their object was to study the phenomena of shock after surgical operations in spite of anaesthesia. Their object, therefore, would have been frustrated if the animals had been allowed to suffer pain. On receiving your letter, the Secretary of State caused investigations to be made into these experiments, and the information he has received from competent persons, who either witnessed most of the experiments or have read the account of them, is to the effect that in no instance had the animals experimented upon been subject to pain. At the same time he caused inquiry to be made of Dr. Crile, from whom the strongest assurance has been received that 'in the entire series of experiments no animal suffered pain.' The use of the term 'incomplete anaesthesia' has probably given rise to the misunderstanding. With regard to this, the Secretary of State is advised that there are various degrees of anaesthesia. In 'complete anaesthesia' the narcosis is very profound, and the 'reflex movements,' that is, the involuntary movements in response to an external stimulus, are suppressed. One of the last of these reflex movements to disappear is the blinking of the eyelids when the surface of the eyeball is touched, thence called the 'corneal' or 'conjunctival reflex.' But short of this is the condition termed by Dr. Crile 'incomplete anaesthesia,' in which the creature is quite insensible to pain, although the corneal and other reflexes can still be obtained. It is quite usual for even severe operations to be performed on man in this state of 'incomplete anaesthesia.'" (I read every word of this so far.) "Indeed in many cases complete anaesthesia could not be induced without great danger to life during the operation. But absolute freedom from pain exists, and the patients are wholly unconscious in this 'incomplete anaesthesia.' Under these circumstances the Secretary of State can find no reason to think that any offence has been committed against Section 3, Subsection (3) of the Act, which requires that the animal during the whole of the experiment shall be under the influence of some anaesthetic of sufficient power to prevent its feeling pain. I am, at the same time, to return the copy of Dr. Crile's work, and to thank you for having temporarily placed it at the disposal of the Secretary of State.—I am, Sir, your obedient servant. (Signed) Henry Cunynghame." I read the whole of that letter last time.

11012. Did you? I thought you only read an extract from it?—No, I read the whole of it, so far as I am aware. I had better read on now the whole of the correspondence, I think. To that a reply was sent from my office on the 16th September, 1899. This is written by myself.

11013. I never found the last part of that letter. I thought you stopped at the word "obtained"?—I may have done so; I do not think so.

11014. (*Dr. Gaskell.*) It is at No. 10,301?—The whole point is that last sentence on which I base my letter to the surgeons.

11015. (*Chairman.*) It is no good reading more letters about a matter which has been already passed?—(*After referring.*) I think you are right. I seem to have left out the important item of it.

11016. (*Sir Mackenzie Chalmers.*) You extracted the part referring to these medical men?—Yes, no doubt.

11017. Looking at it now, do not you think you would have saved correspondence with these men if you had set out rather more fully the Home Office letter?—I gave them the actual words: "It is quite usual for even severe operations to be performed on man in this state of 'incomplete anaesthesia'; and I gave the definition that you give me, namely, that the corneal reflexes were abolished."

11018. (*Chairman.*) What is the passage that was omitted in the letter that was sent to these gentlemen?—I shortened it a little, that is all.

11019. (*Sir Mackenzie Chalmers.*) You shortened it by leaving out "Indeed in many cases complete anaesthesia could not be induced without great danger to life during the operation, but absolute freedom from pain exists, and the patients are wholly unconscious in this incomplete anaesthesia." Those are the words which we should like to have seen in your letter to the surgeons?—I quite admit that. That is what you were

told by your advisers. I was not in the least inclined to place that advice before these surgeons. I wanted their independent view; I did not want their replies to be biased by this advice from the Home Office. I placed the case naturally before them without any bias.

11020. You took a passage away from its context, and rather, perhaps unintentionally, altered the meaning of it?—No, I do not think so. That is only your advisers. It is a complete statement: "It is quite usual for even severe operations to be performed on man in this state of incomplete anæsthesia." That is the question I want to know, whether it was true or false according to the advice of these great surgeons.

11021. That sentence is very materially affected by its context in the reply to you, is it not? It is the introduction to what follows afterwards?—I think the only context which has any weight with it is "What is incomplete anæsthesia," and I used your advisers' definition of it, namely, "before the corneal reflexes are abolished." That is the only qualification necessary for getting an independent answer.

11022. I am not quite sure that I agree. The correspondence speaks for itself, I think, now?—Yes, I quite agree. I should like to go on with it.

11023. (Chairman.) With what?—With this correspondence; I do not want it stopped in the middle.

11024. (Sir Mackenzie Chalmers.) You quoted the letter, and all I wanted was the whole of it.

11025. (Chairman.) We may be going to inflict upon ourselves a great deal of correspondence that is not very material. We have had the letter that you wrote to the surgeons, and we have had their answers. You are now being asked questions as to whether certain of these gentlemen either had not the proper material before them or gave answers which contained passages which modified your view of their answers. That correspondence bearing on those points?—This is the correspondence between myself and the Home Office that I am alluding to at the moment. It is a letter that I wrote in answer to that to the Home Office. I do not press it.

11026. I do not know what it is?—I do not know either; I have not read it.

11027. The correspondence between you and the Home Office may be material. At the same time, I hope you will not read anything that is merely controversial correspondence?—It is on this question of incomplete anæsthesia, which is the whole point of this dispute between myself and the Home Office.

11028. But in the letter which has just been read they state the view of their advisers. The Home Secretary himself does not profess to know anything if he is not advised. We have your view about it?—This next letter shows what the views of those advisers are out of your own report, in which they define incomplete anæsthesia and complete anæsthesia themselves; and I thought it was *ad rem*: "Sir, I showed your letter of the 2nd September to Mr. Coleridge yesterday at his country house, where he, unfortunately, is laid up, and he desires me to point out that the distinction you now draw between 'complete' and 'incomplete' anæsthesia cannot possibly be unknown to Dr. Poore, the Government Inspector. Yet that official makes the following statement in his last report: 'In experiments performed under the licence alone, or under certificate C, the animal suffers no pain, because complete anæsthesia is maintained from before the commencement of the experiment until the animal is killed.' Mr. Coleridge therefore instructs me respectfully to inquire what is now the rule of the Home Office. Is incomplete anæsthesia permissible under licence alone, as you state in your letter under reply, or is only complete anæsthesia permissible under licence alone, as Dr. Poore states in his report? The other matters contained in your letter will have Mr. Coleridge's attention when he is better."

11029. I do not understand that Dr. Poore in that letter was drawing any contrast between complete and incomplete anæsthesia?—Yes, he says so most distinctly. He uses the words "complete anæsthesia," and defines it.

11030. How does he define it?—He says "The animal suffers no pain, because complete anæsthesia is maintained from before the commencement."

11031. That means that complete anæsthesia does produce it, but he was not contrasting complete with incomplete there. That was not the point he was on?—No.

11032. Supposing he had used the words "The animal was in a state of complete insensibility to pain; that probably would have been expressing what he meant there. If you asked him to define the difference between complete and incomplete anæsthesia, and asked him whether the animals were always under complete anæsthesia in the strict sense of the word, or sometimes under complete anæsthesia without suffering pain, he might have given a different answer?—I think I am entitled, when a Government Inspector who is their adviser uses the phrase "complete anæsthesia" as being his way of defining a condition without pain, to deduce that when he uses the term "incomplete anæsthesia," he is defining a condition not without pain.

11033. He does not use it as a definition, I think. He says that complete anæsthesia includes a state of being without pain?—I do not think he says that.

11034. That is what it comes to. I do not think he is using it as a definition. We seem to be on very minute points with these verbal criticisms?—Very well, my lord.

11035. (Sir Mackenzie Chalmers.) I think I asked you whether you were aware that Dr. Crile's experiments were most of them seen by Dr. Goodbody?—You told me so. I was not aware of it before.

11036. Did you see a letter from Dr. Goodbody in "The Times" of the 13th March, 1902, who was present at the experiments, and gave his account of them?—No, I did not—not that I remember. I have something to say about Dr. Crile before I pass away from him.

11037. By all means say it?—I understand that one member of the Commission has a letter from other people, and has received evidence from other people who were present, all affirming that no pain was inflicted on the animals on this occasion. All I wish to point out to the Commission is this: There is no doubt that Dr. Crile in his own book admits that when he applied flame to the dog's paw, as he said, "The animal struggled on the application of the flame." That is a very definite statement by him.

11038. Was that one of the English experiments?—No, I quite admit that it was not an English experiment, so far as I know. It is not one of the first sixteen.

11039. (Chairman.) Then it is not one of those experiments about which you complained to the Home Office?—No; but I am only pointing out this, and I think it is *ad rem* in this way, to show that Dr. Crile is a person who does not hesitate to inflict what in my opinion is torture upon animals. He applied flame to the dog's foot, and the dog struggled on the application of that flame. I call that torture. There you have it under the man's own hand, the moment he gets out of England, that he does a thing which is undoubtedly torture of a most gross kind on an animal. I say under those circumstances, the whole book being before the Home Office, when they received that letter of excuse they ought to have taken into consideration what he did out of England.

11040. Had they got the book before them?—Yes, I placed that very experiment before the Home Office myself, and drew attention to the expression "flame was applied to the dog's paw."

11041. (Sir Mackenzie Chalmers.) When you called the attention of the Home Office to it, it was some time afterwards, and Dr. Crile had long left England?—Yes.

11042. What do you suggest that the Home Office could have done?—But my letter to you was before you consulted Dr. Crile as to what he had done; therefore, when you received his excuses you knew that he was a man who did torture animals, and his excuses, according to your own showing last time, were wholly incorrect, because he made a statement in the letter which you produced that was wholly false, showing that his evidence is not to be relied upon—first, because he was a man who did torture animals; and, secondly, because he was a man who, in his letter of excuse, was not a man incapable of making false statements.

11043. I should agree with you if Dr. Crile's evidence had not been confirmed by independent witnesses whom we have every reason to respect—Sir Victor Horsley

The Hon.
S. Coleridge.

3 July 1907.

The Hon. S. Coleridge. and Dr. Goodbody?—I only wanted just to get that out.

8 July 1907. 11044. Then I called your attention to Dr. Noel Paton's experiments on fasting, and perhaps it would only be fair if I read his own explanation in a letter written to Sir James Russell in response to a question as to how and why he performed these fasting experiments: "Dear Sir James Russell,—I suppose the experiments referred to by Mr. Thane in his letter of the 14th inst. are those published in 'The Journal of Physiology,' vol. xix., p. 192, *et seq.* (1895-96)." That is what you quoted from?—Well, Sir Mackenzie, if these letters are to be put in against me, I ought to have had them before I came here. It is very difficult for me to conduct my case from my point of view if things are thrown at me without my knowledge. This is a private letter, apparently, from Dr. Noel Paton to the Home Office. How can I deal with it without having it before me?

11045. (*Chairman.*) It is quite unusual for witnesses to be furnished with all the letters to be put to them. You must do the best you can, and we shall do the best we can?—It is very difficult for me, is it not?

11046. (*Sir Mackenzie Chalmers.*) "The experiments there described are upon the influence of fasting on the fats of the liver. I have always been most reluctant to inflict suffering upon animals, and before performing these experiments I gave them most careful consideration. I had already in 1888 studied the progress of a fast of thirty days undertaken by a Frenchman, Jacques, the results of which are published in the proceedings of the Royal Society of Edinburgh in 1889. I found that Jaques throughout his fast suffered no pain or distress—except for a curious attack of gout in the middle of the period."

11047. (*Mr. Tomkinson.*) He had water to drink, I suppose.

11048. (*Sir Mackenzie Chalmers.*) Yes, certainly he had. "I had also read the accounts given by Professor Luciani of a similar feat undertaken by an Italian, Succi (Das Hungern), and the descriptions of the fasts of Dr. Tanner in America and of Cetti in Berlin. In none of these did the abstinence from food, though extending over prolonged periods, cause inconvenience or suffering. Further, it is well known that many warm-blooded animals undergo prolonged fasts voluntarily and without suffering. Among mammals the male fur seal, I am informed, remains on shore in the breeding season sometimes for one hundred days without eating. Among cold-blooded animals very prolonged fasts are undergone without suffering or impairment of function, e.g., in the salmon, in fresh water. Food is simply required as a source of energy, and so long as the animal body is not depleted of its stored material there is no necessity of a supply of food from without. From these considerations it seemed evident that the experiments I proposed to perform were not in any sense painful, especially as the animals were kept warm and supplied with water throughout the period. I therefore did not think that they could be brought under the Act, and I did not apply for a certificate. Indeed I do not know what certificate would have covered them. For the purpose of my experiments more prolonged fasts were desirable, but my repugnance to inflicting even possible discomfort upon animals prevented my extending the period beyond the limit of four days. No one has a greater love of the lower animals than I have, and all through my physiological career I have never willingly inflicted pain. I am, yours very truly, (Sd.) D. Noel Paton." That is his explanation, whether you accept it or not?—I understand the case to be that Dr. Noel Paton said that he did not consider that these experiments were within the four corners of the Act at all, and the Home Office accepted that explanation. My answer is that if any one of the Commissioners starved his horse for four days he would be summoned, and properly so, by the Royal Society for the Prevention of Cruelty to Animals, and probably convicted and fined. The law of the land would call it cruel, and any magistrate in the country would call it cruel. Before you pass on there are two or three things which were left open when I went away last time, and I should like to deal with those if I might. You put to me, for instance, at Question 10,914 a question on the Association for the Advancement of Medicine by Research.

11049. Yes?—"Each licence and certificate goes to them. Are you aware what happens when they

come back? Are you aware that certificates are continually altered and modified and sometimes refused?" and I say, "My point is that so far no evidence has been offered here before this Commission to show that the Home Secretary has ever gone contrary to this Association till the Commission was sitting." Then Sir William Collins, at Question 3,873, asked Mr. Starling: "Do you know whether the Home Secretary ever acts contrary to the advice of the Association?" and Mr. Starling says: "He has done so lately, I understand"; and it continues: "(Q.) In how many cases?—(A.) He has in two cases lately refused to allow certificates (Q.) During the present year?—(A.) During the last three weeks." That is what I based my assertion upon. I based it upon that evidence (I could not find it at the moment); and it is upon that evidence that I said my point was that no evidence had been offered "before this Commission to show that the Home Secretary has ever gone contrary to this Association till this Commission was sitting." The three weeks was within the period of the sitting of the Commission. I also want to point out before I leave this that Professor Schäfer himself, I see, in his evidence at Question 10,172 admits that the confidential reference to this Association in his own words is "clearly outside the four corners of the Act," though he states very naturally that he does not object to it himself. The next point is that there was a question and answer about Professor Schäfer drowning and redrowning dogs, which I could not find at the moment. I have found it and have it here; it is in "Hansard" of the 29th June, 1903. You will find the allusion to this under Question 10,925, etc., down to No. 10,928. It is as follows: "Colonel Lockwood: To ask the Secretary of State for the Home Department whether the certificate given to Professor Schäfer, in accordance with the provisions of the Act 39 and 40 Vic., c. 77, permits him partially to drown and then resuscitate the same dog without any anaesthetics more than once; and if so, how many times; or permits him partially to drown and then resuscitate the same dog without any anaesthetics only once; and if so, how many dogs has he permission so partially to drown and resuscitate." That is the question. Then Mr. Secretary Akers-Douglas answered thus: "The certificates obtained by Professor Schäfer for the purpose of testing the best means of effecting resuscitation in cases of apparent drowning are limited to ten experiments. It is not stated whether the experiments shall be on ten separate dogs, or whether the same dog will be used for more than one experiment; but I have ascertained that Professor Schäfer does not propose to submerge any unanaesthetised animal more than once." That is the question and answer I had in my mind when I said that the Home Office had permitted the drowning and redrowning of the same dog, but that I had heard with pleasure that Professor Schäfer said he had not done what he had been permitted to do.

11050. There was another question and answer on the same subject. Mr. Perks asked a question on the general question of Professor Schäfer's drowning experiments. Have you got that?—I am very pleased to hear it. I do not remember it.

11051. "Mr. Perks: To ask the Secretary of State for the Home Department whether, as reported at a recent meeting of the Committee of the Royal Medical and Chirurgical Society, Professor Schäfer has obtained permission to conduct experiments on dogs without anaesthetics for the purpose of investigating the phenomena of death from drowning; is he aware that these experiments have hitherto been conducted under chloroform; and will he take steps to prevent the infliction of cruelty which the experiments authorised involved"?—Was this after the other one?

11052. No, this was before the 15th June, 1903?—About a fortnight before.

11053. And Mr. Akers-Douglas in reply said: "Professor Schäfer has obtained the certificates required by the Act to enable him to perform certain experiments on dogs for the purpose of investigating, not the phenomena of death from drowning, but the best means of effecting resuscitation in cases of apparent drowning. The experiments will be performed on behalf of the Royal Medical and Chirurgical Society of London, who have appointed a committee to consider this question. I am aware that Professor Schäfer has performed experiments on the effect of immersion on anaesthetised dogs, but I am informed that the use of

anæsthetics frustrated the object of the experiments. In view of the great importance of the subject in connection with the saving of human life, and of the strong recommendation received in support of the experiments, I have not felt justified in disallowing the certificates?"—The next point I want to point out is this. I could not find the number, Volume XV., of the *Journal of Physiology*. At Question 10564, Lord Selby asked: "You spoke of keeping the animal under anæsthetics first while it continued lying on the table," and so forth. "And then, in another place, you added the words 'until the wound has been aseptically closed.'" I say "I have not got the volume here now, I am afraid." I have got it now. This is an experiment by G. N. Stewart: "Experiment 2. Bitch, 3,708 grams. 50 mg. curare subcutaneously. Artificial respiration. Exposed right internal jugular vein, right carotid artery and right vago-sympathetic nerve, below thyroid gland—carefully separated right vagus from sympathetic for some distance. Cannula in left external jugular vein." There is no specific mention of anything in the way of anæsthetics, but 50 miligrammes of curare subcutaneously injected. This experiment is described as beginning at 2.2 in the afternoon, and it goes on till 8.14, and the point I want to make is this. There are curious notes all along down the side; I should like the Commissioners to look at this. It has been going on since 2 o'clock, and at 6.31 there is a note at the side "Curare action passing off; twitching of muscles." It does not say anything more was done to the animal to stop those twitchings or to allay pain, which I presume was beginning to be severe, or was severe all the time for all we know; but at 6.53 "injected 25 mg. curare into blood"—no doubt to keep the animal still.

11045. (*Colonel Lockwood.*) Where was this experiment?—I do not know where it was. It was done by G. N. Stewart, a licensed vivisector. He wrote several accounts of these experiments, and the difficulty is that he puts at the heading of this paper, which contains a great number of them, "From Physiological Laboratories, Cambridge and Strasburg." As he does not say which were done at Cambridge, in England, and which were done at Strasburg, if we convict him of cruelty—I do not say we do, but supposing he is convicted of cruelty in any one of these experiments, it is very easy to say that the pea at that time was under the Strasburg thimble, and not under the Cambridge one.

11055. (*Dr. Gaskell.*) If you look at the end of that paper, I think you will see a statement by Dr. Stewart on that subject?—Yes. "The above paper is based upon experiments made by the author while holding the George Henry Lewes Studentship in the Physiological Institute at Strasburg. The results were embodied in a report to the Government Grant Committee of the Royal Society, dated March 22nd, 1892. The paper formed part of a thesis sent in for the Goodsir Memorial Prize in the University of Edinburgh, April 30th, 1892, of which an abstract was read before the Physiological Society, June 25th, 1892." He says it again. "The experiments in the above paper were made," and so on. "In all the experiments performed in the Physiological Laboratory at Cambridge, and in most of those done in the Physiological Institute at Strasburg, the animals were anæsthetised with chloral, morphia, urethan, chloroform, or ether. When curare was given, it was generally in addition to one of these anæsthetics"—generally, not always.

11056. And in all the experiments in Cambridge the animal was anæsthetised?—No. "In all the experiments performed in the Physiological Laboratory at Cambridge, and in most of those done in the Physiological Institute at Strasburg, the animals were anæsthetised." What with? "Chloral, morphia, urethan, chloroform, or ether. When curare was given" (this holds with all of them, I presume) "it was generally in addition to one of these anæsthetics."

11057. (*Chairman.*) But if that statement is taken as a whole, it means that it was never administered without anæsthetics at Cambridge?—I quite admit that it is impossible to prove that any one of these experiments was not done at the Physiological Laboratory at Strasburg, but my point is that anybody who reads the account of this experiment through would think—I do not set up to be a physiologist, but it seems to me, and I have been advised (just as much as the Home Office has been advised)—that this is a most dreadful experiment. There are two points that I want to make on this experiment. My chief point is

that, even if this were done (as it was admittedly done somewhere) at Cambridge, this could be done under Certificate B with perfect legality. That is one point which I wish to raise, because Certificate B permits animals to recover from anæsthesia so soon as the initial operation of the experiment has been performed. The initial operation here is carefully described, and after that there is no further operation upon the animal, so far as I can see by the notes at the side, from 2.2 to 8.14. We do not know whether the animal died, or what happened.

11058. (*Colonel Lockwood.*) I want to know whether curare was administered, according to the paper, alone, or in conjunction with anæsthetics?—According to this specific statement, there is no mention of any other anæsthetic, but there is this general statement, which seems to cover them all, in a sense. I admit it is a covering statement.

11059. (*Chairman.*) The statement is that if it was done at Cambridge it was done with an anæsthetic?—No, I say it need not have been so.

11060. Unless you say he is not speaking the truth; because he says expressly that in every case anæsthetics were used at Cambridge?—Yes, no doubt it was for the preliminary operation—the initial operation.

11061. Therefore, if he says that in some of the whole mass of cases curare was administered, then that means that in all the Cambridge cases, wherever curare was administered, it was after the anæsthetic; because he says that in every case at Cambridge there was an anæsthetic?—If he says that an anæsthetic was used in all the experiments at Cambridge, I am not prepared to deny it. But I say that if he had Certificate B, as I presume he had, he was only bound to use anæsthetics during the initial operation.

11062. That is not the point. I was speaking of Colonel Lockwood's point. He wanted to know whether curare was used without an anæsthetic. I say that if curare was used at Cambridge, the statement is that an anæsthetic was used with it?—No, not necessarily during the period of observation.

11063. I am speaking of one operation, which is all he is speaking of. I do not know whether he says anything about using one or other anæsthetic during the period of observation?—That is the point.

11064. (*Dr. Gaskell.*) Were not these experiments done under licence? There is no Certificate B, so far as I know?—I do not know.

11065. (*Chairman.*) You seem to me to be making too much of this, and trying to get more out of his statement than it will bear?—No, my Lord; I am upon this: that Certificate B, as now issued by the Home Office, permits of any amount of torture, and I cite this as an instance.

11066. I was on what Colonel Lockwood put to you, and you answered it by giving an answer which seemed to me to relate to another matter?—To resume, what I mean is this, that this experiment was done either at Strasburg or Cambridge; if done at Cambridge, if he had Certificate B, it is perfectly illegal.

11067. (*Sir Mackenzie Chalmers.*) But had he Certificate B?—I will look and see.

11068. (*Mr. Tomkinson.*) Is your contention that if the word had been "invariably" instead of "generally" that would have covered it?—No, because when he said that he used anæsthetics that is perfectly true as regards his statement, because he would use it for the initial operation if he had Certificate B.

11059. But he says that when curare was administered it was generally in addition to an anæsthetic. If the word "invariably" had been used, it would have satisfied you that curare was never used without an anæsthetic?—During the initial operation only.

11070. (*Chairman.*) You are losing sight of the other part of what he says—that anæsthetics were always used at Cambridge. If this was done at Strasburg, that is another matter; he may have used curare without anæsthetics. But if it was done at Cambridge, it was an operation under the Act, and there must have been anæsthetics?—I am not citing it for that point. I cite it as an illustration of my contention that under Certificate B any amount of torture can be inflicted on animals in England under the present law.

11071. That is not immaterial at all, but it is not the question in point?—That is what I brought it up for.

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The Hon. S. Coleridge. (Dr. Gaskell.) That is a question which we shall come to afterwards.

3 July 1907. (Witness.) My next point is this. Since I was here I have placed my Bill once more before my committee, and I have incorporated the various little alterations that were suggested. For instance, I have incorporated in Clause 3, about inserting matters into the eye, these words: "There shall not be applied to the eye of an animal by way of experiment any matter or substance." Then I put the words "calculated to give pain," which brings it up to and makes it more what I meant it to mean. Then also, in deference to something that was said, that in Clause 6, that the names of the gentlemen who signed the certificates would not necessarily by law be published, I have inserted, after the words "Secretary of State," in the third line, "and three copies of each of the two certificates hereinafter mentioned made in support of the application for the said licence if so granted"; and also in line 31, after the word "reports," "and certificates" should be deposited. That incorporates what Sir Mackenzie pointed out to me was an omission in the Bill. I laid it before my committee with those alterations on Monday, and they passed it with their entire approval. Then there was an allusion to the picture of a dog at Khartoum. Sir Mackenzie said he had not seen it; that is at Question 10842, so I have brought it here (*producing the same*).

11072. (Sir John McFadyean.) It was not done in England?—No, at Khartoum, under English Government.

11073. (Sir Mackenzie Chalmers.) The dog suffering from sleeping sickness?—Yes.

11074. (Mr. Tomkinson.) But that is put in as typical of the effects of inoculation, and as a case which may occur equally well in England?—Yes.

11075. (Sir John McFadyean.) Might I ask whether that picture is exhibited in order to convince the Commissioners that the dog must have suffered pain? Do you wish us to accept that as evidence that the dog must have suffered pain?—I should not care to have any dog that belonged to me reduced to that condition.

11076. That is not the question?—I should say that it was a condition of suffering, certainly. To the ordinary lay eye it is; it may not be so. To the ordinary lay eye it looks like a dog in a condition of suffering. I think that a man in England who reduced his dog to that condition outside his laboratory would be prosecuted by the Royal Society for the Prevention of Cruelty to Animals successfully, if the dog was seen in court.

11077. But, I suppose, as a layman, you would admit that there are absolutely painless diseases which reduce dogs to such a state of emaciation as that?—That is a question for an expert. I do not know. I daresay there are, if you say so.

11078. I ask whether you suggest the contrary?—I should myself say, as a layman, that evidence might be brought before the court that it was a perfectly painless condition, by experts. That would be for the judge and jury to decide.

11079. (Dr. Gaskell.) Is not that the characteristic of sleeping sickness—emaciation without pain?—That I do not know.

11080. (Sir John McFadyean.) So that it does not, as it stands, prove that this animal suffered?—I do not say it does.

11081. It is a perfectly reasonable supposition that this dog suffered no pain, in spite of the emaciation?—It seems always the supposition that any animal vivisected for anything suffers no pain. It is a supposition, I suppose, that can be accepted for purposes of argument.

11082. (Sir Mackenzie Chalmers.) With reference to your statements about Sir Victor Horsley, are you aware that he applied to the Home Office to be allowed to perform certain experiments with regard to disease caused by *filaria sanguine hominis*?—How could I possibly be aware of that?

11083. Which only arise at nighttime; and he asked, therefore, to be allowed to experiment outside laboratory hours?—You put that in the form of a question to me, but you know perfectly well that I know nothing about it.

11084. You complain of the permission being given,

and I was asking if you knew the reason why it was given. You do not know?—Of course, I do not.

11085. (Sir William Collins.) Have these applications and the permission given thereon been published in any official return?—No, none.

11086. (Sir Mackenzie Chalmers.) You know the fact, but you do not know the circumstances which lead to it?—I am perfectly indifferent, and so are those whom I represent, as to what reasons may be assigned on which Sir Victor Horsley asked to be permitted to be placed outside the provisions of the Act. My complaint is that Sir Victor Horsley was given permission to vivisect in a place outside the region of inspection, and therefore outside the Act altogether, which contemplates that every place should be inspected.

11087. Are you aware that Mr. Asquith attached a condition to that leave that Sir Victor Horsley should report to the Inspector on each occasion when he performed any experiment at any but a registered place?—No, I am not aware of it.

11088. If you are not aware of it, why complain?—I have stated my complaint, and will state it again. It is immaterial to us, or to anybody whom I represent, for what purpose Sir Victor Horsley was placed outside the purview of the Act. Our contention is that no vivisector should be placed outside the possibility of inspection, and that the Act of Parliament did not contemplate anything of the kind, and did not intend it to be so.

11089. (Chairman.) Do you say that it is contrary to the Act?—We say that there is no reason in the world why Sir Victor Horsley should be placed by the Home Office outside the possibility of inspection. In the case of persons allowed to place grouse on a moor for the convenience of experiments, that is a different thing.

11090. This was not a case of that kind?—No, I understand that it was not.

(Sir Mackenzie Chalmers.) This was an inoculation experiment.

11091. (Sir William Collins.) Are the endorsements or conditions of a licence published in the official returns?—Never; the Act is administered, if I may say so, in the strictest secrecy.

11092. (Sir Mackenzie Chalmers.) Then you raise a point as to Professor Schäfer's certificates. I can only put it to you for the purpose of clearing it up. You applied to the Home Office to know by whom Professor Schäfer's licence was signed?—Yes.

11093. I do not know that it is very material. May I put it to you in this way: That his application for a licence was signed by Sir Joseph Hooker and Sir John Burdon Sanderson, as you said?—That is a new name; I never heard of Hooker.

11094. And Burdon Sanderson?—The Home Office said that Sir John Burdon Sanderson signed his certificate.

11095. That is so?—That is my statement.

11096. Then his lecture certificate, Certificate C, was signed by the same people. Then he afterwards held various certificates from various people for special researches?—My contention was that it was impossible for the public to ascertain by any means published by the Home Office who signed these certificates. When we tried to investigate it the Home Office seemed confused, and could not tell us. The Home Office said one thing, and Professor Schäfer said another. The two statements are contradictory.

11097. They are not contradictory. One referred to Certificate B and the other to Certificate C?—There is a fourth man in now, Hooker. I never heard of him before.

11098. You objected to Sir William Harcourt asking the advice of the Society for the Advancement of Medicine by Research?—I do not put it upon Sir William Harcourt. It was done in his time.

11099. At any rate you objected to it?—Yes.

11100. I want to ask you this: Do you think it would be better if, say, a committee of the Council of the Royal College of Physicians was substituted for that society?—That is a very large question. I am not the adviser of the Home Office.

11101. I am asking you?—I am making the com-

plaint. If you ask me to say at a moment's notice I decline to give an opinion.

11102. I ask you whether, in your opinion, it would give a greater sense of security to you and your society?—The sole reason why all those who sign certificates sign them, so far as I understand it, is as testimonial to the scientific fitness of the applicant. Their scientific fitness is of no interest whatever to myself or my Society. Our interest is in the animals and their fate; and it is a matter of perfect indifference to us whether these gentlemen have scientific fitness or not.

11103. But the Society for the Advancement of Medicine by Research do not sign certificates; they are merely consulted. I am asking you whether you would have more confidence, for instance, in a committee of the Royal College of Physicians or a committee of the Royal Society of Medicine, if they were consulted in lieu of the Society for the Advancement of Medicine by Research?—I should have much more confidence if any body was consulted upon which there were no licensed vivisectioners sitting. This society that you do consult is a society composed very largely of licensed vivisectioners.

11104. (*Chairman.*) Then you would not approve of the Royal College of Physicians?—Not if the persons sitting on that committee to give that advice were licensed vivisectioners.

11105. (*Sir Mackenzie Chalmers.*) But if you had a committee of the Royal College of Physicians who were not licensed vivisectioners?—I should certainly regard it with greater respect. I think they would very likely take into consideration the humanity of the applicant. But even then they are not asked that. I have never had any evidence before me that the Home Office ever ask the persons to whom they refer what the humanity of the applicant may be.

11106. It is not the Home Office that grants certificates. The certificates are granted first, and the only question is whether the Home Office shall disallow them?—Yes; you can veto them.

11107. However, that suggestion does not approve itself to you?—I do not say that.

11108. Is there any body with expert knowledge that you can suggest in preference to the Society for the Advancement of Medicine by Research?—I should say that any body is fitter than the Society for the Propagation of Vivisection, which is really a better term for them.

11109. (*Colonel Lockwood.*) You have got no definite body in your mind at the present minute?—No, I am not interested in the scientific side of the matter. These men only testify to the scientific fitness of the person. If you ask me who are the right persons to ask as to the humanity of the applicants, I think that a committee of the National Anti-Vivisection Society would be a very proper body. It is just as public a body as this private association is, and just as able to give proper consideration to the humanity of the applicants as that other body is to consider their scientific fitness.

11110. (*Sir Mackenzie Chalmers.*) You raised the point, and it is a material point, about prosecutions under the Act. At present, as you know, all summary prosecutions are limited by six months?—Yes, by the Summary Jurisdiction Act.

11111. You suggest a longer period?—I suggest it, because, as you are aware, at present the yearly report, which is one of our sources of information, seldom comes out till about three or four weeks before that six months has elapsed; so that there is no possibility that a prosecution can be based upon that report, even if it revealed any breaches of the law. And, again, this short six months precludes us from taking advantage, as a rule, of any publications of the vivisectioners, if by a slip they let out that they have broken the law, because they do not, as a rule, publish them until six months after the experiments.

11112. But it would, of course, be applying a special law to vivisectioners which is not applied to the rest of the community; it would be an exception to the general law?—I do not see why. There is a definite reason that I give you, which, I think, is a reasonable one, and I think anybody ought to see it is a reasonable one. I do not imagine that even the Home Office want to place vivisectioners beyond the possibility for indictment for an offence under this Act.

11113. Certainly not?—I am not sure that putting

them under the Summary Jurisdiction Act is not an exception in their favour. *The Hon. S. Coleridge.*

11114. They are not put under it. It applies universally, does it not?—I do not know. *3 July 1907.*

11115. As regards the prosecution of licensees, you do not think it is the duty of the Home Secretary first to give leave to prosecute, and then himself prosecute?—I think the provisions of my Bill are more desirable.

11116. But under the existing law you would hardly contend that the Home Secretary should give himself leave to prosecute, and then himself prosecute?—I think he should prosecute in cases where the law is broken.

11117. When he has given the licence?—Yes.

11118. Inasmuch as under the Act the prosecution requires his leave, do you think that the Act contemplated that he should give himself leave and then prosecute?—I should have thought it was possible for the Home Office to move in prosecutions. I should have thought that they could direct a prosecution against the man. I should be surprised if they cannot.

11119. Under that Act, wherever the Home Secretary has to give leave, do you think the Act contemplates that he should himself prosecute?—Why not? I see no reason against it.

11120. That he should simply say, "I hereby authorise you, Herbert Gladstone, to prosecute," and then proceed to prosecute?—That is mere formality.

11121. I will take your answer upon that?—It would not be a formality which would frighten me if I were Home Secretary.

11122. You are aware that the Home Office has no legal department whatever?—I daresay.

11123. In how many cases have your Society asked for leave to prosecute? Are you aware of any case?—Not since I have been there, of course, because there was no possibility to get material for prosecution under the present law.

11124. You have not applied?—Naturally; how could we get the material to prosecute?

11125. There is only one small point I want to ask you about as to Dr. Grünbaum's experiment. Do you know that he was experimenting at that time under his certificates with antivenin?—How could I know that? That is your evidence. It is not a question to me.

11126. I am asking you whether you do know it?—Of course I do not.

11127. Was not all that happened this: that after a lecture in his own laboratory he went on with an experiment he was conducting, in the presence of certain chemists?—That may be your version of it. I was not there. That is not the account which was published in the papers.

11128. And, apart from any technicality, what is your objection to these chemists seeing the experiment?—My personal objection, you mean?

11129. You make it a ground of complaint?—I am here in an official capacity.

11130. I say (identifying you with your Society), is it not better, if experiments are performed, that they should not be performed wholly in secret?—Unfortunately, when I complain of that (because at present we are under the Act 39 and 40 Victoria, cap. 17), and I say that an illegal act was done under that Act, the Home Office take no notice of it. The Home Office cannot defend themselves by asking me whether, in my opinion, the law is wrong or right.

11131. I say, apart from technicality (I am quite prepared to deal with the technicality), you allege a substantial breach of the law?—Certainly.

11132. You said that it was illegal that these experiments should be seen by skilled persons—chemists. They are not the general public?—They are not physiologists, I think.

11133. They are persons who would sell antivenin if antivenin came into use?—My point is that an illegal act was committed, and the Home Office condoned it. That is my point.

11134. That is a matter of dispute, of course?—Yes. I only say that that is my point, for what it is worth.

11135. Then you complained of some experiments performed in Ireland. You know that the Irish Office

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and the Home Office have no connection?—I know that they are all under the Act.

11136. Under the Act—yes?—I suppose there is no connection under the Act. There is no connection in that sense.

11137. There is no connection whatever. Therefore, you do not hold the Home Office responsible for what is done under the Irish Government?—I am very glad to acquit you, Sir Mackenzie, of Professor Shaw. When I brought that forward, I was not bringing it forward as an attack on the Home Office.

11138. I only asked, because, as you would say, unfortunately, from your point of view, both the Home Office and the Irish Office take the same unfortunate view of their duties under the Act?—In that case they certainly did take the same unfortunate view. I think in that case, as a matter of fact, the six months' limit prevented the Home Office from doing anything. I do not think the Home Office knew about these experiments until we drew their attention to them.

11139. The Irish Office, if you please?—Yes. At that time the six months was out, and I do not think they could have done anything with the best will in the world. I think they said so in their letter to me. I am only judging by memory now.

11140. (Mr. Ram.) With regard to your attitude generally, as I understand, your attitude and that of your Society is one of regulating the practice of vivisection as much as you possibly can?—Yes.

11141. That, of course, is a view that we can appreciate and sympathise with much more than with some views which others have given us. With regard to that, your Bill, which I will deal with in a moment, proposes certain alterations of the existing law with that view?—Yes, with that view.

11142. With regard to the past, the present Act is 30 years old, and the law has been administered under that Act for that length of time?—Yes.

11143. Your Society is older even than that?—About contemporaneous; two months before, I think.

11144. And your Society, no doubt, during the whole of that time has been composed of persons active and anxious in every possible way to repress any cruelty or stop any illegality?—Certainly.

11145. Under the Act the powers granted to private persons to put the law in operation are found in Section 13, under which a person may apply to a Justice of the Peace if he believes that any act has been performed by an unlicensed person in any place not registered under the Act. Has any application ever been made under Section 13, under which a person may apply to a justice for such warrant to examine any premises under that section?—Not that I am aware of.

11146. Has an application ever been made under Section 21 to the Secretary of State to allow a prosecution against a licensed person who has been doing an experiment for any reason other than that allowed by the Act?—Not since I have been honorary secretary, certainly.

11147. Is that because you have never had any case which, in your opinion, would justify such a request being made?—Oh, no, it is because under the way in which the Act is administered, and within the four corners of the Act, it is impossible for us to find out what happens inside the laboratories.

11148. Let me sever the two things for a moment, under the Act and under the administration; I am coming to the administration shortly. Under the Act you say that you have never been able to ascertain any instance which would have warranted anyone on your behalf making an application for a prosecution?—I do not think I have ever known of any instance, which I thought was a breach of the law, which was published until after six months.

11149. Therefore there has never been any such application made?—No.

11150. With regard to publication being made six months afterwards, you have given us a list of five or six different cases in which there has been, in your opinion, a breach of the Act, and as to which you have found fault with the action of the Home Office; those are the cases that you gave us in your evidence the time before last. You will recognise them by name. There is Crile's case, Grünbaum's case, Starling and

Bayliss' case, Cecil Shaw's case, Noel Paton's case, and Dixon and Brodie's case?—Yes.

11151. Those are the cases which you gave?—Yes.

11152. With regard to those cases, you believe that there has been some contravention of the Act?—I should like to look at each case. The *gravamen* of my attack is that in each case when I have complained to the Home Office of actions done which seem to me to be inhumane, the Home Office have accepted the *ipse dixit* of the person whose conduct was impugned. It may not necessarily have been a breach of the Act.

11153. I think in each of those cases you did allege that there had been a breach of the Act, and you called the attention of the Home Office to it?—If you would give me them one by one, I can answer you.

11154. Crile's case was the case in which you said that there was an operation performed under incomplete anaesthesia?—Yes.

11155. That would have been a breach of the Act?—In that case pain was inflicted under the licence.

11156. Yes?—I should say that that was a breach of the Act.

11157. In Grünbaum's case, he performed an experiment, as you allege, before the public?—Yes.

11158. In Starling and Bayliss' case you do not doubt that there was an allegation of illegality?—Yes.

11159. And that was established largely—I mean the allegation?—I ought to distinguish now. My assertion is now that Professor Starling broke the Act by not killing the "brown dog" before he gave it over to Mr. Bayliss. That has never been before the court.

11160. I quite gather that. In Dr. Cecil Shaw's case it was an alleged breach of the law; that is "my work"?—Yes.

11161. Then Dr. Noel Paton's was the case of fasting experiments?—Yes. There is a little distinction in the case of Professor Noel Paton. Professor Noel Paton asserted that these experiments were outside the Act.

11162. Quite so?—That is not a breach of the Act. His defence was that they were outside the Act, and I represented that they should be within the Act.

11163. And Dixon and Brodie's case was a case of incomplete anaesthesia?—Light anaesthesia.

11164. Those are the cases that you brought before us?—Yes.

11165. Were those all the cases that you reported to the Home Office as having been, in your opinion, contraventions of the Act?—I could not say that.

11166. At all events, they are all that you have brought before this Commission?—Yes.

11167. In all or in any of those cases, do you allege that any suffering was caused to the animal by reason of the alleged breach of the Act?—Yes.

11168. In which case?—In Dr. Crile's case, for instance, certainly.

11169. Dr. Crile's was a case of incomplete anaesthesia?—Yes.

11170. You are aware that in that case, whether the animal suffered pain, turns upon whether the anaesthesia was or was not complete to the extent of removing all such pain?—Yes.

11171. And if, as Dr. Crile asserted, and as the bystanders seem to have asserted, the animal was insensible to any feeling of pain, then there would be no pain caused by a breach of the Act?—If they were accurate.

11172. With regard to any other cases, do you allege suffering being caused by an alleged breach of the Act?—Most decidedly to the case in Ireland—shoving shot into a rabbit's eye and watching it for six months.

11173. The Irish case I have purposely avoided; I was dealing with the cases reported by you to the Home Office?—In Dr. Noel Paton's case, of course, I assert that to starve an animal for four days is a painful thing.

11174. Eliminating the case of Dr. Noel Paton for a moment, as to starvation, are there any other cases in which the alleged breach of the Act involves suffering to the animals?—You want to know which of those cases are both illegal and painful?

11175. Yes?—If you will rehearse them again, I should like to have them separately. I have answered as to Crile and Noel Paton.

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11176. And as to Grünbaum?—Yes, Grünbaum's experiments certainly were painful.

11177. In what way were they painful?—He describes the rabbit after 30 minutes dying in convulsions of snake poison.

11178. In that case the animals were being legally operated upon under the Act, save in so far as the operation was public?—Yes, but you ask me whether they were painful, and I say that they were.

11179. What I put to you was whether the illegality increased the pain?—No, not the illegality.

11180. Then I think it comes to this, that except in the case of Dr. Noel Paton starving animals, the illegality did not cause pain to the animals—did not cause any increase of pain?—You mean, if you accept the defences?

11181. Yes?—That is so, if you accept the defences.

11182. May I take it that in all the years during which your Society has been investigating these matters, those are really all the cases, as to which you have brought before the Home Office allegations of impropriety or contravention of the Act?—I am not prepared to say that. I have brought forward a few of them. I do not think I could say that I have brought them all.

11183. Would you let us have any others which you consider important. I take it that even if these are not all, you brought before this Commission those which you regard as the most serious?—I have got instances, which, to my lay mind, seem to involve a great deal of suffering, but not illegality.

11184. I am pointing out that these are the only matters which you have brought to the notice of the Home Office as being illegal?—Possibly. I am not prepared to say that offhand. I have only brought a few of the cases.

11185. But you have brought, no doubt, the cases which seem to you the most important. You have not given us the worst of your cases.

11186. (*Chairman.*) Was it your intention, in going through them, to bring before us all the cases in which there was illegality?—I am constantly corresponding with the Home Office, I am sorry to say.

11187. If you say that it was not your intention, that is an answer?—I am not prepared to say that these are the only cases in which I complained of illegality.

11188. That was not the question. Are there others in which you complained of illegality?—I think very likely.

11189. Do you know of any others?—I have a very bad memory. I cannot say offhand. Sir Mackenzie Chalmers would be able to say.

(*Sir Mackenzie Chalmers.*) I quite agree that you did write about others; but these are like the strawberries—the best at the top.

11190. (*Mr. Ram.*) We can only deal with those that you have put before us. Everyone of these matters was, in your opinion, and in the absence of explanation, a technical breach of the Act?—Yes.

11191. With regard to the case of the chemists, your allegation there is that there was an illegality, because the experiment was performed before an Association of Chemists, which constituted the public?—Yes.

11192. You have been asked whether you had ever witnessed an experiment yourself, and you said No, that you would not assist the physiological experimenters in breaking the law?—That is so.

11193. Do you think it would be an advisable thing, in the interests of humanity, if some such person as yourself, say, who takes an interest in humanity, did witness some of these experiments from time to time?—Most certainly.

11194. Have you been precluded from doing that in the past by the Act?—Yes.

11194A. You think that it would have been a breach of the Act?—Yes.

11195. Let me refer you to the part of the Act which deals with that matter. It is Section 6, the words are: "Any exhibition to the general public, whether admitted on payment of money or gratuitously, of experiments on living animals calculated to give pain shall be illegal." Do you think that when you are finding fault with an experimenter, if he said to you

"Come and see what I do," which I understand is what you have been asked?—I have been asked.

11196. In your opinion would it be a breach of the Act if you, as one of the public, went to see it?—Yes. I have always thought so.

11197. You do not arrogate to yourself that you are the general public?—I am one of the public.

11198. (*Chairman.*) The question is, whether it would be a breach of the Act on the part of the operator if he allowed it?—Yes.

11199. (*Mr. Ram.*) But you said that you would not assist him in that breach?—Yes, I feel that I should be assisting him in that breach.

11200. I can understand it, but I cannot agree with it. May I put it to you in this way, that if you had gone you might have done infinite good from the point of view of humanity?—I do not think that any animal would be subjected to torture in my presence by any vivisector. That is why I want the Inspector always present. My presence would have the same effect, I think, as the presence of a Government Inspector.

11201. At any rate, you would have known much more about what was done in these experiments if you had been present?—Yes.

11202. But I understand that that was your reason, and the only one?—If I was present they would know that I was present.

11203. May I ask whether, had it not been for this section of the Act you would have gone?—I do not think that I should have served any general purpose by going, unless I had a right to go when I liked. To go by invitation would mean nothing. Of course, I should see no infliction of pain on an animal, being a more or less well known anti-vivisectionist.

11204. You would have learned a great deal of the circumstances under which experiments took place?—No doubt.

11205. Your evidence, which I have gone very carefully through, is something like, I may say nine-tenths of it, an indictment, and a long and acrid indictment, not of the Act but of the administration of the Act by the Home Office?—And of the Act also.

11206. I leave one-tenth to the Act?—Yes.

11207. There is an indictment with twelve different counts against the administration of the Act by the Home Office?—Yes.

11208. In your Bill you also propose that licences should be granted by the Home Office and the inspectors appointed by the Home Office and so forth?—Yes, I leave some of the Act of 1876 alone.

11209. But I think you leave all of it, so far as that goes. You safeguard it when they are appointed, but you leave their appointment to the Secretary of State?—Yes.

11210. The same Secretary of State whom time after time, man after man, you have distrusted so much in the past?—First of all in my Bill the certificates would not be merely as to the scientific fitness of the applicants, but also as to their humanity.

11211. I am coming to that afterwards?—And secondly, of course, the mass of operations will be performed in the presence of the Government Inspector.

11212. I am coming to that also. In the meantime the appointment and administration must, as you put it in your own Bill, be under the Home Secretary?—Yes.

11213. Now with regard to the certificate of humanity which you ask for in the eighth clause of your Bill, do you really attach importance to that?—Yes, very great importance; always provided that the names of those who sign the certificates are published.

11214. You will exclude, I suppose, any person who avowedly did not love animals?—Oh dear me no, I only want a person not to be unmerciful.

11215. I want, if I can, to get at something tangible. You want a certificate of a Justice of the Peace, or a Minister of Religion, for instance, that a person is humane?—Yes, that he is a person who would not torture an animal.

11216. There are a great many Justices of the Peace and Ministers of Religion. Should you really attach importance to two Ministers of Religion or two Justices of the Peace having signed a paper to say that they

The Hon. S. Coleridge. believed that the applicant was a humane person who would not torture an animal?—I should if their names were published, so that they were publicly made responsible.

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11218. If it was said *per incuriam*?—I should have put it in my Bill if I had thought it proper.

11219. Do you suggest that it would be a good thing?—I think that my Society is quite as fitted to advise on humanity as that other society is to advise on scientific fitness.

11220. Do you know and can you name any experimenter whom you would consider humane?—Yes, I believe there are some vivisectioners who are quite humane, and if it was my duty to find out who were humane, I should do my very best to ascertain it.

11221. And the statements of such men as that you would take as being accurate statements when they were asked what they meant?—Yes, if I was satisfied that they are humane.

11222. Then may I take it that you regard untrustworthiness and, to a certain extent, untruthfulness and inhumanity as being almost identical?—I do not quite say that. I say that if a man is inhumane and is then asked questions by the Home Office or by any authority as to what he has done, if he has been inhumane it is contrary to human nature for him to admit it. No man would in that case. If a man is inhumane he is driven, if he is asked what he has done, into a certain amount of inaccuracy no doubt.

11223. I do not quite follow that. Sometimes even dishonest prisoners plead guilty?—No doubt.

11224. (*Chairman.*) The certificate is not to be that the Justice of the Peace knows that he is humane, but knows that he has a reputation for humanity?—Yes.

11225. (*Mr. Ram.*) How does a man get a reputation for humanity?—That is as near as I can get it.

11226. I am only anxious to know how much importance you really attach to that?—I attach very large importance to the fact that these persons who, by signing, more or less undertake in the public eye that these men are humane men, should have their names published so that everybody should know who they are. It is a responsibility which a man will not lightly take upon himself unless he is quite sure.

11227. There are two or three detached questions which I want to put to you with regard to your evidence. In answer to Question 10517 you said that you take no interest in the object with which any experiment is made?—That is broadly my attitude.

11228. You said it once or twice; you used those words?—Officially I mean to say that my Society is interested absolutely from the point of view of the animal.

11229. But you do recognise, because I see that you put it in your Bill, very wisely I think, in Clause 2, Sub-section (1), that "the experiment must be performed with a view to the advancement by new discovery of physiological knowledge or of knowledge which will be useful for saving or prolonging life or alleviating suffering?"—That is in the old Act. I leave that.

11230. But you recognise there and you perpetuate, of course, naturally, that these experiments must have a humane object in view?—What I meant was, I may have been misunderstood, that when there is a question of severe pain, my Society does not recognise any justification of that pain by any lofty motive being asserted.

11231. I am glad you have answered the question, and I am glad to have that explanation. I want you now to look at your Bill, please, on two or three points. You say in Clause 2, Sub-section (4), "The animal must be killed while it is so under the influence of such general anaesthetic as aforesaid, and before it recovers therefrom sufficiently to feel pain." Is it your object to preclude all operations which, while painful in the inception, and therefore necessarily requiring an anaesthetic to be administered while they are being performed, are of such a nature that the result cannot be ascertained unless the animal be allowed to recover?—Yes.

11232. Then you wholly obliterate Certificate B and all cases under it?—That is our desire.

11233. That is your object?—Yes, we believe that under Certificate B most severe pain is inflicted.

11234. You do contemplate, and under your Bill you allow, the perpetuation of experiments not calculated to give pain at the initial stage?—Yes, those inoculations.

11235. But which may produce pain afterwards as the result of the inoculation?—Yes.

11236. Supposing that an animal has an operation performed upon it under anaesthetics, and is allowed to recover, and is treated, as we have been told here, as carefully with antiseptics, and generally treated as a human creature would be, so that there cannot be anything like severe suffering, do you think it is more objectionable to keep the animal alive under those circumstances than to allow the animal to live for a certain time, at any rate, while suffering from the effects of inoculation?—Under my Bill the moment the animal displays any sign of severe pain it is to be destroyed.

11237. I am coming to that in a moment. I quite see that that is your point. If the animal operated upon under anaesthetics and allowed to recover, making recovery under antiseptic treatment, was plainly not suffering severe pain, would you have any objection to allowing it to live?—It all depends upon the statement that the animal is not suffering.

11238. It does?—We are not prepared to accept absolutely the statement that all operations, however severe, leave no suffering behind.

11239. There has been no such statement made before this Commission or anything like it. The statements made before the Commission have been that in many of these cases, although the operation was painful, and therefore demanded anaesthetics in the performance of it, there was no subsequent suffering at all. But you say that you are not prepared to accept that statement?—I am not prepared to accept it.

11240. But under your Bill you provide that the Inspector should often see such animals, in the last sub-section of Clause 2, and that the animal is to be killed if suffering?—Yes.

11241. My object is to see how far your Bill would help us. Would it not be sufficient, in your opinion, to allow such operations as I am now indicating, the animal recovering, if the Inspector who saw the animal repeatedly satisfied himself that it was not suffering, or, if it did suffer, caused it to be killed?—I think that is outside.

11242. You see, my anxiety is this: You have said yourself repeatedly, and that is what has made your evidence, I think, on many points valuable, that you do not want to stop research?—That is so. We are quite misunderstood if anybody thinks that we want to stop science. We wish to promote science.

11243. You would wish, therefore, to make such provisions as, while safeguarding the animal, will secure the continuance of research?—Yes.

11244. I may take it, then, that you agree with me that if the Inspector sees the animals and certifies that they are not suffering, or causes them to be killed, if they do show signs of severe suffering, there might be operations under what we call Certificate B for short, which still might be permitted?—Yes, with this proviso. We are now talking of Certificate B, and under the present law my contention is that long before that period to which you now allude when the animal is healed—

11245. Not healed?—Is healing; and long before that under Certificate B, as now worded, atrocious suffering, in our opinion, can be inflicted upon it.

11246. At the moment I am on this point, whether you can suggest such a modification or, in your view, such an addition to Certificate B as will grant its existence in future, although perhaps modified in the way that you yourself wish?—If Certificate B could be so framed as to preclude such experiments as I have instanced of Mr. Stewart's—I am not agreeing for a moment with the description of Mr. Stewart's operation—it might be that the animal was under curare, for I do not know how many hours, and nothing else. It is such experiments as those that I am aiming at destroying in this Bill.

(Dr. Gaskell.) Those operations are under licence, not under Certificate B.

11247. (Mr. Ram.) That does not touch the point in one sense, but I want to follow this point. If you obliterate Certificate B altogether you do prevent an enormous number of investigations, the results of which cannot be ascertained, unless the animal is allowed to live for some time?—Yes, it must be so.

11248. And those operations, so far as this Commission has been instructed, seem to be the bulk of the operations which in the past have thrown most light upon the different humane objects which it has been desired to carry out. You accept that?—I accept that you have been told so.

11249. Then what I am anxious to get you to assist me in, if I can, is some such alteration or modification in Certificate B, as in your opinion might make it safe to allow it to continue safeguarded, rather than put a stop to all the investigations which at present can take place only under Certificate B?—Of course we are very suspicious of Certificate B, and it is very difficult for me, as representing my society, to say. I should only agree to its being continued provided that the animal after the operation is as much safeguarded as it would be under inoculation.

11250. That is the very point I wanted to bring out. Now with regard to inspectors, you, I suppose, realise that you are asking one of two things, either for an army of inspectors or for a very great diminution in the number of experiments that will take place?—Not so, necessarily. At present the Inspector, as I have said, wanders about on the chance of seeing anything, and wastes an enormous amount of time in that way. If every experiment had to be performed before an inspector he would never waste a minute; he would go from one place to another and witness experiments, and all his time would be occupied in actually seeing experiments, instead of wandering about the country seeing nothing as now.

11251. At all events I gather that there should be an inspector present at every physiological experiment?—Yes, but there are not so very many. If you leave out the inoculations the number is not very large.

11252. (Chairman.) Upon that point would it satisfy you if, instead of the operator being forbidden to perform an experiment unless the Inspector was there, a notice was given always beforehand that an operation would be performed at such and such a laboratory of such and such a kind; because there might be some purely insignificant operation which the Inspector need not attend. At any rate, there would always be knowledge on the part of the Inspector that it was going to be done at a particular time, and the operator might expect to see him coming in at any minute, but at the same time he might not be there in every case. For him to be there in every case would seem to me a tremendous interference?—We view it from the point of view of the animal. We feel that the animal is entitled to the presence of the Inspector when it is subjected to a severe operation, whatever it may be.

11253. A notice would not meet your view?—No.

11254. (Dr. Gaskell.) Do you mean that you would not allow more than one experiment to be made in a laboratory at the same time. There are often three or four experiments going on?—Do you mean in one room?

11255. No, in one laboratory?—Certainly not.

11256. You would not allow that?—No, I think that the Inspector should be present at every severe physiological experiment on an animal. He might go from one room to another.

11257. (Chairman.) You do not say severe in your Bill?—I mean any physiological experiment.

11258. You do not mean to include every inoculation?—No.

11259. Or every operation for which a licence was necessary?—No, a licence is necessary for inoculations.

11260. You mean any operations for which anaesthetics are necessary?—Any operations for which anaesthetics are necessary, certainly, because the presence of an anaesthetic means suffering, and therefore whenever there is suffering or the chance of suffering, our opinion is that the inspector should be present.

11261. (Mr. Ram.) While we are on the question

of suffering I think you want some modification of your Bill at the bottom of page 2. In the last two lines you provide that the animal may be allowed to recover, although inoculated, and you say, "Directly the animal begins to feel pain it must be placed, and thenceforth while it suffers or is likely to suffer must be kept under the influence of a general anaesthetic of the nature of a respirable drug or gas, such as chloroform, or ether, or alcohol, chloroform, and ether combined, of sufficient power to prevent the animal feeling pain; or, if the animal is not so placed and kept under the influence of such anaesthetic as afore-said, it must be forthwith killed"?—Yes.

11262. But in your evidence I observe that you say, and you have said just now, severe pain; you mean, I take it, appreciable pain?—I am not unreasonable. There is no pain gauge that I know of.

11263. You have said repeatedly, and said reasonably enough, in your evidence, for instance, in answer to Question 10765: "It will be the duty of the Inspector to go and see that animal frequently, and if it be in a severe condition of suffering it will be his duty to order it to be killed painlessly"?—Yes.

11264. You must therefore in your Bill modify that?—Where the suffering is really trifling, of course, I do not want to interfere.

11265. But you see we are or may be advising legislation here, and I want to see whether you can assist us in any way?—May I say serious pain.

11266. You would be willing to insert "serious pain" in line 36 at the bottom of page 2 of your Bill?—Yes.

11267. You have put it so clearly in your evidence in reply to Question 1078: "It depends entirely upon the amount of pain involved," and in the next answer, "Under my Bill it would be the duty of the Inspector to have that animal destroyed as soon as it manifested signs of severe suffering"?—Yes.

11268. That is what several Inspectors who have come before us here have said in answer to myself and others, that as soon as they saw any signs of appreciable suffering they would cause the animal to be destroyed. With regard to Clause 3 of your Bill—I remember the experiment which you told us of about the eye—do you think it is necessary to have a clause dealing with the eye expressly?—There is a very large feeling among the humane that it is a necessary clause in the Bill, because the eye is so frequently used, I believe, for the purpose of breeding bacteria and such things. That is the ground upon which we put it; and the eye is a very sensitive organ.

11269. Is it suggested that now in England matters which cause pain are inserted into the eye of anaesthetised animals?—I think so. I have seen pictures of diseased eyes.

11270. Would it meet your view to say that no operation of any sort shall be performed on the eye, save when the animal is under anaesthetics?—That would be so already. The eye is not put outside the Act. If an operation is performed on the eye it is as much under the Act as if it is performed anywhere else.

11271. I wanted to see whether you still think it necessary to except the eye from any experiment at all?—From inoculations we think that they should be eliminated altogether. Our view is that inoculations in the eye must always be painful—we may be wrong.

11272. Under Clause 4, Sub-section (2), you provide that every licence shall specify the time and place at which the experiment or series of experiments is or are to be performed. Is that with the object of letting the Inspector know?—Yes, and the public and everybody else; to lift up the veil of secrecy from these things.

11273. We have heard something about the veil of secrecy; but why on earth should the public know when an operation takes place?—Because this is part of the thing that is put up; this is part of the notice. It is the licensee who is to say when he is going to do it.

11274. I ask why?—In order that the Inspector may know.

11275. Then you eliminate the public?—Yes; I was not looking at it. "Every licence granted hereunder

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The Hon. S. Coleridge. shall specify the time and place at which the experiment or series of experiments is or are to be performed." That is in order that the Inspector may know for certain when it is going to be performed. I was thinking, and no doubt you were, of the putting up of the notice somewhere.

11276. I was. One other question on the Bill, and, I think, only one. You propose in Clause 9 that proceedings may be taken at any time within two years?—Yes.

11277. And yet you propose that a report shall be sent in forthwith?—Yes.

11278. If a report is sent in forthwith, how can it be necessary to have so long a time as two years within which to take proceedings?—I put both those conditions in, so as to get the longest possible time.

11279. May I point out that the object of shortening the time, as you know, in all these Acts is to preclude a charge being made against a person, when perhaps the circumstances are forgotten, or the evidence is lost, or some person is dead?—I thought that would be met by the provision that the prosecutor shall put down the sum of £50.

11280. Would that affect your Society, with its large income?—We should be the last persons to bring an action.

11281. (*Chairman.*) This is a question of instituting a prosecution?—Yes.

11282. (*Mr. Ram.*) Seriously now, supposing you got your way in having a report sent in forthwith, as to which we should want a definition, I think, but, say, within fourteen days, would you want so long a time as two years?—Perhaps that report might have omissions which might be supplied in a subsequent publication by the vivisector.

11283. You would have five and a half months, then?—That would depend upon when the vivisector published it. In the report sent to the Home Office, it is conceivable that the vivisector might leave out something accidentally or intentionally, and might subsequently fill it in in a publication like the "Journal of Physiology."

11284. Would it be sufficient to say that any proceedings must be taken within six months of publication, or giving information of that which was impugned?—Yes, I would accept that at once. All we want is that we may have the possibility of doing it.

11285. What I want to get rid of is the oppressiveness of these proceedings hanging over for two years?—I accept that at once.

11286. There is one matter I should like to have your view upon, because you are aware that statements have been made which have been investigated before us in the "Shambles of Science"?—I have read the book some time ago.

11287. I wanted to ask you about that experiment on a marmot?—I cannot tax my memory with that.

11288. Then I do not think I will go into it at all?—I am not connected with that book in any way.

11289. There is one other point in your Bill. In Sub-section (6) of Clause 2 you say, "The experiment shall not be performed for the purpose of attaining manual skill"?—Yes, that is the old Act; I retain that.

11290. A good deal has been said about it; do you see any objection if it is limited to animals which are wholly under anæsthetics, and must die under anæsthetics?—It is hardly for me to suggest to the Commissioners to enlarge the Act.

11291. But you know so much about it, that I wanted to know your view. Have you considered that point?—Yes, I have. Personally, I am afraid that if this provision was taken out, it would immediately be taken to be a permission to students at once to set about vivisection for the purpose of getting manual skill.

11292. By a licence?—Yes, getting a licence for this very thing.

11293. Students?—Yes. Certainly they would if this clause was eliminated; there is nothing to prevent their applying for a licence for that very purpose, to get manual skill.

11294. You do not suggest that taking these words out would give them permission?—No, I say that they would apply for licences, and would get them from the Home Office. I believe that they get them now.

11295. There is one other matter only that I want to put to you. Several witnesses have been asked, but I do not think that you have, and I put the question to you, does your Society take any action at all, or any interest, in what has been termed a farmyard operation—namely, castration, and so forth, of animals?—My Society does not. It is a Society simply for the purpose of combating vivisection.

11296. Has your Society, or have you as an individual, any views as to the thousands of operations in every county in England, which must, I think, every day cause more pain than all the operations in all the laboratories?—No, my Society has no such view, of course. It is purely an Anti-Vivisection Society. If I were to use its funds for the purpose of advocating a crusade against any other suggested form of cruelty, I should be committing a breach of trust.

11297. Your Society, then, not only concerns itself with vivisection alone, but solely concerns itself with vivisection which is scientific?—With vivisection under the Act.

11298. Of course, all these operations are vivisection in the crudest and cruellest form?—If so, they come under the purview of the Royal Society for the Prevention of Cruelty to Animals. That is our view. That is the proper society to deal with any such cases.

11299. At any rate, your Society does not concern itself with that matter?—It is outside our Society.

11300. I think you have been careful to abstain from going into any of what are called the ethical considerations?—No, the whole of my evidence has been in that direction. I stated that I threw myself open to cross-examination.

11301. (*Chairman.*) I invited Mr. Coleridge at the beginning to express his views shortly on that point, as we have had it fully before us, and he agreed to that. He stated shortly that he was opposed to vivisection on ethical grounds.

(*Witness.*) Yes, in my opening statement.

(*Mr. Ram.*) Perhaps I misread what the noble Chairman put to you; but, at any rate, I will not go into it with you, your evidence having been of another sort.

11302. (*Sir John McFadyen.*) I am afraid that after a remark that fell from you in answer to a previous question, you may rather resent the suggestion that you ever use any language that is ambiguous; but it seemed to me that you did in your evidence make use of two or three words that are capable of more than one interpretation. One of them is the word torture. I submit to you that the word torture is employed in two senses. In the first place, it is employed to denote very severe pain, and in the second place it is employed in describing operations and acts where the main object of the operation or act is to cause severe pain. I want to ask you in which of these senses you use the word torture. For instance, to-day you said that in your opinion animals are tortured in laboratories attached to large hospitals. Do you mean simply that in vivisection operations conducted in these laboratories severe pain is occasioned to animals?—That is what I mean.

11303. That is all?—That is all.

11304. You did not mean at all to imply that it was the main purpose of the operation to inflict pain?—I was not thinking of the purpose. I am looking at it entirely from the point of view of the animal.

11305. I am asking you now because it is a very important distinction; that is to say, whether you merely make the charge that severe pain is occasioned to animals?—That is all.

11306. You do not mean that it is a deliberate design on the part of the operator to cause severe pain?—No; it is incidental to the investigation.

11307. Still, you admit that the word "torture" is used in both senses?—It may be used in many senses. That is not my intention at all.

11308. Then you make use of the word "cruelty"?—Yes.

11309. I do not know whether you admit that that is in any degree ambiguous?—I will define cruelty in the way I use the word.

11310. Will you tell the Commission what your de-

finition is?—When I say “cruelty,” I mean the infliction of severe pain upon an animal, not for its own good. It is very difficult to define these words.

11311. It is not quite the definition that I expected?—That is what I mean.

11312. I thought you would probably say that all unnecessary pain comes under the head of cruelty?—No; that would imply that I judged what was necessary and what was not necessary for the purposes of experiment, of which I know nothing.

11313. But your definition does involve your judging the question, because you come to the conclusion that it is cruelty in all cases in which pain is inflicted, if it is not for the animal's good?—Yes.

11314. I am quite content with that definition. You will, I suppose, admit that the Act now in operation, the Act 39 and 40 Victoria, was not meant to prohibit painful experiments on animals, but merely to place certain restrictions on such experiments?—The Act as it left Parliament.

11315. That was the purpose of the Act?—Yes, it permits pain.

11316. It legalises pain?—It legalises pain; that is my objection to it.

11317. Your suggested amendment of this Act is intended to prevent the infliction of serious pain?—That is so.

11318. Or even serious discomfort?—Utter wretchedness.

11319. In connection with experiments on animals?—Yes.

11320. You wish to make it illegal to cause serious discomfort to the animal, even although, in the opinion of persons well qualified to judge, the experiment might extend man's knowledge of preventing or curing disease or alleviating suffering?—I am not prepared to accept or admit, not being an expert, the claims put forward by the vivisectionists as to those benefits.

11321. That I did not put to you. I asked you to assume that?—That there is benefit, do you mean?

11322. To assume that man's knowledge may be increased thereby?—That is a different question.

11323. Knowledge is power?—But the Act itself divides between the two, you remember; it says for benefit or for knowledge. There are two objects in the Act.

11324. Is it, or is it not, the purpose of your amendments to prevent the infliction of serious pain or even serious discomfort?—Serious pain.

11325. You admit that?—Yes.

11326. And you desire to have the infliction of serious pain or serious discomfort made illegal, even although, in the opinion of persons well qualified to judge, by carrying out experiments involving such pain, man's power of preventing or curing disease or alleviating suffering might be extended?—The evidence laid before the Commission has been almost unanimous in declaring that vivisection and experiments performed do not include pain.

11327. But I assure you that that has not been the invariable character of the evidence that we have had here, and I think it is within our own knowledge, as Commissioners, that some experiments involve pain at the outset. I have asked you to agree to the statement that the Act legalises pain?—Yes.

11328-9. Therefore, there is no reason why those who are licensees should not admit that they do cause pain. But I put it in another way. You do not admit that the fact that useful knowledge might be extended by inflicting pain on animals, with regard to the cure and preventing of disease, is any justification whatever for causing pain to animals?—No. I say that we have no right to inflict severe pain upon animals.

11330. I perfectly understand your position. My next point is this. You are, I suppose, quite well aware that no such restrictions as you propose to impose upon experiments performed on animals for the purpose of extending human knowledge are placed on man's dealings with animals for other purposes, although these involve severe pain and suffering?—I am not prepared to admit that.

11331. I shall have no trouble in getting you to admit that?—Very well.

11332. Will you take it from me that it is perfectly legal, and a common operation, to take a male animal, tie it to a table, or otherwise secure it for the safety of the operator, and while it is in complete consciousness cut out its testicles? As a layman, do you feel able to express an opinion as to whether that is a painful operation or not?—I have no doubt that it is painful.

11333. Have you any doubt as to its being an exceedingly common operation?—I have no doubt it is quite common.

11334. I suppose you are also aware that it is not regarded as an illegal operation?—I do not know that there has ever been a test case on that. I think it is cruelty to animals generally.

11335. Then I will put it to you in this way. Would you encourage the creation of a society, such as your own, having for its main object to bring about legislation to prevent the castration of male animals?—I do not say that.

11336. But you admit that it is cruel?—Yes.

11337. Would you, then, be in favour of an agitation which would compel the use of an anæsthetic pushed to complete anæsthesia on every farm animal that is castrated?—Yes, if it was practicable, certainly.

11338. Then, take your own words, what is the meaning of practically? Does it take into consideration the profit of farmers?—I am not thinking of that for a moment. I am thinking of it purely from the point of view of the animal and cruelty.

11339. Quite irrespective of the fact whether you would render farming operations, so far as relates to the breeding of male sheep, for instance, entirely unprofitable?—I do not accept that suggestion for a moment.

11340. It is not a suggestion; it is a question?—You suggest that it would make it unprofitable. I do not accept the suggestion.

11341. I ask you to assume that it would?—I will assume it, for the sake of argument. What next?

11342. You would still insist upon every male animal that had to be castrated being put under anæsthetics?—Certainly, but I do not accept the premise, you know.

11343. I did not ask you to accept the premise. But what I asked you to assent to was that at the present time such restriction as you seek to impose in your Bill are not imposed on men in other dealings with animals which involve great pain?—I may, in passing, say that so far as I know there is no Act of Parliament that makes it cruel, or would in any way support a prosecution for the circumcision of Jews, which is, I suppose, a painful operation, but it is done in very extreme youth, and it is quickly over, and it is done. I presume, for some purpose that the Jews support. It is somewhat similar to that, I think.

11344. Will you forgive me for suggesting that one might, at any rate, contend that it is done for the benefit of the individual Jew operated upon?—Yes. Whether it is so or not I am not prepared to say.

11345. You would not seriously put that forward with regard to castration of lambs and pigs?—The castration of lambs and pigs I presume is inflicted for their benefit; they would not be permitted to live unless it was done to them.

11346. Do you suggest the same with regard to horses?—Yes.

11347. Are you not aware that in some countries a very large proportion of the working horses are uncastrated?—If that be so, and there is no reason against it, I am in favour of their not being castrated here. But I have always understood that they would be dangerous in the streets.

11348. I think it is unquestionable that they are more dangerous, but not to the point of being unworkable?—Then we should have to do without them, I suppose.

11349. You would be in favour of a society which would agitate for the suppression of the castration of horses?—I did not say that. I never said it.

11350. But I think you must say so in order to be perfectly logical?—No, I say that it should be painlessly done.

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11352. I am speaking of the common rabbit trap. I only know the humane trap by name. I am well aware that the common trap is an exceedingly painful trap, which very often breaks the leg, and the animal may be in a state of torture for many hours?—I abominate that.

11353. It is not a case of your individual opinion?—I thought that was what you asked.

11354. I mention all that in order to lead up to this. It is not the individual feelings and actions of witnesses that have to be justified?—Are you asking me as representing my Society?

11355. No. You want this Commission, I take it, to approve of your Bill?—I do.

11356. Nothing would please you more than that we should recommend Parliament to pass it?—I should be still better pleased if you abolished vivisection altogether. I am afraid you will not do that.

11357. I am looking forward to the possibility of your Bill being recommended in Parliament, and I have foreseen that in Parliament these facts that I have just been bringing under your notice might be raised. I thought perhaps that you would help the Commission to find some clearly defined simple principle on which they could recommend Parliament to thus discriminate between painful experiments for the extension of useful knowledge, and the infliction of pain which is incidental to operations which are inspired by some other motives. So far we have not got that assistance from previous witnesses, except that some have said that no operation of a painful character on the lower animals is justified unless it is for the individual animal's good?—I would answer you in this way. I am not in Parliament unfortunately, but if I were in Parliament I should meet arguments of that nature, that you ought not to pass this Bill until some other cruelty was disposed of or dealt with, that you would never do anything in this world if you tolerated evil in one place until you stopped evil in another. If you must not stop a man beating his wife in Stepney until you have stopped another man jumping on his wife in Upper Tooting you would never do any good at all. You must take things in detail. You cannot put down all cruelty at once. The object of my Society is to deal with one branch of cruelty which we think very severe and which might be stopped with advantage to the community. It is no answer to us to say that there are other cruelties in the world. Of course there are. And in due course of time I should hope to see some of them mitigated.

11358. Would I be wrong in interpreting that answer as a confession that you find it impossible to state such a principle?—Not at all. The broad principle that I go upon is to put down all cruelty when I find it.

11359. I ask you if you could assist the Commission to find a principle on which they could ask Parliament to discriminate between painful experiments intended to extend man's knowledge and the infliction of pain in painful operations which are inspired by some other motives? I ask you if you can give us a simple principle?—No, I cannot put down a broad principle of that sort.

11360. Then I am afraid it is rather hopeless to ask Parliament to do so?—Not at all. I do not think persons in Parliament will be influenced by the argument that you are not to stop one cruelty until all cruelties are stopped.

11361. Would I be misjudging your reply if I took it to mean that as soon as you had disposed of this particular form of cruelty you would approve of going on to prevent the castration of animals except under the influence of anaesthetics, and that you would put down field sports, which, as you, I think, must admit, involve a great deal of infliction of pain?—Are you asking me now my own private views or the views of my Society?

11362. I will take both?—The views of my Society have nothing whatever to do with those other things.

11363. But, if you will pardon me, in a sense you have a great deal to do with them. You come here to advise us to take a particular course of action, and I think you will admit that we ought to be able to give a reason for advising Parliament to discriminate between pain which is inflicted in the course of operations intended to extend man's knowledge and pain inflicted in the course of operations inspired by any other motive whatever. Your Society ought, if it, I was going to say, presumes to advise us in the matter, to be able to help us in this case?—Our position is this, you see. These other cruelties that you justify have no Act of Parliament legalising them; they are open at the present moment so far to prosecution. It might not be successful, because public opinion may not have got up to the point at which such prosecution would be successful. But there is nothing on the Statute-book legalising these inflictions of pain that you are speaking of now.

11364. Would you be content to put what we call vivisection on the same footing exactly as these other operations?—No, because vivisection, in my opinion, under the present law certainly can be far greater cruelty than any of the cruelties you mention, because in the case of castration it is an operation that is done in a very few seconds, I presume; I do not suppose it takes a very long time, and some vivisections, at any rate, are very prolonged.

11365. I confess I am rather surprised that you should suggest that there are greater possibilities of cruelty in the one than in the other, because there are no legal restrictions whatever on the one?—Yes.

11366. You really, I take it, would not assert that the people who perform these operations on farm animals are on the average more humane than the licensed vivisectioners?—In the very nature of things all the farmer wants to do is to get rid of the animal's testicles as quickly as he can. It must be a very short operation. With regard to the man in the laboratory, his object may entail a very long operation; the thing must take twenty to thirty minutes even to prepare the animal and the various vivisection arrangements and appliances.

11367. Are you taking into account that most of the seriously painful experiments at present performed for the extension of knowledge have to be done under anaesthetics?—Not under the Act as it stands necessarily. I say that the present law admits of any amount of torture. That is my objection to it.

11368. What I want to know is whether you contend that under the existing law a great deal of exceedingly severe pain is caused to animals?—I say it can be.

11369. That was not my question?—I dare say it is.

11369A. Do you say that it is?—I am not there. I will neither admit nor deny what goes on in laboratories. I am not there. All I know of what goes on in laboratories is what vivisectioners choose of their own will to publish. That is all we know and can know of what goes on in laboratories.

11370. At any rate, you are not able to help the Commission by giving us any simple principle on which we could discriminate between painful experiments for the extension of useful knowledge and the infliction of pain which is incidental to operations which are inspired by some other motive?—I do not think any simple principle would discriminate.

11371. I quite agree with you. To pass to another point, the Act 39 and 40 Victoria legalises experiments for teaching purposes, and your amendment would make instruction in that way illegal?—Yes.

11372. In answer to a question put to you last week I think you grounded your objection to experiments for the purpose of teaching, on their being unedifying and bad?—Yes.

11373. Assuming that such experiments are painlessly conducted before medical students, will you tell us in what respect you think they are bad and unedifying?—You see you always put upon me an assumption that I do not grant.

11374. What is the assumption?—That there is no pain being inflicted upon the animals.

11375. I will allow the operation to be safeguarded by your own restriction that there is to be an inspector there the whole time. Will you tell the Commission then in what respect the operation would be un-

edifying and bad?—Because I think that vivisection of an animal not for its own good, merely for demonstrating some well-known principle of physiology, must be naturally approached by all the spectators in a perfectly different spirit from an operation upon a human being done for that human being's benefit. To me that is a perfectly self-evident proposition.

11376. But that should not be compared with operations on human beings. It ought to be compared with other legal painful operations on the lower animals. Is it less noble to use an animal in an absolutely painless fashion to demonstrate facts of physiology to a class than it is to kill an animal for food?—I thought the comparison was, certainly it was in my evidence, between a vivisection experiment in a laboratory and an operation in the theatre of a hospital.

11377. I never mentioned anything about that?—In my evidence that was the comparison put to me.

11378. You will forgive me if I am wrong, but so far as I could make out the only objection you have expressed to allowing painless vivisectional experiments to be performed for teaching purposes, was that they were unedifying?—Yes.

11379. I invite you to explain in what respect they are unedifying?—I am influenced very largely by the opinions of others upon the matter.

11380. By unedifying, do you mean that they are of no purpose, and are useless for the purpose of teaching?—I did not mean that. I meant by unedifying—

11381. Demoralising?—Yes, rather that. I did not use so strong a word. I used the word unedifying because I did not want to use such a strong word.

11382. I am sorry to repeat the question. Supposing the word to be used in its mildest legitimate sense, I want to know how it comes about that the operations are demoralising when the whole of the operations are painless and are known to be painless by the spectators?—I shelter myself under the opinion of others. The Inspector from Ireland gave that as his mature opinion after many years being Inspector. I do not know whether he used the word unedifying.

11383. I can assure you that he did not say they were unedifying. He expressed doubts whether they were of any real value, and he expressed a doubt as to whether they were always under complete anaesthetics. But I am going to have the experiment safeguarded by your Inspector. We were not able to put that to him.

11384. May I put it to you in this way. Do you see any reasons to modify your Bill by legalising operations for purposes of instruction when the Inspector is present and is assured that the animal is throughout the whole course of the proceedings completely anaesthetised. I may remind you that in answer to a question last time you said that you would place confidence in the opinion of the Inspector?—Yes. I am quite prepared to admit that at present I have no reason to suppose that any torture has ever been inflicted on an animal in the presence of the Inspector. That is why I am ready to trust the Inspector to see that it does not take place in future.

11385. Since the main purpose of your Bill is to prevent cruelty to animals (I mean you are not engaged in any religious crusade) why, if you were assured that these operations might be conducted without pain, should you expressly make them illegal in your Bill?—I may say that there is a very large feeling among the public whom I represent, that vivisection experiments, it may be only sentiment, are something peculiarly distasteful. For instance, there are many persons among them, and those whom I not the least respect, who would say that deliberately to place a dog under perfectly complete anaesthesia to throw it from the top of a high tower to see what happened to it when it hit the pavement was a brutal thing and ought not to be done even under anaesthetics.

11386. Such a thing I may say would never be done for teaching purposes?—You say that. I only say that there is a feeling among a large mass of people that they do not like an animal to be treated in that manner merely for the purpose of inquiry.

11387. But in this inquiry we are endeavouring to get at the moral or other justification for people's feelings one way and another; that is rather what I was

inquiring after?—I think it is a respectable feeling on the part of people who would protest against a dog being put under complete anaesthesia and thrown from the top of a tower to see how many bones were broken or in what condition its interior parts were.

11388. That would be an experiment?—Yes.

11389. That would not be conducted at demonstrations?—The students might be all invited to come and see the demonstration conducted. It is very unedifying to a spectator, in my opinion, although the animal is under complete anaesthesia.

11390. I perfectly agree with you; but might not that be safeguarded in wording your Bill?—It would be very difficult.

11391. Would not the Home Office be apprised of the nature of the experiment that was to be done for teaching purposes?—In my opinion many of the experiments detailed by Dr. Crile, even if they were under complete anaesthesia, are repulsive to every decent person who reads them.

11392. Were those done for teaching purposes?—They might be.

11393. But they were not?—They might be.

11394. That is not the point?—Certainly they might be.

11395. They might be. But even supposing they had been absolutely painless, you are taking objection to Crile's experiment because you are not satisfied that the animal was completely unconscious?—Yes, altogether. I assure you I represent, I am not speaking for myself now, a very large number of people who, if every single experiment in Crile's book were granted to be completely under anaesthesia, would still say that such experiments were brutalising—to pour boiling water, for instance, into a dog's inside to see what would happen. There are very many people who, rightly or wrongly, think it is not the way in which dogs ought to be treated by human beings. They are the friends of man, and it is not the way to treat them; and for anybody to witness such a thing must be a brutalising spectacle and cannot be edifying. That is my answer to you about edification. And you yourself agree with me, because you say that you agree about the tower. You say it is a repulsive thing to do.

11396. For instruction purposes. My attention has been called to the fact that Sir Thornley Stoker did express the opinion (in answer to Question 761) that certain demonstrations would be of a demoralising character to young men and women who witnessed their performance, and that they seemed to him to be an offence against humanity. So that to that extent you are supported?—I thought he said some such thing.

11396a. Some doubt has been expressed as to whether the infliction of pain on animals for what might be called trade purposes comes within the purview of the existing Act. Do you think it desirable that it should?—That is really a question for lawyers. You mean whether it should be in my Bill. It would not be desirable in my opinion.

11397. Yes, I understand that your intention was to leave such cases outside the Bill; but I wondered why you discriminate between operations that are identical in every respect except the motive?—I have taken the present Act of Parliament and endeavoured to amend it. I have not drafted a new Bill altogether to deal with cruelty to animals. I find the Act as it is, and I have not changed it in that respect.

11398. Will you forgive me for saying that though in a sense it is an amendment, in another sense it is an absolutely new Bill; because you admit that the present Act legalises pain?—Yes.

11399. You want to make pain illegal?—Yes, I do.

11400. And in leaving out painful experiments incidental to trade purposes you are discriminating?—No, they are not experiments.

11401. That is only begging the question. I submit that you call the thing when it is done for the extension of knowledge an experiment, and when it is done for trade purposes not an experiment; and if you like to put the question in that way, why do you discriminate between experiments which after all are identical experiments in every respect except motive?—The difference, I think, is fundamental. I am dealing in this Bill wholly with vivisection as practised in laboratories for the purpose of either learning, or instruction, or whatever it may be. These trade products, as they

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call them, whether they be cruel or not, differ in no way from any other ordinary cruelty to animals in the streets by mankind, and ought to be dealt with in that manner. They have ceased to be anything but trade products. If in the production of any material there is cruelty involved, that is a matter for the Royal Society for the Prevention of Cruelty to Animals; it has gone out of the purview of experiments. I am dealing solely with vivisection here. I am not going on a crusade against all experiments now; it is not my opportunity, and I did not come for that. I am not even on the committee of the Royal Society for the Prevention of Cruelty to Animals.

11402. So that it is a mere matter of convenience?—Not at all. If I was on the committee of the Royal Society for the Prevention of Cruelty to Animals, I should try and galvanise them into greater activity against many cruelties.

11403. You mean that you would try to prevent painful operations for such a purpose as the manufacture of serum?—I should if I could possibly do so.

11404. Can you assist us with a definition of what is an experiment. I noticed that you were inclined to classify the operations connected with the standardising of sera as outside the Act?—That is a question for the lawyers. It is outside the Act I presume.

11405. Why?—I do not know, I am not here as a lawyer.

11406. Do you think they ought to be outside the Act?—I do not know whether they are or not. I am not here to give a legal opinion.

11407. You have already given one?—I believe that some of the standardising experiments have been placed inside the Act. Sir Mackenzie Chalmers can tell you that.

11408. You are aware, I suppose, that a good many materials, such as milk, are tested by animal experiment as to the presence of bacteria and so on?—I was not aware of it; I will take your word.

11409. Have you never heard that medical authorities have been endeavouring to control that?—Yes, I have heard of it.

11410. Do you think that that is covered by the Act, that it comes within the Act?—If it is not painful I have no objection to it.

11411. But supposing it is as painful as other things which you say would be done for trade production and are outside the Act, what is your opinion?—If it is a painful experiment I object to it; of course, I am not here to give an expert legal opinion whether certain things come within the present Act or not—certainly in the presence of Sir Mackenzie Chalmers I would not venture to do so.

11412. I thought it was permissible to ascertain your intentions. It would fall to others, of course, to interpret your Bill if it became an Act?—Yes.

11413. Then in Clause 4 of your Bill, page 3, Sub-section (1), you say that no licence is to be granted to perform more than one experiment, or one series of not more than six consecutive experiments; and in the next paragraph, that the licence is to specify the time and place at which the experiment or series of experiments is or are to be performed. Can you tell me what is the object of that? It is not, is it, to make experimentation difficult?—Oh, no.

11414. What is the object?—The idea I had in framing that was to prevent a man getting leave to experiment from the beginning of the year to the end on as many animals as he likes, without any reference to the Home Office again.

11415. Surely you might find the happy mean somewhere between a licence for each experiment and a licence extending over a year. I put it to you that there are medical officers of health, and, I believe, veterinary officers also, who daily have to deal with infective matters, or matters suspected of being infective and dangerous, and in their case it would be quite impossible to carry out experiments under an Act framed like your Bill?—What you are describing sounds to me as if it was outside the Act altogether.

11416. No, you have just admitted a minute ago that the testing of milk, for instance, ought to be considered as inside the Act?—I do not give an opinion of that kind. I said nothing of the sort. I should not say so before Sir Mackenzie Chalmers. I will not give an opinion upon it at all.

11417. But nobody has ever suggested the contrary. It is within the Act?—I do not accept your statement that it is within the Act. I do not know what the case is. I want a specific case before I admit anything tangible.

11418. I will give you a simple case. Veterinary inspectors in the employ of the London County Council have to make periodical visits to cows within the metropolitan area, and when they come across a cow whose udder appears to them to present indications of its being tuberculous, but they are not absolutely sure, then they take a sample of the milk, which is taken to a bacteriological laboratory, and there and then injected into guinea pigs; and that is done pretty frequently. There are many similar operations where the thing is coming in daily, and has to be dealt with at the time it arrives. I suggest that the provisions of your Bill would make that sort of work practically impossible. Was that your intention?—I am not quite clear that it is legal, as it is under Section 3, Sub-section (1) of the Act. "The experiment must be performed with a view to the advancement by new discovery of physiological knowledge or of knowledge which will be useful for saving or prolonging life or alleviating suffering."

11419. (Sir Mackenzie Chalmers.) Sub-section (4) is the material one. All these, I think, come under Sub-section (4)?—Yes, but you never issue that certificate; you have not for years. That is just my complaint. I quite agree with you, that if these experiments are to be performed they ought to be under Certificate D, which has not been issued for years.

11420. (Sir John McFadyen.) After all, you are expressing legal opinion?—I am on that point most distinctly. I have quite a clear opinion upon it. I accept Sir Mackenzie Chalmers' view.

11421. It was that that I had in my mind when I asked you to define an experiment. I put it to you that when a sample of milk, taken in the circumstances I have mentioned, comes before a bacteriologist, he wants to extend his knowledge with regard to it, and the only means of extending his knowledge is his experiment which he performs upon the guinea pig?—You are putting that upon me. I do not admit it.

11422. Will you admit that his object is to extend his knowledge?—Yes, but not that that is the only way to extend his knowledge.

11423. That is not the material point, if you will forgive me for saying so. It is the extension of knowledge by new discovery?—Yes.

11424. Surely he must extend his knowledge by a new discovery with regard to this particular sample of milk?—Well, I will accept that.

11425. Lastly, in answer to Question 10,952, you said that all these experimenters have the greatest contempt for the Act of Parliament. You may read the rest of it if you like?—No, I do not in the least wish to do so. Yes, I see that is my one little slip, I remember.

11426. Will you forgive me for saying that it seemed to me a very hasty generalisation? Would you desire to qualify it?—Perhaps the whole answer was a slip; part of it was, I remember.

11427. I did not want to go into that?—It is rather sweeping, I admit.

11428. It is too sweeping?—It is too sweeping, I admit.

11429. Will you go so far as to say that not more than a small minority have a contempt for the Act?—No, I think a good many have a contempt for the Act. They have called it "an Act of which we are all ashamed," as I have quoted once already.

11430. You ask the Commission to believe that the majority of licensees hold the Act in contempt?—I think they do.

11431. Have you any evidence of that?—I read out one letter, at any rate, in which it was alluded to in public as "an Act of which we are all ashamed."

11432. You do not possess any evidence probably in that direction that you have withheld from the Commission?—No, except that I have heard it in various ways myself.

11433. (Chairman.) You said just now that you knew all that you know, and all that you can know, from what is published, and that you cannot see these things

yourself?—Yes, that is with regard to bringing prosecutions.

11434. But these experiments are attended by a great number of students, are they not?—Not the research experiments.

11435. But the other ones are?—Most of those under Certificate C, yes.

11436. (*Sir Mackenzie Chalmers.*) Under licence?—Yes, demonstrations under Certificate C.

11437. (*Chairman.*) And the research operations are witnessed by skilled men, I suppose?—You mean brother vivisectioners?

11438. Yes?—Yes.

11439. Does it occur to you that if anything took place at demonstrations which was cruel and negligent, I do not say that you would hear of every case, but amongst the students there would be some who would resent it?—No doubt.

11440. And who would make complaint?—No, I think very likely not. I should think that the general feeling in the profession would be much too strong for them. I do not think that a student would risk his future by making complaints against the demonstrator.

11441. You have heard no complaints, at all events?—Yes, I have, and indeed you have had them before you.

11442. From students?—Yes; you have had Miss Lind-af-Hageby.

11443. But she was a student simply for the purpose of fitting herself to inquire—to follow this matter up, as she told us. I was speaking of ordinary students who are being educated for the profession?—I should like on this to show you the kind of evidence that is constantly brought before me. I have had letters from students myself—of course, I do not give their names away.

11444. Have you made complaints upon them to the Home Office?—No, I have never done anything at all with them naturally; I cannot do anything. I should very much like to say on this question, that in this very last week I have received a letter signed by five or six undergraduates in an Oxford College, directly opposite the laboratory at Oxford, in which they complain of hearing the most dreadful howls of agony from dogs in that laboratory. Here it is, I now bring it forward to show you the kind of evidence that is brought before me constantly.

11445. These were not persons present in the laboratory?—No; but they lived opposite, and heard screams of what they say are the vivisected animals.

11446. If dogs are kept there, they may have been the screams of animals under operation; but really on this subject we have so many people who bring very strong feelings to bear that I should like something more if I am to be satisfied that these were under operations?—I am only showing the sort of evidence that comes before me. I did the obvious thing that a gentleman ought to do. I wrote at once to the head of the laboratory, told him the information that I had received, and asked for his explanation; and he gives the lie to the six gentlemen at once. All I can say is that in a Court of Justice the six witnesses would go against the one.

11447. (*Sir John McFadyean.*) They could only prove that the dogs howled?—Yes, that they heard the howls. But the answer is that there are no dogs there at all; there are only puppies.

11448. And you can always tell the difference between a howl of pain and any other howl?—Yes, that is what these young men say—that they know dogs well enough.

11449. (*Mr. Tomkinson.*) May I ask, would it be a very awkward thing for a student to give evidence against the demonstrator?—These young people are not medical students. They do not mind their names being given at all.

11450. I am speaking of students who have witnessed demonstrations, and have complained to you about them; would they not be marked men if they came forward?—Very much so.

11451. Might it not interfere with their profession hereafter?—I have not the slightest doubt of it.

11452. They would be looked upon as disloyal to

their profession?—Certainly. If the Commission like to have this letter, it is very interesting.

11453. (*Chairman.*) It does not touch upon our inquiry, apparently?—It touches upon it in this way: the extreme difficulty from our point of view of instituting any proceedings or taking any possible means of protecting animals.

(*After some deliberation.*)

11454. (*Chairman.*) A member of the Commission would like to hear the letter?—“Kemble College, Oxford, June 12th, 1907.—To the Honble. Stephen Coleridge, Sir,—I (an undergraduate member of Keble College, Oxford), knowing how keenly you feel about the practice of vivisection, wish to draw your attention to the following facts with reference to this practice. My rooms are exactly opposite the University Museum. Every day, almost without exception, I hear the piercing yells of a dog in agony. There can be no doubt that it is a dog in agony and not simply one in confinement. I have ascertained as a fact that vivisection is carried on in the museum from an official to whom I spoke on the subject when I went over on Sunday last in the morning (June 9th) after hearing these same yells. Being a keeper of dogs myself, I am in a position to know the difference between a dog in agony and one in confinement, and that it is not the latter I am perfectly confident. It is always the same, and I am convinced that it is something more than merely the beating of some dog, because that would not happen every day, whereas students' scientific classes constantly take place. I wish something could be done to put a stop to these devilish practices condoned in the name of science. To one who is a lover of animals it is perfectly excruciating to have to listen to a dumb animal being put to the torture, as I hear it every day, by these scientists who, being apparently devoid of all feelings of humanity, don't care what pain they inflict to gain their object. I should be so glad if you can suggest anything to be done. I should have thought at least they were bound to do these things in places where every sound could not be heard outside. Putting aside the fact of the inhuman cruelty which this practice necessitates, it is, to speak mildly, a public nuisance. Please use my name or letters as much as you wish.—I am, Sir, yours faithfully, HAROLD S. SHARPE.” And then: “We, the undersigned, desire to state that we have also heard these yells, and wish to corroborate all Mr. Sharpe has said.—ARTHUR H. M. PEAT, CYRIL A. BONSER, ALFRED C. C. HARNEY EVANS, GEOFFREY HEALD, BASIL W. TRUMAN.” I at once addressed a letter “To the licensees for the performance of experiments upon living animals, The University Museum, Oxford,” dated the 14th June: “Gentlemen,—I have received from a correspondent, whose credibility I have no reason to doubt, an account of cries of dogs or of a dog in agony that emanate from the buildings of the Museum at Oxford. A portion of the buildings in question being a place registered (under the Act 39 and 40 Vic., c. 77), for the performance of experiments upon living animals, and you, gentlemen, being under the same Act licensed to perform such experiments in that portion of the buildings, I beg leave to inquire whether these cries come from your laboratories, and, if so, whether you feel disposed to offer any explanation of these most distressing sounds.—Yours obedient servant, STEPHEN COLERIDGE.” Then I got this letter from Mr. Francis Gotch: “Physiological Laboratory, Oxford, June 16th, 1907.—Sir,—In answer to your letter of the 14th inst., I beg to state that no vivisection experiments on dogs have been carried out in this laboratory during the present year. Four puppies were born and kept in the adjoining yard, and the shrill yelps of these animals when at play were presumably the sounds heard by your correspondent. This might have been ascertained by making appropriate inquiry.—Yours faithfully, FRANCIS GOTCH.” I sent this letter with my compliments to these young gentlemen, and this is the answer that they sent back to me: “Dear Sir,—I am sorry I have been away and only just returned and found your letter. Professor Gotch is a liar.” These young gentlemen write like young men. “A man in Keble who is in for anatomy told Mr. Heald (one of those who signed my paper) that they did cut up rabbits, and that lately they had cut up a small dog, and this had been yelling for a week. It is perfectly absurd to say that the yells I hear are anything else than dogs, or possibly some other animals in agony.

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The idea of puppies is ridiculous. I have heard the puppies yelp very frequently, and I have also heard a dog scream, and I would swear to it anywhere. Mrs. Herford, writing to me to-day, says: "It is distressing to pass the Museum for the yells one frequently hears." Would two people be such fools as to mistake a painful yell for a puppy's yelp? It shows that Mr. Gotch is a liar, because one dog has been cut up this term. The man who told my friend (Heald) this said they did cut up rabbits. That is sufficient. Mr. Gotch says I might have ascertained the truth by making appropriate inquiries. I asked an official, as you know, and he said vivisection was practised. I would maintain, in spite of Mr. Gotch's statements, that animals have been cut up this term. If a man is devil enough to be a vivisector he can easily reconcile his conscience to a lie or two, especially when Mr. Coleridge writes to him, for he knows matters might be made hot for him." (This is very youthful.) "I am unable to get more facts than these: (1) One dog for certain has been vivisected this term; (2) also rabbits; (3) the cries I hear are *not* puppies."—Yours sincerely, HAROLD S. SHARPE." That is the sort of thing that is constantly coming before me, and that is the result that I get.

11455. It seems to me that the letter is about as reasonable as if Mr. Gotch were to answer it by saying: "No such thing has happened as you say, and as to your saying you heard it, all antivivisectors are liars, and I do not believe them." It does not carry it much further?—I do not think that is quite it, my Lord. We cannot carry it further. Here are six young gentlemen who write to me with no motive whatever but the motive of humanity. I admit that the language is the language of boys.

11456. (*Dr. Gaskell.*) What is the date that Mr. Gotch gave when he asserted that no animals had been experimented upon?—"No vivisection experiments on dogs have been carried out in this laboratory during the present year." The date is June 16th, 1907.

11457. "This term" meant distinctly during the time that Mr. Gotch was there and must have known?—It must have meant that. They give Mr. Gotch the lie.

11458. (*Chairman.*) It is possible that Mr. Gotch may be a witness here?—The difficulty is that under the present Act there is no Inspector, and nobody knows what happens. You get these opinions outside and inside, and who can judge?

11459. (*Sir John McFadyean.*) After all, the only direct evidence produced in that correspondence is that the cries that they heard are interpreted as cries of pain?—Yes; but these young fellows say that they know the difference between cries, and that they were the cries, at any rate, of dogs.

11460. (*Dr. Gaskell.*) Was there any definite date given when that small dog was cut up?—"Because one dog has been cut up this term"—it is "this term."

11461. (*Sir John McFadyean.*) That is not direct evidence?—No.

11462. (*Mr. Tomkinson.*) Does he not say that he called at the door, and an official admitted it?—"I asked an official, as you know, and he said vivisection was practised." That is his statement.

11463. (*Dr. Gaskell.*) He does not even say whether that dog that was cut up was alive or dead?—He does not. The context rather indicates that it was alive.

11464. You do get them for dissection dead?—Yes; but the suggestion in the context here is that the dog was alive.

11465. That is the suggestion, certainly?—Yes.

11466. (*Sir Mackenzie Chalmers.*) Did you send that second letter to Mr. Gotch, and did you get a further reply?—No, I left the matter there.

11467. There is a contradiction of fact?—Yes, a flat contradiction. It was no use carrying it any further.

11468. (*Dr. Gaskell.*) I do not quite understand your position with regard to Certificate B. I should like to have a little more explanation about it. I understand that you are desirous to abolish it in your Bill altogether, because you look upon it as entailing a greater amount of torture than anything else?—I think that more pain is inflicted under the present law under that certificate.

11469. Certificate B, of course, you know is for the purpose either of cutting out some organ or cutting

some nerves and keeping the animal alive afterwards; that is the kind of operation?—Yes, doing some serious operation and allowing the animal to recover; that may be one of the objects.

11470. That is the object?—No, not the object; it is one of the objects of Certificate B.

11471. I understood you to assert, in answer to Question 10319, that Certificate B dispenses with anaesthetics immediately after the initial operation is over?—Yes, the new wording does do that ever since 1899.

11472. That is your main point about the torture?—Yes.

11473. Do I rightly understand that it is the initial operation that you think torture may be inflicted in especially?—No, you have mistaken me. My point is that under Certificate B, as now worded, anaesthesia must be applied during the initial operation of opening the animal and fixing the electrodes to different nerves, or whatever it may be. Then I say that under Certificate B the animal can be kept in that condition on the board, and allowed to recover its consciousness, and the animal can be kept there disembowelled, or whatever it may be, as long as the operator chooses, without any anaesthesia to allay its sufferings.

11474. That is your interpretation of the word "initial"?—That is what the certificate means.

11475. Pardon me; I think not. That is your interpretation of the initial operation?—Yes.

11476. If such certificate were worded differently, you would have no objection to it. Supposing, instead of being worded as it is here, "Being during the whole of the initial operation of such experiment under the influence of some anaesthetic," and so on, it was worded, "During the whole of the operative procedures in connection with such experiment," would that meet your difficulty?—No; because my objection is that, after the operative procedures are over, during the period merely of observation of the animal, it may be lying in dreadful pain without anaesthesia.

11477. That is still part of the operative procedure?—No; looking at a board and watching the blood pressure is not an operative procedure.

11478. Stimulating the nerves is operative?—No, you need not touch the dog. To operate is to touch, certainly.

11479. Then we will have any form you like. There is no question whatever that that initial operation is included under Certificate B, and is intended to include the whole procedure while the animal is under anaesthesia?—You say that.

11480. That is so?—I will not admit it for a moment. Why were the words "During the initial operation of such experiment" put in? It used to run "During the whole of the experiment."

11481. The whole point of Certificate B is that the animal should have recovered from the initial operation; is not that so?—The initial operation, yes, of such experiment.

11482. And the initial operation means the cutting of the nerves or the removal of the organ, or what not?—Yes.

11483. And then after that till the animal is sewn up—the whole thing?—Not necessarily at all. It may be for hours on the board in that condition before it is sewn up.

11484. How can it be so?—There are such experiments.

11485. Would you mind showing me a single experiment of that sort under Certificate B?—I say that such can be done.

11486. Will you show me one that has been done under Certificate B? I have never seen or heard of such a case?—I say that the words of the certificate admit of it.

11487. Are you aware that in order to make the thing quite clear, in every Certificate B of late years (I know it in the case of Mr. Ritchie in 1902, and I know it in the case of Mr. Akers Douglas in 1904) there is always this inserted: "All operative procedures in connection with any experiments, except experiments under Certificate B, plus EE, to be carried out under an anaesthetic of sufficient power to prevent

the animal from feeling pain"?—I quite admit that during the operative procedure—that is, using the knife.

11488. No?—I think it is obvious.

11489. Surely everything up to the stitching up is an operative procedure?—No, my contention is—I may be right or wrong —

11490. I think you are absolutely wrong?—That is on the knees of the gods.

11491. No, it is a fact?—Here are the words of the certificate—this is the old certificate I am going to read you: "Such animals being during the whole of such experiment under the influence of some anæsthesia of sufficient power to prevent their feeling pain"; then the animal is allowed to recover under that.

11492. During the whole of what?—During the whole of such experiment; that was the wording of the certificate before October, 1899.

11493. Does that not confuse the term "experiment"? The experiment is not simply the operative experiment to begin with; it is the whole thing?—My point is that in 1899 the words were put in, "During the whole of the initial operation of such experiment." My contention—I may be right or wrong—is that the whole experiment may consist of first opening the animal and fixing electrodes to its cut nerves, or putting tubes down its arteries, and then it may be left on the board in that condition, fixed tight, so that it cannot move, and in that condition it may be allowed under this certificate to recover consciousness, and lie there bound at the will of the operator. I say that is part of the experiment, and it is not an operative procedure.

11494. But it is not Certificate B at all?—Yes, it is.

11494A. Certificate B is always for the purpose of keeping the animal alive afterwards; and for that purpose you would certainly not begin by putting tubes in a cut up animal in that sort of way?—I asked the very question of the Home Office, and I got a reply which confirmed my view of it.

11495. I should like to hear that reply?—I read it last time. Here it is. I asked, "Whether conditions are invariably added to this certificate" (that is Certificate B), "enforcing the employment of anæsthetics ensuring insensibility to pain from the commencement of the first operation until the conclusion of all manipulations of the animal, and until the last wound is closed aseptically." That is a very plain question, which obviously is directed to the very point on which you and I are at issue.

11496. Quite so. What is the answer?—The answer is: "Whenever the experiments would involve a series of cutting operations (which is very infrequent), conditions are added to the licence enforcing the employment of anæsthetics ensuring insensibility to pain during all operative proceedings." I maintain that that question and answer leave my contention untouched; that the animal may be opened and operated upon, and then left upon the board as long as no fresh cutting operation is performed (they absolutely say "cutting" here) without anæsthetics, under the conditions of that certificate.

11497. I should have thought that any commonsense person would understand that "all operative procedures" meant the operations right up to the time that the last stitch was put in?—That is the question that I asked.

11498. That answer implies it?—The question is: "From the commencement of the first operation until the conclusion of all manipulations of the animal and until the last wound is closed aseptically." I asked whether the anæsthesia really covered the whole of that period—the initial operation, the period of observation, and the final sewing up—and I did not get a reply which supports your view.

11499. Have you ever asked anyone who has operated under Certificate B?—I asked the Home Office the plain question what the certificate means. I give them the opportunity of giving me an answer, and they give me an answer which satisfies me that your contention is wrong.

11500. But that answer is distinctly in accordance with my contention?—I think not.

11501. Most emphatically?—I do not agree with you; "during all operative procedures"—that is all.

11502. The operative procedure I should have thought, certainly, so far as my knowledge goes, as

recognised by all persons in laboratories, does include the whole of that initial thing?—The animal may be opened, as I have said, and be prepared. What is there in the wording of this answer to prevent that animal being allowed to recover, and lie on the board for hours before they begin to sew it up?

11503. The words "operative procedure"?—There is no operative procedure. The animal is lying there, and nothing is done to it; it is not being touched; it is only being looked at. To look at an animal is not an operative procedure. I say the animal might be only looked at for an hour of two, or three hours, in that condition without any anæsthesia, under that certificate.

11504. You cannot give me an instance, I presume?—I am not a vivisector; I do not go into laboratories. I am only on the law.

11505. Have you examined the various Certificates B, which have been issued, to see the nature of the experiments?—They are not sent round to my office to be examined.

11506. Can you not get them?—No, I cannot. I have asked for them, and have met with a flat refusal. I have been given a courteous reply, but they will not submit them to me.

11507. You know that it is stated on the certificate that the man must describe the nature of the operation and his object in performing the experiment?—I dare say.

11508. Then it goes on to say, "All operations in connection with the experiment to be carried out under anæsthesia of sufficient power to prevent the animal feeling pain." My contention is undoubtedly that that does include the whole thing. You say that in your opinion it might be used, but you can give me no evidence that it has ever been used in the way you indicate?—I did not come here to give evidence of what I have seen in laboratories. I have never been in one.

11509. You suggest that?—I say so.

11510. To my mind it is a most extraordinary suggestion. I never heard of such a suggestion before?—I daresay not.

11511. It is impossible for me to conceive how it can come into your head?—Here is the law.

11512. No, the operative procedure takes in the whole of it. Then, apart from that, I presume that that is your main objection to Certificate B?—That is a very considerable one.

11513. Then that part of the objection would go if it was shown to your satisfaction that such an occurrence as you indicate has never taken place?—You could not possibly inform me that such an occurrence has not taken place. You are not in the laboratories any more than I am. It may be your opinion that such things do not take place, and it may be my opinion that they do; neither of us has a right to say that it has or has not occurred.

11514. Do you not understand that the very kind of experiment that you are putting down is an experiment that would be conducted under licence alone?—No, I do not say that for a moment.

11515. You do not understand it?—Yes, I do perfectly. I understand it too well.

11516. Then, I presume that you also consider that under Certificate B there is pain or considerable liability to pain afterwards?—What do you mean by afterwards?

11517. I mean to say after the whole thing?—What is your interpretation of "the whole thing"?

11518. After the operative procedure is finished with, and after the animal is kept in the laboratory?—When it is sewn up?

11519. Yes?—My answer to that is that I should think there may or may not be pain. In some cases there might not be pain, and in some cases there might be. I suffered considerably after I had an operation upon myself, I know.

11520. Are you aware that it is the custom now to add to that Certificate B these words: "The animals experimented upon under Certificate B, plus EE, must be treated with strict antiseptic precautions, and if these fail, and pain results, the animal must be killed under anæsthetics?"—I hear you say so.

11521. That is usually put on the certificate?—I accept your statement. I do not see these certificates.

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The Hon. S. Coleridge. Sir Mackenzie Chalmers ought to answer that question, not I.

3 July 1907. 11522. Is it not the fact that in consequence of asepsis, as used in hospitals, a very large number at all events of the operations are followed by painless results?—The suggestion was made, I think, by Mr. Starling that the surgical wards of hospitals are a kind of merry place for everybody, laughing and joking. I have asked several people, and I have not had that extreme view of the happiness of surgical wards confirmed. I do not deny what you say. I daresay that under asepsis very many operations may heal without very much pain. I imagine that others will be painful.

11523. In answer to Question 10,839, you said that by pain you meant real severe suffering?—Yes.

11524. Not small discomfort?—No, serious pain was the term I used, which I have agreed to add to the clause in my Bill.

11525. And you also said, in answer to Question 10,834, that you were willing to accept the Inspector's verdict as to whether an animal is in pain or not?—Yes.

11526. Of course, you know that one of the main duties of the Inspector when he comes to a laboratory is to examine all the animals which have been subjected to an operation under Certificate B?—I do not know what the instructions given to him are. The only definite instruction that I recollect having been given here in evidence is that he is not to consider himself a detective. I do not know of any other definite instruction which has been given to him, so far as I can gather from the evidence.

11527. We have had it from the Inspectors themselves that they invariably examine these animals?—If they say so, I have no reason to doubt it.

11528. Consequently they do see all the cases of animals which have been operated upon under Certificate B?—No, I am not prepared to admit that, for very good reasons. I have brought information before the Home Office that an Inspector had been to a registered place, and had not seen a very considerable number of horses which were in great suffering in another part of the building; and he was not taken to see them. I have that evidence with me, for what it is worth. I submitted it to the Home Office at an interview. Therefore I am not prepared to admit that the Inspector always sees every animal in the building.

11529. The Inspectors told us that they do go, at all events to all the places in the building where they can find animals?—They might have found these horses, but they did not. They go where they are taken by the vivisectors.

11530. At any rate we have had evidence from Professor Thane and Sir James Russell, that again and again—in fact, in all cases when they have examined these animals, they have found that they were not suffering pain?—But that evidence of mine is before the Commission.

11531. So that I do not see why you are willing to retain Certificate A, and are determined by your Bill to abolish Certificate B. Is there not much less suffering, from the evidence of these Inspectors, under Certificate B, than there is under Certificate A?—If that is the evidence of the Inspectors, I am not prepared to combat it; but what I have pointed out, and what I think I have made perfectly clear, is, that my objection to Certificate B mainly is as to the conditions that are inflicted upon animals when there is no Inspector there.

11532. After the initial operation?—Yes, that is my point; that is my chief objection to Certificate B.

11533. We may have more evidence upon the question, no doubt, but so far at present as the evidence goes I have no reason to suppose that such an operation as you suggest has ever been made under Certificate B. I wish you could produce some evidence that it has been, because it is new to me entirely?—You must remember, when you put up the evidence of the Inspectors against anything that I have said, that I have already asserted my opinion, and I think I have supported it by a large amount of evidence, that the Inspectors are not unbiassed.

11534. Still, you have asserted definitely here that you are willing to accept the Inspectors' verdict?—The verdict of future Inspectors. I would certainly not

have accepted the opinion of Dr. Poore on the question of the humanity of vivisectors.

11535. I thought you said just now that the future Inspectors were still to be appointed by the Home Secretary?—Yes, but there is that consideration about their humanity under my Bill.

11536. (*Chairman.*) No; it is the humanity of the operators that you get a certificate for, not the humanity of the Inspectors?—I think I have provided for the Inspectors too. It is in the proviso to Clause 7: "Provided always that in the appointment or assignment of such Inspectors by the Secretary of State, special regard shall be paid to the applicant's reputation for humanity."

11537. That is not a certificate, but "special regard"?—Yes.

11538. (*Dr. Gaskell.*) Would you consider that Certificate B might be retained if it applied only to animals which were operated upon under strict aseptic conditions. Do you consider that aseptic conditions before operation make a great difference with respect to the after question of pain to the animal?—That is a matter for experts. If the experts say so I am ready to accept, certainly, that asepsis does make a difference.

11539. What I am thinking of is this: I can understand your objection to experiments like those of Dr. Cecil Shaw, where septic material was put into the eye—that is, undoubtedly, I should think, a painful experiment; but in the case of a large majority of these experiments under Certificate B the Home Office directly insist on their being done aseptically for the purpose of preventing pain. Would you not allow Certificate B to exist if you were sure that that was always done?—My objection to Certificate B is that in its present form it permits something very much worse than that. You see what I am after.

11540. May I ask you, will you describe Certificate B in such terms as to avoid that initial operative difficulty of yours?—It is not in my province to be a draftsman, I have not done it, but I think it might be drafted in such a way as to preclude that possibility. I think it might be done with a little care.

11541. And if it was so drafted, would your contention still be that Certificate B ought to be abolished? If it was drafted so that the operative procedure, as I understand it, was really put in plain English so that you could understand it, would you still object to Certificate B?—That is the same question that Mr. Ram put to me, I think.

11542. I want to make it quite clear, because I want to understand why you make such a dead set against Certificate B when, so far as I can judge from what we have been told in evidence already, there is very little pain involved under it.

(*Sir John McFadyean.*) Mr. Coleridge has already said that he would put the animals under the same conditions as they are under Certificate A, because then he would be assured that they were anaesthetised throughout the whole of the operative procedure.

11543. (*Dr. Gaskell.*) What I put to you is this: What the vivisectors generally consider, you would say that you do not accept it, is that Certificate B implies exactly the same thing as occurs in every operation in a hospital?—I cannot agree to that.

11544. It is just like a surgical operation under anaesthesia, the animal recovering just as the patient does in a hospital?—I cannot admit that.

11545. (*Chairman.*) I think we all know what Mr. Coleridge's view is upon that point. (*To the Witness.*) As I understand, it is that you say that after that animal has been allowed to come out of the anaesthesia there is an interval, sometimes long, sometimes short, in which it suffers great pain?—That is my objection to Certificate B.

11546. (*Sir Mackenzie Chalmers.*) Whether that happens or not the certificate allows it, that is your point?—Yes, that is my point.

11547. (*Chairman.*) How far Certificate B does cover that is a question that we can settle for ourselves. It is either a question of the construction of a document or it is a question of fact as to whether the animals suffer pain?—Yes.

11548. (*Dr. Gaskell.*) In speaking about slight anaesthesia you stated in answer to Question 10387,

"You cannot be slightly without feeling. You either feel pain or you do not"—I have taken the word in its proper sense.

11549. Do you mean to say that you cannot be conscious and yet not feel pain?—"ἀναισθητός" means "un-consciousness."

11550. But can you be conscious without feeling pain?—I never have been myself.

11551. Is it not the case often in battle?—I am not a soldier.

11552. When worked up to a state of excitement, is it not the case that people do not feel the pain of wounds when they are cut?—I believe it is a well-known fact that when you are shot you do not feel the pain at once because of the shock to the nerves all round.

11553. Is it not the case in hypnotism that you can still be conscious and not feel pain?—I have not been hypnotised. I do not know about that.

11554. Do you also know that in lunatic asylums patients burn themselves and the sun burns them, but they do not feel pain?—I really do not know what lunatics feel; I have no experience of lunacy, I am glad to say.

11555. But you have told us distinctly that slight anaesthesia means that the animal feels pain?—I do not know that they have lunatic animals in laboratories. The parallel of lunacy does not seem to me to come in.

11556. I mean to say that in all these cases it is possible for consciousness to exist and yet pain to go?—That is your assertion. If you ask me to affirm it or to agree to it, I do not.

11557. You know, of course, I presume, that the corneal reflex is only one of many reflexes?—I am not a physiologist. I decline to discuss physiology with you or anybody.

11558. But you distinctly assert that unless the corneal reflex is abolished the animal feels pain?—I took that from the correspondence before me.

11559. No; all the correspondence before you said was that if the corneal reflex is abolished the animal is under deep anaesthesia.

11560. (Chairman.) I think everybody agrees that the corneal reflex is one of the last to disappear?—Yes, I agree to that.

11561. (Dr. Gaskell.) Will you not agree then that pain may disappear before the corneal reflex is abolished?—That is another thing.

11562. Just a question about Dixon and Brodie's case of light anaesthesia. I understood you to say, in answer to Question 10375, that the animal was under torture?—I say that I regard that experiment with profound distrust. I should like to have had the Inspector there.

11563. Do you not know that at these experiments a registering apparatus was being used?—I do not know in the least. I was not there.

11564. But do you not understand that if an animal is in pain it would show signs of that pain?—Not if it had curare. I do not know whether it had in this case.

11565. It was not a question of curare?—I do not know.

11566. And if the animal was in real pain do you think that the curves could have been registered by the apparatus in the way that they were?—There is a mention of curare (on page 144 of the "Journal of Physiology," Volume XXIX.): "In no instance have we obtained any effect by exciting the central end of the sciatic in a curarised animal" (I did not see that before), "but the experiments are not numerous." So that he did use curare.

11567. Not with the slight anaesthesia?—You do not know that.

11568. That is not the point. You were talking of slight anaesthesia?—Yes, and you asked me if the animal would not show signs of pain. I say, not if it was curarised.

11569. I want to know whether you do not consider when a delicate registering apparatus is going on in the animal, which moves perfectly smoothly and gets real good curves, even although the operator says the animal is under slight anaesthesia, that does not show that the animal was not feeling pain?—Not neces-

sarily. I have an answer to that, of Sir Victor Horsley, in the case against me on that very point that an animal can be held tight when it does not move. (Mr. Rufus Isaacs.) Would it be possible to perform the experiment or the demonstration which we know took place, if the dog had been conscious? (Sir Victor Horsley.) No, absolutely impossible unless the dog had been fixed up with all manner of apparatus, to absolutely fix every bone. You can fix an animal with apparatus of that sort. (Pointing to the operating board.) That is the answer to that—you can so fix an animal on that board—and he pointed to it—they had it in Court.

11570. (Mr. Tomkinson.) Would your Society, if it could obtain all that it wished, go for total abolition of vivisection?—We should willingly and delightedly accept total abolition if we thought that it was within the range of practical politics; but we do not think that it is within the range of practical politics in the present state of public feeling; therefore rather than leave animals to their present fate we wish to get what we can for them.

11571. You are aware that some other societies look coldly on that attitude?—All the other societies I believe are against my own Society for taking that view.

11572. Your main point I think is that the present inspection is extremely imperfect—that it is practically non-existent?—Yes, from the animal's point of view.

11573. And that when there are cases of supposed or stated cruelty or evasions of the Act, which are brought to the notice of your Society, or complaints are made from any other quarter, the whole of the evidence of refutation has to come from the parties who would be incriminated?—That is so; either incriminated as first principals or as aiding and abetting—witnessing without protest.

11574. And you think that permission of some of the public, at all events to pay surprise visits, would be very desirable?—Yes, certainly, if it could be obtained.

11575. The use of curare you wish to prohibit altogether?—Absolutely.

11576. And morphia you do not accept as a perfect anaesthetic by itself?—No.

11577. (Sir Mackenzie Chalmers.) Or urethane?—No, nothing but a respirable gas. Those things take some time, at any rate, to act. If you give a poisonous dose of morphia to an animal, it cannot be said to be anaesthetised, I presume, for a considerable time.

11578. (Mr. Gaskell.) Do you say that morphia does not take away pain?—I did not say so. I am ready to accept expert evidence that a poisonous dose of morphia is a complete anaesthetic near the period of death.

11579. That is on the question of consciousness. I am simply on the question of pain. Is not morphia the one drug that is used for removing pain?—I never heard of anybody having a leg cut off under morphia alone; therefore I do not accept it as a general anaesthetic. If it were so, the triumph with which the discovery of anaesthesia was hailed would not have been justified, if morphia was a complete anaesthetic; because it was discovered many years before the discovery of chloroform.

11580. I should have thought it would be admitted, according to the evidence that we have had, that large doses of morphia undoubtedly remove pain entirely?—I do not accept that.

11581. (Mr. Tomkinson.) A great many questions have been asked, both of you and other witnesses, with regard to other forms of cruelty to animals which we know do exist, and I think that your position is perfectly consistent as a non-sportsman. We must all agree that there are such cases of cruelty, especially in the trapping of animals, which to my mind is the most cruel of all, and also in regard even to the slaughter of animals for food. Animals which are troublesome or violent or very noisy, such as oxen or pigs, are supposed to be stunned before being killed, for instance?—No doubt there are cruelties in slaughter-houses which ought to be put down.

11582. But in the case of what are called farmyard operations, castration and the like, with regard to what Sir John MacFadyen put to you, do you know

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The Hon. S. Coleridge. of any case in which an animal is strapped to a board?—No, I do not.

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11584. Neither bulls nor horses could be allowed to continue entire, on account of the danger that it would be?—That is my belief.

11585. And with regard to pigs, are you aware that the operation is an instantaneous one, and apparently in small animals, such as lambs, so little painful that its after effects are not noticed at all by the animals?—So I should imagine. I have not seen them.

11586. Therefore it is not a question only of convenience, but a question of even allowing these animals to have their lives, certainly in the case of horses and bulls?—They would otherwise have to be destroyed, I suppose.

11587. Undue importance has been attached to that, in my opinion. Did you read a statement in an anti-vivisection pamphlet about the supposed baking of rabbits?—It did not issue from my Society.

11588. Have you read Sir Lauder Brunton's explanation of that before this Commission?—Yes, about its not being hotter than a Turkish bath.

11589. Was that explanation satisfactory to your society?—I have not gone into that.

11590. You know the statement was that so far from there being any cruelty or any baking or any excessive temperature, it was merely a warm bottle and cotton wool in order to prevent the temperature of the patient falling below a certain point?—I am not prepared to give an opinion upon that one way or the other. I have not studied it.

11591. (*Sir Mackenzie Chalmers.*) I think you said that your Society were not responsible for that pamphlet?—I do not think so. I do not think it is any publication of my Society.

11592. (*Mr. Tomkinson.*) Do you know anything of the offender against the law, I think you said his name was refused, who was deprived of his licence?—No, I do not know who he was to-day.

11593. But you said that he was reinstated afterwards?—Yes.

11594. Then as to starving animals, we have been told on a previous day that starvation does not entail pain. It is a rather novel idea, I think, when we remember all the stories we have heard of the experiences of people who have been starved after shipwreck and on other occasions?—Ugolino, for instance.

11595. (*Dr. Wilson.*) And starvation is awarded as a punishment?—I never thought of that. It is awarded as a punishment in prisons—or it was certainly at one time.

11596. (*Mr. Tomkinson.*) It is rather a startling statement to an ordinary person?—Yes.

11597. (*Dr. Wilson.*) Although you do not pretend to give an expert opinion, I take it for granted that your Society has from the outset amassed a large amount of literature on this subject of experimentation on living animals?—Yes.

11598. And your society also publishes a journal called "The Zoophilist"?—Yes.

11599. So that with regard to the usefulness of these experiments and their justifiableness, the editor of that journal is constantly scrutinising all the literature and reports which come to you?—Yes.

11600. May I ask whether the editor is coming to give evidence?—With a view of shortening the proceedings, I think I have represented pretty well all that my Society has to say. If the Commission desire to see him I have consulted with Dr. Berdoo, and he says he does not want to come unless you want him. I have also consulted my committee, and they say that if you would like to have him they will put him forward, but that he would more or less cover the same ground that I have done.

11601. (*Sir Mackenzie Chalmers.*) Is Dr. Hadwen connected with your Society?—No, we have nothing to do with him at all.

11602. You do not wish to answer for him?—Oh, dear, no.

11603. (*Dr. Wilson.*) You have already been asked

many questions concerning the various clauses in your Bill which you have drafted and which has been approved by your Society. I suppose you will admit that the element of pain so long as these experiments are allowed can never be eliminated?—I suppose not in inoculation experiments altogether.

11604. Nor in experiments under Certificate B?—Not with Certificate B altogether, certainly.

11605. It has been admitted before this Commission that in a good many of those inoculation experiments pain, of course, cannot be avoided?—That is so.

11606. And also in a few experiments under Certificate B pain cannot be avoided?—No.

11607. The question of credibility has been so often raised, supposing you were assured, as several witnesses have assured this Commission, that no matter how prolonged and severe these experiments may be that are carried out, and no matter under what anæsthetic or narcotic they are carried out, they are absolutely without pain, would you accept that statement?—Certainly not.

11608. Would you accept it as a statement of opinion then?—Yes.

11609. But you would doubt it as a statement of fact?—Yes, I do not think I could accept it. It is not the opinion probably of the animal.

11610. Supposing that experiments are still permitted to be carried on before classes, and that all experiments were required to be carried out under anæsthesia, would it give greater satisfaction to your Society if every experimenter were required, in forwarding his reports of experiments, to fill up a form of certificate in respect to them to the effect that to the best of his endeavour and belief the experiments had been carried out painlessly?—Oh, no.

11611. That would afford you no satisfaction?—Certainly not.

11612. Still, after all, no matter what amount of inspection you may have, you must put some reliance surely upon the *bonâ fides* of the experimenter?—Of the Inspector, certainly.

11613. No, of the experimenter?—I quite admit, as I have already said, I am perfectly ready to admit, that there may be many licensed vivisectors as humane as myself. The object of the Act of Parliament is to enforce that humanity upon others who are not so humane; and that is the object of my Bill, to enforce that humanity upon those who have not got it.

11614. You think there is always a great risk of their becoming so keen in research that they may become callous towards the infliction of pain?—Yes, certainly.

11615. You have raised a strong objection to the Association for the Advancement of Medicine by Research having been selected by the Home Office as their advisers?—Yes.

11616. Whether that society has turned out to be biased in favour of vivisection or not, can you not conceive that the Home Office or the Secretary of State at the time was influenced entirely by a desire to have the Act carried out more strictly and with the greatest possible care?—The evidence of Mr. Byrne, I think it was, is that the Home Office had no knowledge of the constitution of this society. It did not know who its president was, and, so far as I could make out, he knew very little about it.

11617. But when it was originally started I think it embraced many eminent men, such as Lord Lister and the late Sir William Jenner, whose names have been mentioned in connection with it. However, that is immaterial. Would you be satisfied if instead of that society, a statutory body such as the General Medical Council were made responsible?—I have already answered that question, by saying that I should not object to a body which had no licensed vivisectors upon it. I think that they should not report upon themselves.

11618. The General Medical Council consists, of course, of some of the most eminent men and teachers in the profession, and that body is entrusted with the registration of all qualified medical men, with the standard of education of medical students, and also with the power of removing from the register any medical man who misconducts himself in any way.

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Would you be satisfied with a body such as that?—If there were no licensed vivisectors upon it, yes.

11619. That body is responsible for the education of the medical profession, so that the onus then would be thrown upon the profession instead of upon the Home Office for granting licences?—My desire is to separate licensed vivisectors from the medical profession. I do not include them in any way in my mind with the medical profession. In my mind the medical profession, as a rule, are not vivisectors.

11620. But, after all, is not the consensus of leading medical opinion of the day in favour of experimentation on animals?—I expect it is—the expressed opinion. What the unexpressed opinion may be, I do not know.

11621. Then it is a question between the public really and the profession, is it not?—No, I do not say that for a moment. I said the expressed opinion of the profession. There are numbers of medical men who agree with me who do not like to say so out of *esprit de corps*.

11622. I need not press that point?—But my Bill does not abolish vivisection.

11623. No, you only wish to protect animals as much as possible?—Yes.

11624. (Chairman.) Is there anything further you wish to say?—There is one point I should like to mention. I want to read a letter from the Duke of Portland, who is chairman of the Dogs' Home at Battersea, with regard to a suggestion which was put to Mr. Starling by, I think, Sir Mackenzie Chalmers, about using the Dogs' Home as a sort of source of supply of dogs for vivisection.

11625. (Sir Mackenzie Chalmers.) It was suggested by one of the witnesses?—Yes, I think it was Professor Starling. You asked the question, and he thought it would be a good thing. I accordingly wrote to the Duke of Portland as Chairman of the Home, to know whether he would approve of such a use being made of his Dogs' Home.

11626. He objects on behalf of the Home, of course?—Yes, he says: "Dear Mr. Stephen Coleridge,—Many thanks for your letter. I think Mr. Starling's suggestion is not only horrible but absurd, and it is one which, both as President of the Lost Dogs' Home and as a private individual, I should feel bound to resist

as strongly as I possibly could. I say the suggestion is absurd because I am convinced it would never be accepted by the majority of the subscribers to the Home, and I have no doubt if persisted in it would mean the closing of the Institution and the loss to the public of much useful work at present undertaken and carried out by voluntary subscription. I am sending your letter and my reply to the Secretary of the Lost Dogs' Home, and, personally, I shall be glad to know that you have opposed Mr. Starling's suggestion, and I am, yours sincerely, (Signed) PORTLAND."

11627. (Chairman.) Is that all you wish to say?—Before I go, my Lord, I should like to say this one word in parting. I have to thank the Commissioners very cordially for their kindness in listening to me so long, and I wish just to say this one thing. As regards the courtesy with which Sir Mackenzie Chalmers has treated me, I shall always feel very grateful, and personally nothing but pleasure, in having been able to bring accusations against the Home Office face to face with the Chief of the Department. But having said that, I wish to emphasise, if I may, that I cannot help feeling rather forcibly that after I have left this room Sir Mackenzie Chalmers remains in the position of judge upon the indictment which I have felt it my duty to bring forward; and I therefore feel that the situation is a peculiar one—it is not one of my making at all; but I do think it ought to be remembered as a fact that after I have withdrawn Sir Mackenzie Chalmers remains as the judge upon the very points which I have brought as an accusation against the Home Office of which he is the head.

11628. Whom he has to advise?—Well, to advise. But I feel sure that in the circumstances, having had the great pleasure of being cross-examined by Sir Mackenzie, he will meet that difficulty in a spirit to which nobody could have, and certainly I have, no objection.

11629. I think I should say that yours is not an extraordinary position. Upon every Commission relating to an Act of Parliament there is some member representing the administering department. It is quite usual. It is only that this happens to be a case in which feelings are moved more than they are, perhaps, in regard to motor-cars or London traffic?—It puts me somewhat at a disadvantage.

TWENTY-SIXTH DAY.

Wednesday, 10th July 1907.

PRESENT :

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.

Sir W. S. CHURCH, Bart., K.C.B., M.D.

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.

Sir J. MACFADYEAN, M.B.

Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.

Mr. W. H. GASKELL, M.D., F.R.S.

Mr. J. TOMKINSON, M.P.

Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. C. J. MARTIN, M.B., D.Sc., F.R.S., called in; and Examined.

10 July 1907. *Mr. C. J. Martin*, M.B., D.Sc., F.R.S.,
11630. (*Chairman*.) You are, I believe, Director of the Lister Institute of Preventive Medicine?—Yes.
11631. Chairman of the Anti-Typhoid Inoculation Committee, appointed by the Secretary of State for War?—Yes.

11632. A member of the Advisory Committee for the Investigation of Plague in India, appointed by the Secretary of State for India?—Yes.

11633. A member of the Tropical Diseases Committee of the Royal Society?—Yes.

11634. And you were formerly Professor of Physiology in the University of Melbourne?—Yes.

11635. At present your principal post is that of Director of the Lister Institute of Preventive Medicine?—That is the only post I hold.

11636. I think you appear before us at the request of Professor Starling's Committee of Delegates of Medical and Scientific Societies?—I was asked by that committee to attend in order that the evidence should not be repeated over and over again, their object being to arrange which persons would be most suitable to present particular pieces of evidence.

11637. I am afraid it is impossible at this stage of our inquiry to hope that everything you tell us is something that we have not heard before, but no doubt there is a great deal that we shall learn from it. You propose to give us evidence under several different heads, and the first head, I see, you propose to speak about is the relation of experiments on animals to the knowledge of the causation of infectious diseases?—Our knowledge of the causation and control of them.

11638. Would you give us your views upon that point, and we will interrupt you if we think it necessary at any point?—Our knowledge of the causation of infectious diseases has been gained by experiments on animals. By the same method has been discovered the means whereby the causal agents exert their injurious influence on the body, and the means whereby the animal body defends itself against attack. I am unable to conceive any other method by which this knowledge could have been gained. It is only by the employment of the experimental method that medicine, like other sciences has been enabled to emerge from quackery.

11639. Is not that rather a strong phrase?—I am using "the experimental method" as a philosophic term.

11640. I meant the word, "quackery" which rather

infers that all knowledge not discovered by the experimental method is quackery?—By "quackery" I mean practice not based upon knowledge.

11641. All knowledge derived by clinical observation you think is quackery?—But the experimental method includes that.

11642. If you include clinical observation in the words "experimental method," I understand what you mean?—That emerges from the next sentence.

11643. When you speak of the experimental method, you do not mean the experimental method on animals?—Not exclusively. Since the function of medicine is the treatment of disease—i.e., of derangement of functions in living organisms, the only possible way in which the experimental method can be applied to the study of medicine is by observation and experiment upon living beings. That, of course, includes clinical and pathological observations upon man. The fundamental conception of the nature of infectious diseases we owe to Pasteur, who established the truth of his conception by experiments upon animals. Pasteur first showed, in a series of epoch-making researches into the nature and causation of fermentations, that each fermentation (e.g., alcoholic lactic acid, acetic, etc.) was due to the observation of a specific micro-organism. If the germs of one particular organism were sown in a nutrient fluid the products of one particular fermentation resulted. His experience on this subject led him to the great generalisation that infectious diseases might themselves be interpreted as particular fermentations, and as due to specific micro-organisms. By a series of masterly experiments upon animals he established the truth of his hypothesis in the case of anthrax, chicken cholera, and swine erysipelas. These results of Pasteur's may be regarded as the foundation of the whole modern study of contagious diseases both in man and in animals; and their extension by Pasteur and his pupils, and by bacteriologists and pathologists all over the civilised world, has led to the discovery of the causation of most of the infectious diseases to which man is liable. It can hardly be disputed that if we are to acquire control over any disease we must have knowledge of the agent which is responsible for its production, and that if we are to frame measures to hinder the ravages of any disease, we must study (1) the life history of the micro-organism which is the exciting cause of the disease; (2) the conditions under which it can live and flourish; (3) the conditions which are detrimental to its existence; and (4)

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the circumstances by which we may modify its virulence. An accurate knowledge of the life history of the micro-organism in question is indispensable, if we are to trace in detail how the disease is transmitted from one individual to another, whether directly from individual to individual, or indirectly by other living organisms, or by dead material. Only by the possession of such knowledge can we hope to devise the best means of hindering the extension of epidemics. In a certain number of diseases this knowledge is already at our command, and has afforded means of stamping out these diseases from places where till recently their ravages were responsible for many deaths annually—e.g., malaria, yellow fever. Moreover, knowledge of the nature of the exciting causes, their conditions of existence, and the reaction of the animal organism to their introduction, has already led in some cases to the finding of a curative agent, and has indicated a way by which we may hope to gain curative powers over other infectious disorders. Even in certain cases where the causation of the disease was not directly established by experiments on animals, because no susceptible animal was available—e.g., in typhoid fever—the discovery of the causation of the disease in man was indirectly due to such experiments, since the experience gained by Pasteur, Koch, and others on animals with regard to the characters of specific infectious disorders, has enabled us to correlate our observations on man, so as to establish the casual nexus between the typhoid bacillus and the disease. Indeed, as a result of animal experimentation a new science can be said to have grown up, the science of infection. It is by experiment that Robert Koch and his pupils alone have during the past few years succeeded in establishing the causation of the following common human ailments—namely, tuberculosis, cholera, typhoid fever, diphtheria, erysipelas, tetanus, and pneumonia. In support of the above general statement, I will give a few instances from the history of scientific discovery during the last half-century.

11644. You have referred to the success in establishing the causation of certain common human ailments, and the first you name is tuberculosis. Will you tell us now what you have to say about that?—Amongst the inhabitants of temperate climates tuberculosis is responsible for more deaths than any other cause. Although the mortality from tuberculosis has declined greatly of late years, during 1903 58,150 persons, or 11.3 per cent. of the whole mortality, died from this cause in England and Wales. Tuberculosis is also a common disease of food animals. Up to the middle of the nineteenth century phthisis and other manifestations of tuberculosis were not recognised as connected, and although some clinicians, on the basis of their experience, suggested that phthisis was a contagious disease, this was not generally recognised. The prevailing view was that phthisis and other forms of tuberculosis were the results of constitutional peculiarities capable of hereditary transmission. It was realised that by keeping the individual in the most favourable surroundings for the maintenance of robust health, the onset of such manifestations was in many cases delayed, the explanation of such delay being that the patient had “outgrown the tendency to the disease.” It was not known that the essential cause of tuberculosis was introduced from without, and, consequently, it was not recognised as a preventable disease. In 1843 Klencke* by experiments on animals, showed that tuberculosis could be conveyed from one animal to another by inoculation, and in 1865 Villemin†, by carefully-conceived and conducted experiments on animals, proved that tuberculosis was an infective disease due to a specific virus of a parasitic nature, which was introduced from without. He showed that the virus was capable of multiplying indefinitely in the bodies of animals, and of being handed on from one animal to another by inoculation. He also demonstrated by experiments on animals the casual unity of the numerous manifestations of tuberculosis, such as phthisis, caseous pneumonia, scrofula, lupus, and joint disease, etc., and also the important fact that “grapes,” a common disease of cattle, was due to the same virus.

11645. What is caseous pneumonia?—Cheesy.

11646. Why is it so called?—Because it was a pneumonia with a formation of patches of degenerated tissue

of a cheesy nature. At the time of Villemin's discovery the prevailing view of the medical profession was that these diseases were the products of deranged functions. Much controversy occurred for several years, until the confirmatory experiments of Cohnheim and Salomonsen (*Sitzungsberichte des Schles. Gesellsch. für vaterland Kultur*, 13 July, 1877) secured the adhesion of medical opinion. In 1881 Koch, following the lines laid down by Pasteur, who, by experiments on animals, had established the cause of splenic fever in stock, discovered, isolated and cultivated a bacillus in the tissues of men and animals which had succumbed to tuberculosis. By a series of experiments upon animals Koch demonstrated this bacillus to be the virus of tuberculosis. Having ascertained the casual agent in tuberculosis, Koch (Berlin Klin. Wochsch., 1882, p. 221. *Mitth. a.d. Kais. Gesundheitsamte*, 1884, Bd. 2, p. 22) was enabled to determine the conditions under which it could live outside the body, and to discover the paths by means of which the infective agent leaves a diseased animal and obtains entrance into a healthy one. Further, this discovery placed the casual unity of the very various manifestations of tuberculosis disease upon a sound basis, and permitted a certain diagnosis to be made in early and obscure cases.

11647. You then proceed to puerperal fever.—Puerperal fever is a disease which attacks women after parturition. After a mother has borne all the troubles of pregnancy, has passed through the pains of child birth, and is filled with happiness at the birth of an infant, she is liable to be stricken down with this terrible disease, and perhaps destroyed by it within a few days. In the years 1760-1770 puerperal fever prevailed in London and Edinburgh to such an extent that in some of the lying-in institutions nearly all the patients died; and in Paris, as late as 1866, over 25 per cent. of the women confined at the Maternité died of this disease; so that lying-in hospitals were looked upon by women of the lower classes as the vestibule of death. In England, during the years 1847-1903, 93,243 mothers died of this disease. From the time of Hippocrates down to the middle of the 19th century, with the exception of a few physicians who were more capable than their brethren of analysing their experience and drawing accurate conclusions (notably Dr Alexander Gordon, of Aberdeen, in 1795), the general view as to the causation of puerperal fever was that it was due to the suppression of the serous discharge which invariably escapes from the raw surface of the uterine wall during the few days following parturition. I mention this to show how the human intellect is prone to confuse effect with cause, when the true nature of the latter is not established. About the middle of the 19th century Semelweis, in Vienna, and Oliver Wendell Holmes, in America, drew the attention of the profession in these places to the fact that an analysis of the experience of puerperal fever in lying-in hospitals and in midwifery practice indicated in the strongest manner that this disease was of an infectious nature, and conveyed from one woman to another by the agency of the doctor or midwife. The opinions of both of these men were not, however, generally accepted. One of the principal of opponents of Holmes' view was the Professor of Midwifery at Philadelphia in 1852. In his criticism of Holmes' paper he says: “I prefer to attribute them (the cases of puerperal fever) to accident, or Providence, of which I can form a conception, rather than to a contagion of which I cannot form any clear idea.” Holmes' critics were able men, and I quote the above to show the necessity of supplying direct concrete proof of the nature of a contagion in order to satisfy the minds of most men.

11648. Holmes' opinion was not formed from experimenting, I understand?—It was formed from observation of experiments that were unwittingly performed ready for him by the doctors and nurses in lying-in hospitals. In 1878 we find the Academy of Medicine of Paris formally discussing the causation of puerperal fever. It was at this meeting that Pasteur, after listening to a variety of views as to the causation of this malady, interjected “None of those things cause the epidemic; it is the nursing and medical staff who carry the microbe from the infected woman to a

* *Untersuchungen und Erfahrungen im Gebet der Anatomie, Physiologie und Mikroskopie. Wissenschaftlichen Medecin. Leipzig, 1843. Bd. 1.*

† *Cause et Nature de la tuberculose. Bull. de l'Acad. de Médecine, 5 Dec., 1865, & 30 Oct., 1866. Etu dessus la tuberculose, preuves rationnelles et expérimentelles de la spécificité et de son inoculabilité. Paris, 1868*

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healthy one." The orator having remarked that he feared that microbe would never be found, Pasteur went to the blackboard and drew an organism growing in chains (the strepto-coccus) which he had isolated from the discharges of a number of patients suffering from puerperal fever, and from tissues of those dying of this disease. He also found the same organism in the blood obtained by a prick of the finger in similar cases, shortly before death. Pasteur believed, from analogy with his observations on anthrax and other diseases, that this organism would be found to be the cause of the disease. This was subsequently proved by experiments upon animals. Pure cultures of the organism discovered by Pasteur injected into rabbits multiplied rapidly, and spread throughout their blood, causing the death of the animal with fever and suppuration; a drop of the blood of a rabbit dying from this inoculation, if rubbed into a scratch upon another animal, occasioned its death in a similar manner.

11649. Do I correctly understand that this coccus poisons persons, and will kill persons who are not child-bearing?—Certainly. The conditions following child birth offer a very large surface for the entry of any organism; but the disease is common to all forms of wounds.

11650. You mean that the same coccus causes those diseases and others?—Yes.

11651. It is not peculiar to puerperal fever?—Not at all. The nature of the contagion in puerperal fever having been recognised, the disease almost entirely vanished from the maternity hospitals of Europe and America. The result was accomplished by avoiding every opportunity whereby the infection might be conveyed to a woman during labour, and by employing those principles of antiseptic surgery which we owe to Lister. The antiseptic system of Lister was a direct extension to wound infection of Pasteur's work on the nature of fermentation and putrefaction.

11652. Then we come to diphtheria. Will you tell the Commission what you have to say about that disease?—Diphtheria, a name introduced by Brettoneau for a series of inflammatory diseases of the throat, windpipe, and nose, associated with the development of a membranous exudation, has long been known to be an infectious disease. It is a disease more particularly of childhood, the course of which the physician found himself until recent times powerless to influence. He could only do what little was possible to diminish the sufferings of the patient, and when the larynx became obstructed, establish an entrance and exit for the respiratory air by tracheotomy, or intubation. In 1875 Klebs (*Arch. f. exper. Path. Bd. iv.*) found a bacillus hitherto unknown, in the membranous exudation of cases of diphtheria. This bacillus did not occur alone, but associated with a variety of other micro-organisms, and it was doubtful how far this particular bacillus was responsible for the disease. In 1882 Loeffler (*Mittheilung a.d. Kais. Gesundheits amte. Bd. 2, 1884*) succeeded in isolating Kleb's organisms from the diphtheritic membrane and growing it upon solidified blood serum. Once isolated, the Klebs-Loeffler bacillus was shown by experiments upon animals (rabbits, guinea pigs, and pigeons) to produce a disease presenting a resemblance to diphtheria, including the characteristic paralysis when the animal survived the acute symptoms (Loeffler, Roux, and Yersin) (*Ann. l'Inst. Pasteur, 1888*). As had been found to be the case in man, the bacillus occurred only at the point of inoculation. This fact led Roux and Yersin (*Ann. de l'Inst. Pasteur, 1888, 1889, and 1890*) to the conclusion that this bacillus must produce a poison which was absorbed into the circulation. They therefore grew the bacillus in broth, and discovered that after the separation of the germs by filtration through porcelain, the fluid contained a powerful poison, which, when inoculated into animals, produced all the effects of the bacilli themselves. Thus was diphtheria toxin discovered; but, what was far more important, a method whereby a pathogenic organism exerts its deleterious effects was brought to light. Roux and Yersin's experiments also led to the discovery by Behring and Kitasato (*Deutsche Med. Wochschr, 1890. Behring and Wernicke Feilschrift f. Hygiene, 1892*), through experiments on animals, of a physiological process which is brought into play by the animal body to counteract the influence of the germs of some diseases. This process consists of the production, through the agency of its own tissues, of a substance which neutralises the poison

produced by the microbes. When this substance is manufactured by the cells of the animal in sufficient quantity and with sufficient rapidity, the animal is able to successfully resist the onslaught of the diphtheria bacillus, and recovers from the disease. These observations, in their turn, have led to the development of curative means in imitation of nature's processes (serum-therapy), of great value to humanity.

11653. We now come to tetanus?—Tetanus or lock-jaw is a malady common to man and most animals. Of the latter the horses are particularly susceptible to this disease. The sufferings caused by the intense muscular spasms, which are characteristic of the disease, renders tetanus one of the most horrible maladies one can witness. The mortality from tetanus is 80 per cent. In fatal cases the spasms involve the respiratory muscles to such an extent that the patient dies of suffocation. Since the introduction of the antiseptic treatment of wounds the incidence of this disease has been greatly reduced, but formerly it was much more frequent. It has long been known that tetanus was particularly prone to follow badly lacerated wounds, especially such as became contaminated with soil or road dirt. The danger of tetanus after gunshot or other wounds in battles was formerly very considerable, and when once the infection started in an extemporised military hospital it spread so extensively as sometimes to infect everyone of the wounded. According to the German statistics in the Franco-German war, tetanus occurred in 0.35 per cent. of all wounds (*quoted by Vaillard article Tetanus maladies communes à l'homme et aux animaux, Paris, 1906, p. 351*). Tetanus has also been responsible for a heavy infantile mortality, especially in maternity hospitals. In the Rotunda Hospital, at Dublin, up to 1882, of 17,650 infants born, 2,944 died of this disease (*quoted from Clarke by Vaillard, loc. cit. p. 348*). This mortality could have been entirely prevented if the cause and means of spread of the infection had been known. That tetanus was a disease due to infection by a specific micro-organism was conjectured from the time that the microbial causation of anthrax and chicken cholera had been established by Pasteur. The infective nature of the disease was established by Carle and Rattone in 1884 (*Studi sperimentali sull' etiologia del tetanos. Giorn. della R. Acad. di Medicina di Torino, 1884*), who succeeded in producing tetanus in animals by injecting a small quantity of the pus from the wound of an animal suffering from this disease. The discovery of the causal agent was not, however, determined until 1889, when Kitasato (*Deutsch Med. Wochschr, 1889, No. 31*) succeeded in isolating and cultivating the same bacillus from the pus of wounds in cases of tetanus, and proved, by the injection of pure cultures of the organisms into animals, that it, and it alone, occasioned the disease. This bacillus had been previously seen by Nicolaier in 1884 (*Deutsch Med. Wochschr, 1884, No. 52*), in the local wound of animals which he had succeeded in infecting with tetanus, by the inoculation of a minute fragment of garden earth, and also by Rosenbach (*Arch. f. Klin. Chir, 1885*), in the pus from a wound in the case of a man suffering from tetanus. These discoveries explained how the infection of tetanus could be conveyed from one wound to another, and why soiling of wounded surfaces with earth is particularly liable to occasion the disease. Having isolated the bacillus of tetanus, Kitasato was enabled to precisely ascertain, firstly, where and under what conditions it flourished in nature; and, secondly, those conditions which destroyed it. The bacillus of tetanus remains in the situation at which it finds entrance into the tissues. Under suitable conditions it multiplies in the wound, and produces its characteristic effect upon the nervous system by means of a poison which it secretes. By growing Kitasato's bacillus in broth and filtering through porcelain, which does not admit the organisms to pass, but only the soluble products of their activity, Faber, in 1890 (*Bertin Klin Wochschr, 1890, p. 717*), obtained a poison, minute quantities of which, when injected into animals, produced the disease (1-100,000 of a cubic centimetre). All these investigations have afforded knowledge of the cause of tetanus, the life history of the causative bacillus, the conditions favourable or otherwise to its existence, the means whereby it may be destroyed, and the manner in which the symptoms of the disease are brought about. By taking advantage of this know-

ledge, epidemic outbreaks of the disease can be absolutely prevented, and the great majority of wounds where contamination with soil has rendered them liable to infection with tetanus bacilli can be successfully dealt with. In the recent South African War only three cases of tetanus occurred. Further, the animal experiments of Behring and Kitasato (1890) (*loc. cit.*) showed that by repeated small but increasing doses of the soluble tetanus toxin, animals can be rendered immune to this poison, and that the serum of such highly immunised animals renders inoffensive the tetanus poison with which it is mixed. The serum of horses which have been gradually accustomed to this poison until they support, without ill-effects, quantities many thousand times that required to kill horses not so treated, is a valuable therapeutic agent in human and veterinary surgery. This subject will, however, be referred to in greater detail later.

11654. You now have something to tell us about plague?—Plague is a disease of great antiquity, and previously seen by Nicolaïer in 1884, in the local wound some of the earliest human documents extant record the ravages of this pest. During 1905, 1,040,429 persons succumbed to plague in India. Over 350,000 deaths occurred in the Punjab alone (*official returns*). In some of the villages of this province 75 per cent. of the population were destroyed in two years. The mortality amongst Asiatics varies between 70 and 80 per cent., and amongst Europeans at the present time it is over 35 per cent. of those attacked. As long as the causation of plague was not comprehended, and the means by which it is spread were unknown, suitable preventive measures could not be rationally prescribed. Consequently, even in recent times, wealth and human effort have been squandered in directions that have led to no useful result. The constant association of a mortality of rats and mice, with outbreaks of plague amongst human beings, has been forced upon the minds of many ancient people, and is found to exist among many primitive races of mankind at the present time. The true significance and importance of this observation could not, however, be realised until the causation of the disease was discovered by Yersin and Kitasato (*Lancet*, 1894) in Hong Kong in 1894. These observers found a bacillus constantly present in the swollen lymphatic glands (buboes) of patients suffering from the disease, and they succeeded in growing it upon artificial media. Inoculation of the cultures of this organism into animals reproduced the disease with all its characteristic symptoms. Once the cause of the disease was known, it was easy to determine that the disease from which rats and mice simultaneously suffered was also plague. All the conditions, favourable and unfavourable, to the existence of the infective agent, could now be studied, and the means whereby the infection leaves the bodies of persons or animals dead of the disease, could be determined. The relationship in time and space between human outbreaks and disease in rodents could be worked out, and the influence of epidemics amongst rats in the spread of the disease among human beings determined. The relationship of rat plague to human plague has been found to be of such a character as (taking into consideration the small extent to which bubonic plague is infectious from man to man) to indicate in the strongest manner that the spread of the disease in man is conditioned almost entirely by the occurrence of plague amongst rats in his vicinity. The Commission at present investigating the question of the spread of plague in India, have, by extensive experiments upon animals, succeeded in showing that the infection is carried from rat to rat by the agency of the fleas infesting these animals (*J. of Hygiene*, 1906 and 1907, *special numbers*). By the same agency they have been able to produce epidemics amongst other animals, guinea pigs and monkeys; whereas all other means failed to give rise to the epidemic spread of the disease. The Indian rat flea also feeds upon man when his natural prey is not available, so that a possible, and indeed probable, means whereby the infection is carried from rats suffering from the disease, to mankind has been established. Further experiments carried out in houses or huts, where many cases of plague occurred, have shown that animals (guinea pigs or monkeys) are attacked by the disease when placed in such infected huts, without protection from fleas; but if the simplest means is adopted to prevent the fleas from reaching the animals, by a gauze covering, or a layer of sticky paper wider than a flea can jump, or by suspending

the cage at a distance from the floor greater than a flea can leap, they have in no case contracted plague.

11655. Whose are these experiments with fleas?—The experiments of the present Commission which is investigating the plague in India, of the Advisory Committee of which I have the honour to be a member.

11656. Do these fleas which carry the plague from one to another get the plague themselves?—No, they do not suffer in health apparently. The plague microbes merely multiply inside their alimentary canal, without apparently detrimentally affecting them in the least.

It need hardly be insisted upon that the complete comprehension of the spread of the disease under natural conditions is essential to successfully devise preventive measures. Without such knowledge our efforts will, in all probability, be misdirected. A knowledge of the causation of plague has enabled a useful method of protective inoculation to be devised. Efforts towards the production of a curative serum have unfortunately, so far, been disappointing, as the means employed have not produced an anti-serum of sufficient potency.

11657. Your next head of evidence is the importance of experiments on animals in the diagnosis of infectious diseases?—The value of a means of arriving at an early diagnosis in the case of infectious disease of man and animals is considerable, because the physician or veterinary surgeon is, as a rule, unable to form a diagnosis until the disease has developed. This may mean a delay of weeks, or even months, during which precious time is lost, and precautions to prevent the spread of the disease omitted. Moreover, in mild or latent cases of a disease, it may be quite impossible to arrive at a diagnosis by ordinary clinical means. The advantages of early bacteriological diagnosis are not confined to the patient, but are of importance to the community, or the owner of stock or stable, for upon the early recognition and segregation of cases of infectious maladies, the probability of successfully stamping out the disease depend. Mild or abnormal cases of disease may be equally endowed with the power of spreading the infection. Recent researches into the epidemiology of diphtheria, typhoid, and cholera, have indeed proved, that persons convalescent from these diseases and in all respects perfectly healthy, may be innocent agents distributing broadcast these various infections. In many cases a bacteriological diagnosis of a disease can only be made by direct appeal to animal experiment; but in quite a number a simpler method by which this is avoided, but which has been arrived at in the first instance by experiments upon animals, can be employed. As instances of such methods I cite: (1) The recognition under certain conditions, of tuberculosis, anthrax, relapsing fever, diphtheria, syphilis, gonorrhoea, plague, and sleeping sickness, by observations upon the morphological staining and growth characters of the organisms contained in the material submitted to examination. (2) By taking advantage of the fact that the human and animal body when infected with certain diseases, *e.g.*, typhoid fever, cholera, and Malta-fever, elaborates substances in the blood which have the property of causing the individual organisms of these diseases to aggregate together into clumps, so-called agglutinins. As this agglutinating action is within certain limits only exercised upon the particular species of microbe which is responsible for the disease, a diagnosis may be arrived at. For this purpose a single drop of blood from the patient is adequate. (3) In the case of cholera and typhoid infections a further substance is present in the blood serum, which has the power of killing and dissolving up the respective bacilli. This action is specific, so that the test applied with a few drops of blood can be used for diagnosis. (4) Nos. 2 and 3 can be applied in the reverse direction, *viz.*, to identify microbes suspected of being typhoid or cholera, which have been separated from water or the dejecta and other excretions or discharges from the human body. In this case cultures of these organisms are submitted to the action of a small quantity of the serum, obtained from an animal immunised against infection with typhoid or cholera microbes. (5) The diagnosis of tuberculosis in cattle and of glanders in horses, in the early stages of the disease, when it cannot be recognised by clinical signs, can be made by the infection of a small quantity of tuberculin or mallein respectively. Tuberculin and mallein are the products which are formed by the growth of tubercle and glanders bacilli

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in broth. The organisms themselves are killed by boiling, and are then removed by filtration. Tuberculin and mallein in the quantities employed produce no effect when injected under the skin of healthy animals, but if the animals be suffering from the corresponding diseases, fever, and in the case of mallein, local swellings occur, thereby enabling a diagnosis to be made. They are extensively used to ascertain the existence of tuberculosis in dairy cattle, glanders among horses in large stables in the early stages of the disease. In the cases of glanders and tuberculosis the disease may exist for a long time in a latent form, the animals appearing during this stage to be in perfect health. In such animals the disease is liable to assume an acute and highly infective character. It is, therefore, of great advantage, especially to the owner of valuable stock, to be able to recognise the disease in this form, so that the infected animals may be removed from the vicinity of healthy stock.

11658. Your third head of evidence is the preventive and curative measures discovered by experiments upon animals, and requiring the inoculation of animals for their production and standardisation?—I begin with hydrophobia (rabies). Pasteur discovered that it was possible to modify the virulence of the microbes of chicken cholera and anthrax by growing them outside the animal body under particular conditions. He also found that the inoculation of such attenuated cultures gave rise to merely slight *malaise*, from which the animal speedily recovered. On subsequently inoculating those animals which had received cultures of diminished virulence with microbes of full virulence, he discovered that they had become possessed of a marked degree of resistance to the disease. On the basis of this observation, Pasteur, by carefully controlled experiments on animals, devised a method of protective inoculation against chicken cholera and anthrax. This Pasteurian vaccine has been extensively employed in many countries, and has been the means of saving wealth to stockowners. Pasteur had continuously before him the possible application of his discoveries in the sphere of animal diseases to the alleviation of human suffering. Amongst those diseases which had particularly appealed to his imagination was the tragedy of hydrophobia. In the case of an individual bitten by a dog suffering from rabies, no remedy was available apart from local cauterisation. The patient remained in suspense for months fearing the onset of this fatal disease. Reasoning from the analogy afforded by his previous discoveries, Pasteur thought it might be possible to take advantage of the interval between the infection and the development of the symptoms in hydrophobia to produce in the person bitten a sufficient resistance to the disease to prevent its development altogether. Pasteur commenced this inquiry in 1881 in conjunction with Roux, Chamberland and Thullier (*Comptes rendus*, 1882). His first efforts were directed to the isolation of the casual agent. In this he was unsuccessful, and it has not yet been accomplished. He discovered, however, that the virus existed in the nervous system of rabid animals in a pure form, and that it became attenuated by slowly drying it for two or three weeks at a temperature of 20 deg. C. He next showed that by previously inoculating his attenuated virus under the skin of dogs, these animals did not contract the disease when subsequently bitten by rabid dogs. Pasteur had shortly before shown that the inoculation into the brain of a dog of a minute portion of the nervous system of an animal which had succumbed to virulent rabies, invariably gave rise to the disease. Nevertheless, previous treatment of a dog with his attenuated virus was able to prevent the development of the malady in this case also. Having shown that it was possible to prevent the disease by treatment, previous to the inoculation with virulent virus, he tested the influence of his method when applied during the period between the infection and the development of the disease (*Pasteur, Chamberland and Roux, Comptes rendus*, 1885). The results were entirely successful, and in 1885 Pasteur was induced to try his method upon the human subject. Of Pasteur's first 350 patients, only one died. This was a little girl horribly bitten about the head and face 37 days before she arrived in Paris. Pasteur felt constrained to attempt his treatment upon this child, although the severity and situation of the bites, and the long interval before

treatment began, rendered the case hopeless. Since then anti-rabic inoculation has been employed all over the civilised world, wherever the disease has existed. There are at present 51 institutes for the carrying out of the treatment, distributed over Europe, America, Asia, and Africa. Between 1886 and 1905 more than 150,000 persons have been inoculated. The result of the treatment at the Pasteur Institute, Paris, are published each year in the "Annales de l'Institut Pasteur" under three categories.

11659. You say there are 51 institutes. Can you tell us how many there are in the United Kingdom? None. We have the good fortune to live on an island, and it was possible to get rid of hydrophobia by muzzling the dogs.

Those three categories are:—(a) Persons bitten by dogs, etc., proved to be suffering from rabies by animal experiment; (b) Persons bitten by dogs, etc., certified by veterinary surgeons to have rabies; (c) Persons bitten by dogs, etc., suspected to have rabies. The number of persons treated at Paris, and the mortality since the introduction of the treatment is given below:—

Year.	Cases Treated.	Deaths.	Mortality per cent.
1886	2,671	25	0.94
1887	1,770	14	0.79
1888	1,622	9	0.55
1889	1,830	7	0.38
1890	1,540	6	0.32
1891	1,559	4	0.25
1892	1,790	4	0.22
1893	1,648	6	0.36
1894	1,387	7	0.50
1895	1,520	5	0.33
1896	1,308	4	0.30
1897	1,521	6	0.39
1898	1,465	3	0.20
1899	1,614	4	0.25
1900	1,420	4	0.35
1901	1,321	6	0.33
1902	1,105	2	0.18
1903	628	2	0.32
1904	755	3	0.39
1905	727	3	0.41

The figures for the various other anti-rabic institutions are available, and the results are in most cases equally satisfactory. In the 150,000 persons treated in different parts of the world the mortality has been less than 1 per cent., and in most institutes less than $\frac{1}{2}$ per cent.* Rabies is a fatal disease, but it is impossible to estimate exactly how far persons bitten by animals known, or suspected to have been suffering from rabies, are liable to develop the disease. The reason why this cannot be done is that the circumstances, situation, and nature of the bites vary so greatly in different cases. I have gathered together all the reliable information I could get to form an estimate. Bouley, in an official inquiry into the history of 320 cases made under the Second Empire, found that 40 per cent. of the persons bitten contracted the disease. Leblanc, 1878-1883, head of the sanitary department of the Prefecture of Police in Paris, concluded from an examination of 515 cases that it was 16 per cent.; Faber (Wurtemberg), from 145 cases, 20 per cent.; Werner (Vienna), (1879-1886), from 429 cases, 11.8 per cent.; and Högges (Buda-Pesth), from 470—9.3 per cent. That was the case

* The average mortality of the 150,000 cases indiscriminately collected by myself from all the sources readily accessible to me is 5 per cent.—C. J. M.

when I prepared my statement; but including later details Hogyes (Buda-Pesth) concluded from 855 cases that it was 13.9 per cent. If all the cases are added up together, and the number of deaths, the average comes to 17.9 per cent. as the estimated mortality when bitten by a rabid dog. That estimate is based upon 2,264 cases.

11660. I thought these were percentages of the persons bitten who had contracted the disease. Is this last figure the average mortality?—Yes. I have added the number of cases observed by each man together, and the number of deaths returned by each man.

11661. (*Sir John McFadyean.*) Of persons untreated?—Yes, this is of persons untreated.

(*Chairman.*) It is not quite clear. You begin by saying that Bouley, in an official inquiry into the history of 320 cases made under the Second Empire, found 40 per cent. of the persons bitten contracted the disease.

(*Sir John McFadyean.*) They all died.

11662. (*Chairman.*) It does not say so. That is assumed?—In the paragraph before I stated that rabies is a fatal disease.

11663. (*Sir John McFadyean.*) You might say "invariably"?—I will put, if you think it is stronger, "invariably fatal."

11664. (*Mr. Tomkinson.*) Rabies in the dog?—No rabies in man.

11665. Do you not call it hydrophobia?—If you like I will call it hydrophobia—hydrophobia is an invariably fatal disease in man.

11666. But dogs do not have hydrophobia; the dog has rabies?—Rabies is one name for the disease, and hydrophobia is another.

11667. Is it correct to use the word hydrophobia in the case of a dog?—I think so, certainly.

11668. I thought not. I thought the dog did not have the fear of water.

11669. (*Sir William Collins.*) Do all these figures refer to the period prior to inoculation?—No; some figures are collected later; but they are all of cases which have not undergone treatment.

11670. But your figures in the Table start with the year 1886?—Yes.

11671. Is that what you regard as the commencement of the inoculation period?—Yes; but everybody does not undergo treatment for rabies even now.

11672. Would you be so good as to answer that question? Do you regard 1886 as the period from which the inoculation method was introduced?—Certainly.

11673. Do the figures which you have just given us in regard to the number of cases that were bitten and attacked relate to the pre-inoculation period?—Not entirely.

11674. Do all except those that you have just added with regard to Buda-Pesth?—No, not entirely.

11675. It seems desirable to separate those, because there may be a question whether they are all of the pre-inoculation period or whether you are adding some cases now which though not treated belong to the post-inoculation period?—In the case of Buda-Pesth that is so.

11676. Then will you be so good as to indicate in what cases you are dealing with the post-inoculation period?—I am not certain off-hand about Faber's at Wurtemberg; but I believe those were of pre-inoculation date.

11677. Then are there any except the additional cases which you have added from Buda-Pesth, which were of the post-inoculation period?—I am not sure about Faber's, without looking the thing up again.

11678. That is the only exception?—Obviously for the others I put the dates in.

11679. I gathered from the dates that you were purposely obtaining cases which were pre-inoculation cases; then when you added the Buda-Pesth cases I wondered whether they related to post or to pre-inoculation. I understand now that they relate to post-inoculation?—Yes, but untreated.

11680. (*Sir John McFadyean.*) I suppose you really took all the statistics that you could find on that subject?—Yes.

11681. You did not select them because they were of the pre-inoculation period?—No.

11682. There have been no records made of those of Bouley and others?—No.

11683. You could not find any in the literature to the same extent?—No.

11684. Are you sure that the Pasteur treatment only began in 1886. It would be rather extraordinary if in the first year's treatment 2,671 persons were treated. Were not those the first published statistics merely?—Yes, you are right; I have previously stated that Pasteur commenced to treat patients in 1885. The return for 1886 includes the year before, too.

11685. (*Sir William Collins.*) But you state the mortality since the introduction of the treatment began with the year 1886?—Yes, that is the first year for which the returns were published and are available.

11686. (*Sir Mackenzie Chalmers.*) But still they are untreated cases?—Yes.

11687. Does it make any difference whether they were bitten before or after the invention, as long as they were not treated?—I imagine not.

11688. I do not know. It might?—I see no reason why it should.

11689. There is just this point, that the more severe cases might be treated, and the untreated cases would be the more doubtful cases?—That might be so, of course. But that will be in favour of my argument.

11690. I know?—I can only deal with the figures as they are available.

11691. (*Sir William Collins.*) And it might not be so?—It might not be so.

11692. (*Sir John McFadyean.*) There is just one other question I should like to ask you. I should like to be quite clear whether these statistics relate to people who were bitten merely by dogs suspected of rabies; because many returns distinguish between persons bitten by dogs suspected of rabies and those bitten by dogs experimentally proved to have been rabid or certified by a veterinary surgeon to have been rabid. The older statistics were not experimentally proved. They were on the opinion of the veterinary surgeon. The most careful statistics, so far as I can judge, are the last ones from Buda-Pesth, from Hogyes. He was the director of one of the biggest Pasteur Institutes in the world, which is situated in Buda-Pesth. And in Hungary hydrophobia is particularly important as a disease.

11693. (*Sir William Collins.*) But would the figures which appear in your statement be correct, that of the 470 case of Hogyes an attack rate of 9.3 per cent. was observed?—That was in the first 470 cases.

11694. Would you give us separately what the additional cases are which you now add which produce an attack rate of 13.9 per cent. in the total?—I cannot do it now.

11695. It will complete the statement?—Yes.

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In searching the literature in order to comply with the request of the Commissioner, I have discovered further figures relating to the mortality amongst persons bitten by dogs judged to have been suffering from rabies and not treated by the Pasteurian inoculations. The list giving date and place of observations and source of information is as follows:

Figures collected by.	Locality.	Date.	Number of Cases.	Number of Deaths.	Mortality (per cent.)	Source of Information.
Faber	Wurttemberg - -	1846	145	28	20	Hogyés article "Lyssa" Nothnagel's <i>Spezielle Pathologie u. Therapie</i> Bd. V. T. 5. p. 829.
Bouley - - -	France - - -	1860-70	320	128	40	Life of Pasteur. Valerie-Radot English translation, p. 267.
Leblanc - - -	Department of Seine.	1878-1883	515	81	16	Menetrier, article "La rage" <i>Maladies Communes a l'homme et aux animaux</i> , Paris, 1906.
Werner, Vienna General Hospital	Vienna - - -	1879-1886	429	44	10.3	Menetrier, article "La rage" <i>Maladies Communes a l'homme et aux animaux</i> , Paris, 1906.
Hogyés - - -	Hungary (Official enquiry, Minister of the Interior)	1885-1888	470	44	9.3	Hogyés <i>loc. cit. supra.</i>
Hogyés - - -	Hungary (Official enquiry, Minister of the interior)	1890-1895	985	147	14.9	Hogyés <i>loc. cit. supra.</i> }
Kurimoto - - -	Nagasaki - - -	1892	67	21	31.3	Kurimoto. Virchow's, Arch. 1899. Bd. 158. p. 148.
Kurimoto - - -	Naga-aki - - -	1893	145	15	10.3	Kurimoto. Virchow's, Arch. 1899. Bd. 158. p. 148.
Kurimoto - - -	Nagasaki - - -	1894	51	9	17.6	Kurimoto. Virchow's, Arch. 1899. Bd. 158. p. 148.
Total cases - - - - -					- - - - -	3,127
Total deaths - - - - -					- - - - -	517
Average mortality (per cent.)					- - - - -	16.5

I should like to interject that I have not selected these figures; they are simply all that are available to me for basing a judgment upon as to what the chance of death is if you are bitten by a mad dog. The average of the above is 17 per cent., and is the best estimate that I can arrive at. Assuming this as correct, and taking the mortality of patients treated as high as 1 per cent., the method of Pasteur has in 20 years saved upwards of 25,245 persons from the horrible disease of hydrophobia, and all the attendant suffering to their friends and relations, as well as relieving 148,500 persons of the awful suspense for six months, after having been bitten by a dog suspected of rabies.

11696 (Chairman.) You come next to serum therapeutics in connection with experiments on animals?—Many organisms, and notably those of diphtheria and tetanus, exercise their harmful influence by the secretion of a soluble poison or toxin, which is absorbed from the seat of infection and circulated in the body of the infected animal. One of the means of defence against the invasion of such organisms which is possessed by the animal body, is the capacity to manufacture and set free in the blood stream substances which combine with the toxins, and so render them inert. By habituating a large animal, such as a horse, to the action of the toxin in increasing quantities, some cells or fluids of its body can be thereby stimulated to produce antitoxins, which are then thrown into the blood stream. These antitoxins, when they have reached a sufficient strength, may be employed as a remedy for the disease in man or animals. Von Behring must be regarded as the founder of serum therapeutics. In 1890 he, with Kitasato (*loc. cit.*), showed that the blood serum of an animal, which had been immunised against tetanus, was capable, when injected into other animals, of protecting them, not only against poisoning by the tetanus toxin, but also against infection with living tetanus bacilli. He also proved that under certain conditions a curative action could be demonstrated in animals which already presented symptoms of tetanus infection. Similar though less striking results were described in the case of diphtheria. By showing that the horse reacted to repeated and in-

creasing injections of diphtheria toxin, by manufacturing and circulating in its blood such quantities of antitoxin that a few cubic centimetres of their serum was possessed of sufficient curative power to be useful in the treatment of diphtheria in the human subject, Roux (*loc. cit.*) and Ehrlich first brought the subject from the experimental to the practical stage.

11697. How are the antitoxins prepared?—For the preparation of antitoxins on a large scale the horse is the animal now almost exclusively used. The horse can be suitably immunised against tetanus, diphtheria, and many other diseases and intoxications. In the case of tetanus, diphtheria, and dysentery the sera obtained attain to a very high antitoxic value.

11698. What precautions are taken in the selection of the horses for the preparation of antitoxin?—The horses are selected by a skilled veterinary surgeon. They must be free from all signs and symptoms of disease. Every horse is tested for latent glanders with mallein, and even a suspicion of a reaction is regarded as a sufficient basis for the rejection of the animal. If the animal has satisfactorily passed the test, he is received into the establishment, but is isolated and kept under observation for some time. Having passed through this period of probation, the animal is admitted into the stables, and the process of immunisation is commenced. During the process the animal is kept under the strictest observation—his temperature is duly registered, and the greatest possible care is taken in regard to his comfort and feeding.

11699. (Sir William Church.) Ought you not to qualify that statement by saying where these precautions are taken? It is rather a bold thing to say that they are taken everywhere. You have no means of knowing how strictly the precautions are carried out, except in certain establishments?—I know that these precautions are taken in our own institute; and I think I can safely say that they are taken in the other places where sera are made in this country—that is in Burroughes and Wellcome's.

11700. (Chairman.) Is that from what has been told you?—No; I have seen the way they work.

11701. You have visited their place?—Yes.
11702. (*Sir William Church.*) Would it not be safer to say that in this country the precautions are taken?
11703. (*Chairman.*) You mean in this country; you do not mean all over the world?—I am not aware of all over the world.
11704. At any rate, we had better confine ourselves to this country, because that is all we have to inquire into.
11705. (*Dr. Wilson.*) May I ask, do you use mallein after the horse is bled, for the production of serum? You always use it before; but do you ever use it while the horse is in the institute for experiment?—No. The mallein test is performed before the horse comes to us by the veterinary surgeon.
11706. What would be the effect of using it if used after the horse has been experimented on, after the toxin has been used for a horse that has been bled for some time?—I do not think there would be any effect.
11707. (*Chairman.*) At any rate, in your evidence you speak with confidence about your own institute?—Certainly.
11708. And as to other institutes in this country, I understand it to be your belief, from personal observation, that the process is carried out strictly there in the same way?—Certainly.
11709. Will you describe the process of immunisation?—The animal is treated with increasing doses of the particular poison. The toxins are injected subcutaneously, intramuscularly, or intravenously. At first either very minute doses of weak toxins, or toxins which have been modified by chemical agents, or in others ways, are employed. In the case of tetanus, in the early stages the toxin is usually modified by being treated with iodine. The first injection of the toxin may be followed by swelling at the site of inoculation, loss of appetite, general malaise, and rise of temperature. When these have passed off the animal receives a second rather larger injection, and in this way the quantity of toxin is increased until, within a few months, the horse is capable of tolerating many thousand multiples of what would be a lethal dose if given as a first injection. When the serum has reached the strength suitable for clinical use, blood is withdrawn from time to time by venesection. All the processes connected with immunisation are carried out under the strictest antiseptis.
11710. What precautions are employed in preparing the different sera?—The vessels containing the sterile blood are removed into a small isolated building used for this purpose only. The serum, when it has separated from the clot, is decanted into sterilised vessels, the usual bacteriological precautions being taken to prevent the admission of extraneous germs. To the serum thus obtained 0.2 to 0.3 per cent. carbolic acid is added. As a final precaution the serum is passed through a Berkefeld filter. It is then in a condition to be put into the small phials in which it is sent out for use. Before this is done, however, it is subjected to the following tests:—1. For sterility. It must be germ-free. 2. For absence of toxicity. 3. For antitoxic value. This much reach a required standard. The last two tests are carried out on small animals. The serum is also kept for a certain time, during which the horse remains under observation; if during this time the animal shows any signs of illness the serum is thrown away. It must be clearly understood that antitoxin is a substance which circulates in the blood of immunised animals, and which has been elaborated by the animal body. It has the property of rendering inert the particular poisons with which the animals were treated. It is a chemical substance which neutralises these poisons. Antitoxins are produced when a variety of poisons (all, however, of a complex nature) are introduced into an animal in suitable quantities. This property of stimulating the productions of antitoxins by the animal body is not confined to the poisons produced by the organisms of disease. Snake poisons, most ferments, and several vegetable poisons also give rise to the production of anti-bodies, which neutralise the particular poison injected. Antitoxins exert no influence upon the disease germs themselves, but only upon their products; the bacillus of diphtheria grows excellently well in diphtheria antitoxin. Unfortunately, the useful application of antitoxic serum-therapy is at present limited to a comparatively few diseases. The most important of these are diphtheria, tetanus, and epidemic dysentery. The

value of serum-therapy in snake bite is limited, owing to the fact that the horse will not manufacture and store in its blood anti-venom beyond a certain amount. Once a certain concentration of antitoxin is reached the animal ceases to react in this way to further injections of venom. Moreover, a different antitoxin is required for nearly every species of snake, which imposes practical difficulties in the way of this treatment.

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11711. You have prepared, I think, various tables showing the curative value of diphtheria antitoxin?—At the Medical Congress at Buda-Pesth in 1894 Roux communicated the results of the treatment with antitoxin of 300 cases of diphtheria in the Paris Hospital. The mortality of those treated with antitoxin was 24 per cent., as against 60 per cent. in the non-treated. In another hospital in Paris, where antitoxin was not yet used the mortality was 80 per cent. The experimental and clinical data, and favourable statistics brought forward by Roux, led to the trial of this method of treatment for diphtheria in all civilised countries. Diphtheria antitoxin came into use in England in 1895, and immediately a fall in the case mortality in this disease was noticed in those institutions where it was administered. I give some instances of the case mortality from diphtheria before and after the employment of the antitoxic treatment:—

PARIS HOSPITAL (HERICOURT, 1899).

Year.	Case mortality per cent.
1893 - - - - -	45
1894* - - - - -	31
1895 - - - - -	12
1896 - - - - -	15.2
1897 - - - - -	13.2
1898 - - - - -	14.5
Average 14 per cent.	

* Commencement of the Antitoxic treatment.

STATISTICS OF CASES OF DIPHTHERIA TREATED IN THE METROPOLITAN ASYLUMS BOARD HOSPITALS. (Annual Reports of M.A.B. Medical Supplement.)

Year.	Cases treated by Antitoxin. Per cent. of all cases.	Mortality. Per cent. of all cases.
1888-93 - - - - -	—	28.5
1894 - - - - -	—	29.6
1895* - - - - -	61.8	22.5
1896 - - - - -	71.3	20.8
1897 - - - - -	80.2	17.5
1898 - - - - -	81.4	15.5
1899 - - - - -	—	13.95
1900 - - - - -	—	2.01
1901 - - - - -	—	12.5
1902 - - - - -	—	11.0
1903 - - - - -	—	7
1904 - - - - -	—	10.1
1905 - - - - -	—	8.3

* Commencement of the Antitoxin period.

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CASE MORTALITY OF DIPHTHERIA FOR THE COUNTY OF LONDON. Calculated from the curves in Report of the Public Health Committee, L.C.C., 1904.

Year.	Per cent.
1892	21·6
1893	24·9
1894	23·2
1895 (Antitoxin introduced)	20·4
1896	19·9
1897	16·6
1898	15·0
1899	13·3
1900	12·1
1901	11·6
1902	10·8
1903	10·0
1904	10·0

CASE mortality from Diphtheria in 31 of the largest English towns from 1894 to 1904. (Armstrong, 33rd Annual Report of the M.O.H. Newcastle-upon-Tyne for 1905. Appendix E).

	Per cent.
1894	30·5
1895	33·1
1896	31·3
1897	25·1
1898	25·4
1899	25·2
1900	22·7
1901	22·0
1902	20·8
1903	17·7
1904	18·0

In the County of London a large proportion (65 per cent.) of cases of diphtheria are removed to the Metropolitan Fever Hospitals, where nearly every case is adequately treated with antitoxin.

In the provincial towns the proportion of antitoxin-treated cases to total cases is, I believe, considerably less.

CASE mortality in New York City from Diphtheria from 1891 to 1899. (Billings, New York Medical Journal, 1900).

Year.	Cases.	Deaths.	Mortality per cent.
1891	5,346	1,970	36·7
1892	5,184	2,196	40·6
1893	7,021	2,558	36·4
1894	9,641	2,870	29·7
1895 *	10,353	1,976	19·1
1896 †	11,399	1,763	15·4
1897	10,896	1,590	14·6
1898	7,593	923	12·2
1899	8,240	1,087	13·1

* Antitoxin introduced.
† Use of Antitoxia became general.

VIENNA.—Siegert Jahrluch for Kinderheilkunde, 1902.
4,894 cases of diphtheria.

Year.	Mortality per cent.
1892-4	41
1895-7	20

BUDA-PE-TH.—St. Ladislaus Hospital (Gertoezy, Wiener Klinische Rundschau, 1896).

Year.	Number of cases.	Mortality per cent.
1893	213	48·8
1894	227	44·9
1895*	401	19·9

* Commenced using Antitoxin.

RUSSIA.—St. Petersburg Hospital, Rauchfuss. (XII. International Medical Congress, Moscow, 1897. Centralb. f. Bakt. Vol. 25.)

Year.	Cases.	Mortality per cent.
1870-4	304	59·5
1875-9	610	48·5
1880-4	1,425	58·1
1885-9	735	55·2
1890-4	792	55·2
1895-6*	801	22·7

* Commenced using Antitoxin.

THE results for 51 Russian Provinces for 1895 and 1896 (Rauchfuss loc. cit. were as follows:—

	Number.	Mortality per cent.
Cases treated with Antitoxin	44,631	14·6
Cases not so treated	6,507	34·1

Rosenthal (Medical Press and Circular, 1900, II., p. 293) collected the records of 132,548 cases of diphtheria treated with serum during the years 1894-9, and compared them with 183,256 collected from similar sources before the introduction of serum treatment. The results were as follows:—132,256 cases before serum treatment, mortality 38·4 per cent.; 132,548 cases treated with serum, mortality 14·6 per cent.

11712. Where did Roux collect those statistics?—From Europe and America. That is simply collecting together all available statistics from various countries.

11713. You have some general conclusions which you draw from those statistics?—In drawing conclusions from statistical data of this kind it is obvious that other factors may have to be examined and taken into account as well as serum or no serum. The incidence of a disease like diphtheria is not uniform. It occurs in waves. In certain years it is more prevalent than in others, and no doubt the severity of the disease also varies somewhat in different periods. Another factor which will undoubtedly have modified the case mortality of diphtheria is the adoption of a more accurate method of diagnosing the disease by bacteriological examination. This will most certainly have led to the wider recognition of the disease in mild and atypical cases, and therefore to the inclusion in the statistical data of a number of less severe cases of the disease. The influence of this factor in diminishing the case mortality cannot be directly measured. The universal decline in case mortality coincident

with the employment of a remedy based on rational lines, and capable of being experimentally demonstrated to certainly prevent infection or cure the disease in animals, is, however, highly significant. Moreover, in the early experience in Paris and in that in Russian provinces, quoted above, the mortality of treated and untreated cases at the same time and under similar conditions can be compared. The result is just the same, viz., a case mortality of less than half amongst cases treated with serum.

11714. Then you give particulars as to the mortality from diphtheria per 100,000 of the population living, in relation to the employment of antitoxin?—The employment of a remedy of any value will be a factor in diminishing the death-rate of a disease per 100,000 of the population living. The influence of this factor will be proportionate to the value of the remedy and to the extent to which it is employed. Other factors, however, which must be taken into account, are the prevalence of the disease, and any variation in the severity of its type. As I have already pointed out, the prevalence of diphtheria shows considerable variation from year to year. Groups of bad diphtheria years have occurred, followed by groups of years in which the prevalence of the disease has been less. It will be obvious that if the prevalence in proportion to the total population were doubled and the type of the disease remained the same, the mortality per 100,000 would also be doubled. Again, supposing that a remedy adequately administered halved the case mortality and other factors remained stationary, the mortality per 100,000 would only be halved on condition that the remedy were administered in sufficient amount in every case. If both factors were varying at the same time the result due to the one might be masked by the other. For this reason it is impossible to draw conclusions regarding the value of anti-toxin from general mortality figures alone. I think in previous evidence a question was asked about Hull, as regards the greatly increased mortality from diphtheria in Hull during the last few years, notwithstanding the introduction of the antitoxin. I have the figures for those Hull cases, and they show that the increased mortality is due to increased prevalence. The case mortality for the whole of the cases notified, according to the returns afforded me by the Medical Officer of Health for Hull, remains substantially the same or slightly lower during the last few years than what it was before the antitoxin period. There is, however, a substantial improvement in the mortality of cases treated in hospital where antitoxin was used to a large extent.

11715. (Sir Mackenzie Chalmers.) The case mortality of persons treated?—Yes.

11716. (Dr. Wilson.) The death-rate from the disease per 1,000 of the population has gone up?—The death-rate has gone up. But then they have had an increased number of cases. And I would like to

insist that the antitoxin does not prevent the spread of the disease.

11717. It is used as a prophylactic?—It merely cures the patient.

11718. (Chairman.) The individual patient?—Yes.

11719. (Dr. Wilson.) But it does cure them all, if so many die?—It does not cure them all, but they do not all get it in sufficient amount.

11720. (Sir Mackenzie Chalmers.) A great many are not treated?—No, the average amount the cases must have had in Hull is an extremely small amount, and can be estimated, because they supplied antitoxin free of cost to the profession for their cases, and the amount employed was only sufficient, if equally distributed over those cases, to supply—I am speaking roughly—some-where between 2,000 and 3,000 units per case. That is found inadequate for treating diphtheria, by the experience of the Metropolitan Asylums Board, who give 20,000 units per case.

11721. (Dr. Wilson.) But in early days was not even 2,000 units considered to be a large dose?—Yes. Of recent years the dose of anti-toxin which has been given has steadily risen, and with the increase in dose that has been given the case mortality has come down, as shown in all the figures everywhere.

11722. (Colonel Lockwood.) You say that 20,000 units are given now generally?—By the Metropolitan Asylums Board; that is their average.

11723. (Dr. Wilson.) For slight and severe cases?—At once, in every case directly it comes in within the first 48 hours.

11724. (Sir Mackenzie Chalmers.) Even if it is of several days duration?—Yes, unless it is moribund they always give anti-toxin at the Metropolitan Asylums Board hospitals.

11725. (Chairman.) Your original observation was tending to point out the unreliability, to a certain extent, of statistics?—Of the general mortality statistics. The case mortality statistics are perfectly reliable.

11726. (Sir William Church.) You did not mean the Commission to infer that they only used anti-toxin in doses of 2,000 units at Hull, but that the total supply of anti-toxin to Hull would only afford 2,000 units instead of 20,000?—Yes. Of course that is the most I can say.

11727. (Sir John McFadyean.) Which may mean that those who received adequate doses were only 10 per cent. of those treated at all?—Possibly. I can put those figures in evidence if you like to have them.

11728. (Chairman.) Which figures?—The Hull figures. It did not seem to me that Mr. Power was able to give you the answer to the question you asked him. That led me to communicate with the Medical Officer of Health for Hull.

11729. Perhaps you will furnish those figures, and add them to your evidence?—I will.

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DIPHTHERIA.

CITY AND COUNTY OF KINGSTON-UPON-HULL.

(From Returns supplied by the courtesy of Dr. J. Wright Mason, M.O.H., Hull).

Year.	Population.	Number of Cases Notified.	Total Deaths in City, including those in Hospital.	Number of Cases Admitted to Hospital.	Deaths in Hospital.	Case Mortality in Hospital.	Case Mortality of all Cases.
1892 -	204,750	95	10	13	1	7.7	10.5
1893 -	208,639	105	20	9	4	44.4	19.0
1894 -	212,679	86	19	10	3	30.0	22.0
1895 -	216,722	105	25	12	3	25.0	24.0
1896 -	220,844	156	39	2	1	50.0	25.0
1897 -	225,045	195	19	-	-	-	10.0
1898 -	229,887	119	10	-	-	-	9.0
1899 -	234,270	157	23	2	-	-	14.6
1900 -	238,736	99	15	9	3	33.3	15.0
1901 -	241,622	231	38	57	11	19.3	16.0
1902 -	245,448	480	85	202	27	13.3	17.7
1903 -	249,639	494	75	183	27	14.7	15.4
1904 -	253,865	526	61	164	23	14.0	11.0
1905 -	285,127	700	72	224	31	13.8	10.0
1906 -	262,426	1,253	135	336	44	13.0	10.7
Average Case Mortality before antitoxin used					Total Cases.	Hopeful Cases.	
" after " "					16.2	22.8	
Use of Serum commenced in November, 1901. " "					12.4	13.7	

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11730. You go on to distinguish between diphtheria and the laryngeal cases which, you say, are in the nature of diphtheria. Will you tell the Commission what is the effect of the serum treatment upon the mortality of laryngeal cases?—In a certain proportion of cases the local inflammation of diphtheria, which commonly commences in the throat or nose, extends to the larynx and trachea. This extension increases the gravity of the case, both owing to the larger surface involved, and to the frequent interference with the entrance of air through the narrow orifice in the larynx. To prevent suffocation laryngeal cases commonly require tracheotomy or intubation. The mortality of laryngeal cases has always been much higher than the mortality of those in which the local manifestation of the disease was confined to the nose or throat. In testing the effect of antitoxic treatment upon laryngeal diphtheria there is therefore no question of the inclusion of an unusual proportion of mild cases, so that the mortality of laryngeal cases before or after the introduction of the treatment may safely be compared, and any improvement in the death rate placed to the credit of the antitoxin. Statistics of cases of diphtheria requiring tracheotomy or intubation have, in recent years, been carefully kept by most hospitals. In 8,927 cases collected by Goodall ("British Medical Journal," 1899, volume i., page 197), from hospital records in different countries before 1894, the mortality was 71.6 per cent. With the introduction of antitoxin there was a marked fall in the mortality of cases of diphtheria requiring tracheotomy. In a series of 2,374 cases collected by the same author from similar sources, but in which antitoxin was employed, the mortality was 36.6, or approximately one-half. The same order of improvement occurred

in all the principal hospitals of Europe and America in which antitoxin was administered. With the object of coming to a conclusion regarding the curative value of diphtheria antitoxin, the German Health Office (Arbeiten aus dem Kaiserlichen Gesundheitsamte, 1897, 254) obtained returns of diphtheria cases from all the hospitals in the Empire from April, 1895, to March, 1898. Amongst these were 2,041 cases in children up to five years of age, in which tracheotomy had to be resorted to. Of these 778 died; a mortality of 33.6 per cent. The German health authorities compare this mortality with that of tracheotomy cases in children's hospitals in Germany, previous to the introduction of antitoxin, which, according to the statistics of Bazinsky, Hirsch, Herz, Gläser, and Feer, varied from 58.4 to 86 per cent., depending upon the age of the children.

11731. Are these cases in the report of the German Health Office cases in which antitoxin had been used?—In the first series of cases antitoxin was used with a mortality of 33 per cent.

11732. When you say returns of diphtheria cases from all the hospitals, does that imply that they were diphtheria cases which had been treated in all cases with antitoxin?—Yes, the treatment was general in hospitals in Germany within those years.

11733. Then you give some results obtained by the antitoxin treatment in tables, showing the influence of early or late administration of antitoxin upon the case mortality from diphtheria?—Data regarding this question have been collected by numerous authorities and the total figures amount to upwards of 20,000 cases.

The Results obtained by Antitoxin Treatment, according to the period after the onset of the disease at which the injection is given (*Dieudonné Schutzimpfung u. Serumtherapie*, 1900.)

Author.	Total of Cases.	Percentage of Total Mortality.	First Day.	Second Day.	Third Day.	Fourth Day.	Fifth Day.	Sixth Day.	After Sixth Day.
Welch	1,189	14.2	2.3	8	13.5	19.0	29.3	34.1	33.7
Hilbert	2,428	18.3	2.2	7.6	17.1	23.8	33.9	34.1	38.2
Collective Investigation of the American Paediatric Society	5,794	12.3	4.9	7.4	8.8	20.7	35.3	—	—
Collective Investigation in Austrian Sanitary Department	1,103	12.6	8.0	6.6	9.8	25.5	28.8	30.7	21.0
Collective Investigation of the Imperial German Health Office	9,581	15.5	6.6	8.3	12.9	17.0	23.2	—	26.9

Mortality at the Brook Hospital (Metropolitan Asylums Board) 1897 to 1902, according to the day on which Antitoxin Treatment began. (*Report Metropolitan Asylums Board for 1902. Lancet*, 1903.)

Day of the Disease on which Treatment Commenced.	Mortality per cent					
	1897.	1898.	1899.	1900.	1901.	1902.
First	.0	.0	.0	.0	.0	.0
Second	5.4	5.0	3.8	3.6	4.1	4.6
Third	11.5	14.3	12.2	6.7	11.9	10.5
Fourth	19.0	18.1	20.0	14.9	12.4	19.8
Fifth	21.0	22.5	20.4	21.2	16.6	19.4

Eight thousand and three cases of bacterially-verified Diphtheria treated with Antitoxin by the Chicago Health Department between 1895 and 1905. (*Biennial Report of Department of Health, Chicago*, 1904-5, p. 137.)

Day of Disease	1st.	2nd.	3rd.	4th.	Later.
Number of Cases	608	2,063	2,802	1,496	1,034
Cases of Mortality per cent.	.32	1.66	3.64	11.03	21.08

From the above records it is clear that those cases in which antitoxin was administered on the first day have the lowest mortality, and that the death rate rapidly rises as the period between the onset of the disease and administration of the serum increases. This is exactly what happens in animal experiments. If antitoxin be administered to an animal on the same day as it is injected with a fatal dose of diphtheria bacilli, 100 per cent. can be saved. If on the day after a large percentage of treated animals recover. If on the third day only a small percentage; and after the third day antitoxin is unable to prevent death—that is in animals. In other words, antitoxin is a remedy definitely known to be proportionately more effective the earlier it is administered, and the experience quoted above, derived from a wide source, shows that this is the case when the serum is administered to children, just as was to be expected from animal experiments. If antitoxin were a remedy of no value, whether it were administered on the first or fifth day of the disease would be immaterial.

11734. What is the dangerous date in diphtheria? At what days would you expect in a bad case, in an average case, say, a crisis?—On which day would death take place, do you mean?

11735. Yes?—They may die any time after the second or third day, in very bad cases.

11736. What is the effect of antitoxin upon the incidence of diphtheria paralysis?—The incidence of paralysis in cases of diphtheria treated in the hospitals of the Metropolitan Asylums Board has risen about 6 per cent. since antitoxin was introduced. This was to be expected, from experimental knowledge of the action of diphtheria poison and of the influence of antitoxin upon an animal which had been either injected with this toxin or inoculated with the bacilli. If antitoxin be given to an animal in sufficient quantity and sufficiently early in the disease, both the acute symptoms and the paralysis, which may occur about three weeks later, are prevented. On the other hand, when the dose of antitoxin is not quite adequate the acute symptoms may be prevented, but the late paralysis may still supervene. Reasoning from analogy, it would seem probable that the increased percentage of paralysis is due to a considerable number of cases which would otherwise have died of acute poisoning, being saved by late or insufficient administration of antitoxin. This supposition is supported by the facts adduced in the following table, indicating the proportion of paralysis which occurred amongst patients treated with antitoxin upon the first, second, third, fourth, and fifth days of the disease respectively.

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Day of Disease upon which Antitoxin was first injected.	Total number of Cases.	Total number of cases of Paralysis.	Percentage incidence of Paralysis.	Number of severe cases of Paralysis not fatal.	Number of fatal cases of Paralysis.
First - - - - -	69	4	5·7	0	0
Second - - - - -	277	28	10·1	1	0
Third - - - - -	340	53	15·5	5	3
Fourth - - - - -	323	61	18·8	8	7
Fifth, and later - - -	571	147	25·7	28	8
Total - - - - -	1,580	293	18·5	42	18

11737. Then you give a summary of the facts?—The foregoing facts relating to the influence of antitoxin in the treatment of diphtheria may be briefly summarised as follows:—(1) Diphtheria can be cured with certainty in animals by treatment with antitoxin. (2) The earlier the treatment is applied to an animal the less the quantity required and more certain the cure, and if the administration of antitoxin is delayed beyond a certain time the treatment is ineffectual. Those are the facts as regards animals. (3) A reduction of case mortality of diphtheria to one-third or one-half occurred universally coincidently with the introduction of the antitoxin treatment. (4) In many places a corresponding diminution in mortality in proportion to population occurred; but in some countries this has been masked by a simultaneous increase in the prevalence of the disease. (5) A similar fall in case mortality amongst the severest forms of diphtheria, viz., those necessitating tracheotomy, has been universally experienced, so that the apparent beneficial results of serum treatment cannot be explained by the supposition that after 1895 a milder form of the disease was suddenly and universally encountered. The beneficial results of serum treatment are greater, the earlier in the course of the disease the treatment is applied, and after the fourth or fifth day the administration of anti-toxin produces little or no influence upon the case mortality. This is absolutely in accord with what was known of antitoxin from the results of experiments upon animals.

11738. Do you attribute the discovery of the serum and the efficiency of it and the way to use it, to experiments on animals?—Entirely.

11739. Then you have something to say as regards the prophylactic action of diphtheria antitoxin?—The use of antitoxin as a remedial agent does not materially diminish the spread of the disease, for an individual remains infective for weeks, or sometimes

months after convalescence. With the object of stamping out epidemics of diphtheria, it can, however, be employed as a prophylactic measure with great advantage. As previously mentioned the shorter the interval between the inoculation of an animal with diphtheria bacilli or diphtheria toxin and the treatment with anti-toxin the less is required to prevent a fatal issue. When the antitoxin is administered before the bacilli or toxin the amount of the former required is still further diminished, and a complete protection can be obtained. This prophylactic use of serum can be advantageously employed to prevent the spread of the disease in schools and institutions, and so readily suppress outbreaks of the disease. Hitherto, in England, diphtheria antitoxin has not been much used as a prophylactic, though in certain outbreaks, such as those at Cambridge and Chesterton, it was employed with considerable success (*Cobbett, J. of Hygiene, Vol. I.*). In France, Germany, and America, however, it is commonly employed with this object, and very valuable results have been obtained in this direction. The following experience occurred in Heubner's clinic (*Lohr. Jahrbuch f. Kinderheil kunde Bd. 43, Ht 1, and Slawyk, Die Therapie der Gegenwart, 1899*). From 1895 onwards prophylactic injections were given every three weeks to all the patients, and the outbreaks of diphtheria which had formerly regularly occurred in the wards, came to an end. In October, 1897, this precautionary measure was stopped in a wing not used for infectious cases, and in November cases of house infection broke out in this department. A general prophylactic inoculation was again employed, and the occurrence of cases completely ceased. Netter (*Bull. de l'acad. de med. Paris, March, 1902*), principal health officer for Paris, reports the following instance:—In the presence of an epidemic of diphtheria in Paris among 491 children exposed to infection who did not receive protective injections, 88 contracted the disease; while

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of 502 children, who had been similarly exposed and were given protective injections, only 13 became infected. Of these 7 developed the disease within 24 hours, and 6 more than a month after the injections. Since the immunity does not begin for 24 hours and passes off in approximately three weeks, these figures strongly support the prophylactic value of the remedy. The preventative action of antitoxin is extensively taken advantage of in America. Biggs (New York Med. Journ., 1900, lxxi., p. 234) states that out of 3,109 children to whom antitoxin was administered only 9 acquired diphtheria; and Park (Journ. Amer. Med. Assoc., 1900, i., p. 902) records 6,506 cases of immunisation, among which 28 developed the disease within 24 hours, that is before the protection was effective, while only 274 were attacked after this limit of time.

11740. You now come to tetanus antitoxin as a curative agent?—It is possible to obtain tetanus antitoxin of extraordinary preventive power, as tested experimentally by injecting the serum previously to the toxin. The experimental results are satisfactory when the two are mixed together at the time of injection; but as a curative agent, in practice its value is far less than that of the corresponding antitoxin for diphtheria, because the disease is usually only discovered after serious lesions of the nervous system have already been produced, upon which, unfortunately, the antitoxin can exert little influence. The injection of antitoxin can neutralise toxin circulating in the blood, and combine with any toxin which may be formed after its injection, but it cannot restore damage done to the nervous elements, although it can prevent nervous elements from being attacked. Koehler (*Münchener Med. 12. Wochschr.*, 1898), dealing with the statistics of ninety-six cases treated by antitoxin, found the death-rate to be 30.4 per cent., which is certainly much below the normal death-rate, viz., 86 per cent. On the other hand, Steuer (*Centralbl. f. d. Grenzgebiete d. Chir. u. Med.*, 1900), from his statistics, could demonstrate no improvement over cases treated without antitoxin.

11741. (Sir William Collins.) One observer showed a reduction of 50 per cent., and the other showed none?—That is so.

11742. (Chairman.) Have you any suggestion as to how that could arise?—Different dosage possibly. Unfortunately the damage is done before you know that the person has got tetanus.

11743. You mean it has got too late?—Yes.

11744. Then it may be from that or it may be from the proper dose not being administered?—Yes; it is not very hopeful under any circumstances.

11745. What is not very hopeful?—The antitoxin treatment of a case of tetanus which has developed.

11746. What time after the injury would you say was the time you expect it to develop?—It may be anything from a few days to months; it is not of immediate onset. The most acute cases develop most rapidly.

11747. Do you mean that if a person has a bad fall and scrapes all the skin off his hand in a dirty place it would take weeks to develop?—Yes. I am not sure of the shortest time that has ever been recorded, but certainly it is unusual for the symptoms to occur in less than a week.

11748. (Sir John McFadyean.) The usual period after inoculation is about a week, is it not, in animals?—Yes.

11749. (Chairman.) Then what have you to tell us with regard to the prophylactic action of the tetanus antitoxin?—Whatever doubts may be entertained of the value of this antitoxin as a curative agent—as a prophylactic its power is fully established. In veterinary practice, where it has been used under test conditions, it has proved infallible. The bacillus of tetanus is common in dung, and hence on the skin of horses. A considerable mortality from tetanus occurs in veterinary surgery, owing to the entrance of the bacillus into wounds, especially in castrations, which from the situation of the wound are particularly liable to be contaminated. Nocard has collected the results of the experience of prophylactic injections gained by 63 veterinary surgeons during a period of 22 months.

11750. Does that mean that the tetanus bacillus may be swallowed with impunity? You say it is common in dung. I suppose it has been swallowed by the

horse?—Yes, swallowed in the hay and food, and it usually lives quite comfortably in its intestine.

11751. And it is not injurious in passing through the human body?—No, not unless it gets in through any abrasion. Certain cases apparently arise in that way from abrasions in the alimentary canal of horses. 2,700 cases, both operative and traumatic, were selected for preventive inoculation from those in which by experience they knew that tetanus was most likely to occur. Not one of these 2,700 animals contracted tetanus; whereas, in the practice of the same veterinary surgeons, during the same period, but not so treated, 259 cases of tetanus occurred (Nocard, *Comptes rendus du Congrès de Moscou*, 1897, t. vi., p. 244).

11752. (Sir William Collins.) Perhaps you will add the reference in cases where you do not give the authority. In some cases you do, and in some you do not?—I will, where I can do so. I can nearly always give the reference to the original, but sometimes I have to depend upon the collected statistics. The originals would be very troublesome.

11753. Then in that case perhaps you will give the reference to the collected?—Yes.

11754. (Sir Mackenzie Chalmers.) And the collectors?—Yes. In case of severe wounds in man, such as compound fractures, where there has been much soiling of the wound, and especially if earth has found its way into the depths of the wound, a prophylactic injection of tetanus antitoxin is advantageously employed. Herhold (*Deutsch Med. Woch.*, 1901, page 479), who was surgeon to the German contingent on the expedition to Peking, at first lost several patients by tetanus. Afterwards he used prophylactic injections in all cases of crushed wounds, contaminated with dirt, and no more cases of the disease occurred.

11755. (Chairman.) Then you mention dysentery antitoxin. I do not think we have heard of dysentery antitoxin before?—Epidemic dysentery is an infectious disease, which is more particularly liable to assume serious proportions when individuals are crowded together in gaols, asylums, or camps. It is in war especially that dysentery becomes a formidable disease, both on account of the high death rate and of the number of men thrown out of action, often for prolonged periods. Knaak (*Die Krankheiten, in Kriege*, Leipzig, 1900) states that in the American War of Secession, 1861-1864, there occurred 259,071 cases of acute dysentery, with 5,576 deaths; and of chronic dysentery 28,451 cases, with 3,855 deaths. In the Franco-Prussian War of 1870-1871 there were 38,652 cases (49 per thousand) and of these 2,380 died—22 per cent. of those affected. In addition there were 623 cases among officers, etc., with 25 deaths. The bacillus of bacterial dysentery was first isolated by Shiga, in Japan (*centralb. f. Bakt.*, 1898). Kruse found the same bacillus in an outbreak of dysentery in South Germany (*Deutsch Med. Woch.*, 1900-1901), and Flexner in Manila (*Centralb. f. Bakt.*, 1901). The bacillus is pathogenic for the guinea-pig and rabbit. In these animals the bacillus itself, or the poison obtained by growing it in broth, produces dysenteric diarrhoea, severe nervous symptoms, paralysis, and death within a few hours or days, according to dose. In the intestines of these animals morbid changes, similar to those occurring in man, have been observed.

11756. Then as to serum therapy in this connection?—Shiga (*Deutsch Med. Wochschr.*, 1901) first immunised horses against the dysentery bacillus, and employed the serum as a therapeutic agent in man. Kruse (*Deutsch Med. Wochschr.*, 1903, p. 49) obtained a similar serum by using the bacillus isolated by him in Germany, and Rosenthal (*Centralbl. f. Bakt.*, 1904, p. 463), Todd (*Brit. Med. Journ.*, 1903, *J. of Hygiene*, 1904, p. 480), and others have added to the knowledge of the subject.

11757. What are the properties of the serum?—Anti-dysentery serum possesses preventive and curative properties when employed on animals, inoculated either with the living bacillus or its toxins. All the pathogenic effects referred to as occurring in experimental animals can be prevented or cured.

11758. Have any results been obtained in the case of man?—No extensive data as to the value of anti-dysentery serum as a therapeutic agent in man are as yet available, but so far as it has been used the results recorded have been of a very promising character. Shiga (*loc. cit. supra*) used his serum on 298

cases, and obtained a reduction in the death rate to one-third of the rate in cases not treated by serum, the death rate in cases receiving only serum treatment was 10.8 per cent., and in 2,599 cases, treated by other means, 35.4 per cent. Kruse treated 100 cases with good results. Of 19 patients under 10 years one died, which is equal to a death rate of 5 per cent., the usual mortality in cases of this age being from 13 per cent. to 20 per cent. The injection of the serum was followed by a rapid reduction in the number of stools, and by the improvement in the general condition. Similar results were obtained at Moscow by Rosenthal (Centralb. f. Bakt., 1904), in 157 cases treated with serum. The death rate was 4.5 per cent., compared with 10.11 per cent. in other hospitals during the same epidemic. When the serum was used in the first three days recovery took place in one or two days. Rosenthal's serum was employed in the Russo-Japanese War. Koreutchewsky (Bull. l' Inst. Pasteur, 1905) treated 70 cases, and found the treatment very efficacious. The painful symptoms were controlled as by a narcotic. Barikan (quoted by Vaillard and Dopler) treated 59 cases in Manchuria, with one death. Vaillard and Dopler (*Ann. l'Inst. Pasteur*, 1906) treated 96 cases, all of a severe type, with one death.

Cases of moderate severity, 50; deaths, 0.

Severe cases, 18; deaths 0.

Grave cases, 24; deaths, 0.

Extremely grave, 4; deaths, 1.

These authors found, like other observers, that after the use of the serum there occurred a diminution in the frequency of the stools. The painful symptoms, such as spasms and colic, were ameliorated.

11759. I gather that this antitoxin has not been tested nearly as much as the previous ones for diphtheria and tetanus?—That is so.

11760. I understand that you consider the antitoxin tetanus is a perfect remedy if applied in the right dose at the right time?—Perhaps I might explain a little more fully. If you injected a person with tetanus, it is my belief that in man, as is the case in animals, if you gave him a dose of tetanus serum the next day he would certainly not get tetanus.

11761. That is what I meant. You look upon it as a certain remedy if applied very promptly?—Yes, if it is applied as a precaution. You cannot tell that a person is going to have tetanus every time he gets a wound and soils his fingers. In most cases he will not.

11762. I quite understand the difficulty. It may not be a case of negligence in not giving it to him. But at the same time, if it were given him you say it would save him?—Certainly, I think so.

11763. With regard to diphtheria, do you say the same thing?—My own belief is that if a child could be treated with the diphtheria antitoxin in an adequate amount and sufficiently early in the disease the mortality would be nil. The moment you knew that the child had diphtheria, if you could treat him with antitoxin the mortality would be nil. That opinion has also been stated by the Medical Officer of Health of Chicago from his experience of the disease in that city, where they distribute diphtheria antitoxin free to every doctor, provided that he afterwards fills up a form to say that his patient cannot afford to purchase it.

11764. As to dysentery, you cannot speak with the same confidence, I understand?—No, but from the experiments on animals I regard the dysentery antitoxin as for practical purposes coming after the diphtheria antitoxin. In practical value it will, I think, make a good second to the diphtheria antitoxin.

11765. And you have warning that there is dysentery much more promptly than you have in the seeds of tetanus?—Yes, before you know that a person has got tetanus the damage is unfortunately done.

11766. (Sir William Collins.) Did you say that the mortality in Chicago had been reduced to nil?—No, I said that the opinion which I have just expressed that if you could take a child with diphtheria sufficiently early and adequately treat it with antitoxin, the mortality would be nil, coincides with that of the Medical Officer of Health for Chicago from his experience in that city.

11767. Can you tell us how far that result had been achieved in Chicago?—I can give you the statistics of the general mortality in Chicago. Unfortunately, I have not available the case mortality statistics in Chicago.*

11768. (Sir Mackenzie Chalmers.) May I put it in this way to see if I understand it: According to your belief, if every contact, if every person who comes in contact, and is therefore liable to infection, received a proper dose, you would have no development of diphtheria in contacts?—That is also my belief, but that it not what I was expressing before.

11769. It comes to the same thing, does it not?—Quite so.

11770. (Sir John McFadyean.) What you mean really is, that if, at the moment the first lesions are visible in the throat, the child got 20,000 units, there would be no mortality from diphtheria?—That is so.

11771. (Dr. Wilson.) In spite of the severity of the type?—Yes.

11772. (Sir Mackenzie Chalmers.) What is the incubation period of diphtheria?—About ten days.

11773. (Sir John McFadyean.) I suppose that has been experimentally proved to be not true for the tetanus antitoxin?—Yes.

11774. You quote Nocard's observations here that he collected, but I do not know whether you are acquainted with another set of experiments in which he infected animals deliberately, and then gave them anti-tetanus serum at intervals afterwards, and found practically that no recoveries took place when the first dose of the serum was given on the onset of symptoms?—Yes, I am familiar with those observations.

11775. Is it the fact that all those who were treated before the onset of the symptoms recovered?—I cannot say the whole of them. So far as regards some of those that were treated just before the onset of symptoms, there was I believe a small mortality amongst them.

11776. (Dr. Gaskell.) What do you mean by being treated on the first day. What is the first day of diphtheria?—The first day is the first day that the disease is recognised.

11777. (Sir Mackenzie Chalmers.) By the appearance of the false membrane?—By a combination. I imagine that in most of these cases the first day in the Metropolitan Asylums Board Reports refers to the first day that the disease is recognised by the presence of the bacilli in the throat and by some symptoms of illness, such as sore throat or slight malaise.

11778. (Sir William Collins.) Does not the statement which you have just made to the Commission amount to this: That in your opinion every death from diphtheria is preventible?—Only under ideal circumstances.

11779. (Dr. Wilson.) And by this serum?—By the serum.

11780. (Sir Mackenzie Chalmers.) If every contact was treated you would have no development of the disease in any one?—I believe not; not if it was adequately treated.

11781. (Sir John McFadyean.) I suppose part of the ideal circumstances would be at least a daily inspection of the throat of every young child. I mean in the family?—Yes.

11782. (Dr. Wilson.) But you admit that in a great many healthy persons you find the bacillus?—Yes, occasionally.

11783. How about those?—I do not quite understand your question.

11784. You have admitted that in the throats of apparently healthy persons (they may be in a diphtheria house or not) you do find the bacillus?—Yes.

11785. Would you inject these?—If they are not ill?

11786. If they are not ill?—Certainly—if you want to stop the outbreak.

11787. What about the patients who have had the disease in whose throats the bacillus may be detected

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* The data asked for were already given in answer to Q. 11733 in the table showing the mortality from diphtheria in 8,000 cases treated in Chicago on different dates of the disease, where it may be seen that of 608 cases treated on the first day of the disease the mortality was 32 per cent.—C. J. M.

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months afterwards as you say?—It is no good injecting them.

11788. But you hold that they are in an infectious state?—They are in an infective state; they can infect other people.

11789. And can be kept away from work?—Unfortunately they have to be.

11790. No, I think you would put a stop to the employment of a great many men if that could be done. I think you must go by the physical symptoms and not the bacteriological. If you go in for the bacteriological test out and out you would have to confuse a great many people?—That is so. If you test convalescents and contacts bacteriologically you will find a great many people in apparent health who are carrying about the diphtheria bacilli. That is how the disease is spread.

11791. That is an assumption?—It is my opinion, of course.

11792. (Chairman.) We will go on to the next question. You were going on to speak about vaccines against plague and typhoid fever. What have you to say with regard to them?—The discovery by Pasteur, that animals could be protected against chicken cholera and splenic fever by inoculation with attenuated cultures of the organisms which cause the diseases, and the practical value of this process in counteracting the ravages of these diseases has already been mentioned. The inoculation of mankind with living attenuated cultures, according to the Pasteurian method has been made use of in the case of cholera, but the apprehension that at any moment the virulence of attenuated organisms might possibly become enhanced has prevented the general adoption of this method of protective inoculation in the case of human beings. In 1888 Gamaleia (*Ann. l'Inst. Pasteur*, 1888, 1889) found that guinea pigs and pigeons could be protected against infection with an organism (vibrio metschnikovi) by the previous inoculation of killed cultures of this vibrio. Gamaleia had found this microbe to be responsible for a disease of poultry in Odessa. It also kills guinea pigs and pigeons. This was an important discovery, as it showed that to produce protection it was not essential to infect with the living microbe, but that some degree of immunity could be induced by the inoculation of the chemical products contained in the substance of the dead organisms. Subsequently the principle discovered by Gamaleia was by experiments on animals, extended to other diseases, e.g., cholera, typhoid (Brieger and Wasserman, *Zeitschr. f. Hyg. Bd. 12, and Deutsche Med. Woch.*, 1892; Pfeiffer, *Zeitschr. f. Hygiene, Bd. 21, 1896*) and plague (Yersin). In 1896 plague broke out in Bombay and spread throughout the city, and extended to neighbouring places. The general sanitary measures employed proved powerless to prevent the spread of the disease. Haffkine therefore turned his attention to the problem of preventive inoculation against plague in the hope of arriving at a useful method which could be applied upon a large scale to man. After numerous experiments with cultures of the plague bacillus which had been killed by a variety of means, Haffkine decided upon a broth culture of the organism sterilised by heating to 65 deg. C. Having tested the vaccine by innoculating first himself and then the personnel of his laboratory, the first extensive trial of the efficacy of this vaccine was made in January, 1897, upon the inmates of the Byculla Jail, where plague had broken out (*Statistics of inoculations with Haffkine's antiplague vaccine, 1897-1900. Bannerman—Gov. Central Press Bombay, 1900*). The submission to inoculation was voluntary. The jail con-

tained 337 adults of both sexes, who were not obviously infected with plague. 154 offered themselves for inoculation. Three of these were, however, in the incubation stage of plague, for they developed plague the same evening. For the purpose of arriving at an estimate of the value of the preventive inoculation these persons in the incubating stage of the disease must be eliminated from the calculation, as also many cases amongst the uninoculated, who also developed plague upon the day the injections were made; leaving 177 uninoculated and 151 inoculated. The result of this experiment was as follows:—

	Number of Persons.	Cases of Plague.	Deaths.
Uninoculated -	177	12	6
Inoculated - -	151	2	0

In December, 1897, an outbreak of plague occurred in another jail in Bombay (Umkardi) thus affording a further opportunity of testing the efficacy of Haffkine's vaccine, under conditions as favourable for the formation of a judgment as it is possible to obtain in connection with human diseases.

	Number of Persons.	Cases of Plague.	Deaths.
Uninoculated -	127	10	6
Inoculated - -	147	3	0

Another opportunity for testing the efficiency of Haffkine's vaccine occurred in the village of Undhera. The whole population of this village was assembled in family groups, and the intention was to inoculate every alternate person. The total number of inoculated persons was 513, and uninoculated 437. The larger number of inoculated was owing to the insistence of some individuals to participate in the possible advantage of inoculation. The epidemic lasted 42 days, and the result was as follows:—

	Number of Persons.	Deaths.
Uninoculated - - - -	437	26
Inoculated - - - -	513	3

On the strength of the favourable results of the above experiments Haffkine's method of inoculation was officially adopted by the Government of India, and the arrangements made for the manufacture of vaccine on a large scale. During the plague season 1902-3, 505,398 persons were inoculated in the Punjab alone. (Report on plague and inoculation in the Punjab from October 1st, 1902, to September 30th, 1903.) The official report gives details concerning the villages of 12 districts in which the proportion of inoculated persons was greatest. The number of inoculated in these 12 districts was 224,428, and the uninoculated, as estimated from the total population, 639,600. The number of cases and deaths from plague were as follows:—

	Number of Persons.	Cases.	Deaths.	Cases per cent.	Case Mortality per cent.
Inoculated - - - -	224,428	3,393	814	1.5	23.9
Uninoculated - - - -	639,600	49,430	29,733	7.7	60.1

In the case of the district of Amritsar (Browning-Smith. Report on plague and inoculations in the Amritsar district, 1902-3, "Indian Medical Gazette," 1904) the inoculations and subsequent observations

were all made by one individual, and as these show evidence of having been conducted with particular care, they are worth quoting.

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	Number of Persons.	Cases.	Deaths.	Cases per cent.	Case Mortality per cent.
Inoculated	30,839	572	150	1.21	26.2
Uninoculated	130,927	11,233	7,888	8.58	70.2

The value of inoculation against plague is undoubted. According to the less favourable of the above figures the chances of a subsequent infection are reduced to one-fifth by this means, and the chances of recovery are two and a half times as great as in the case of the uninoculated. In other words, the mortality has been reduced to one-twelfth by inoculation.

11793. You now come to anti-typhoid vaccination?—Typhoid fever is a disease to which our soldiers are particularly liable, especially those stationed in warm climates. In India the loss to the British Army from deaths alone amounts to half a battalion a year, apart from the pecuniary loss to the State through sickness and invaliding. In war the loss is much more serious; more men being often placed *hors de combat* through this cause than by the bullets of the enemy. Consequently any practicable means whereby the incidence of typhoid fever amongst troops can be reduced is of the utmost importance to the British Army. The subcutaneous injection of minute quantities of killed typhoid bacilli in animals—guinea-pigs or rabbits—protects them against a subsequent fatal dose of the living bacilli. The immunity is greater when three or four such injections, separated by an interval of 10 to 14 days, are made, than with a single injection. A method of preparing a vaccine against typhoid fever

was devised by Wright and Semple in 1896. They satisfied themselves of the innocuousness of their vaccine by experiments—first upon themselves, and subsequently upon their friends. The first trial of protective inoculation against typhoid fever was made in October, 1897, at the Kent County Asylum, where there was an outbreak of the disease. (The figures quoted in par. 11793 to 11799 are taken from "The Practitioner," March, 1904.) The treatment was offered to the nurses and attendants of the Asylum, and 84 out of 200 accepted the offer. Four cases of typhoid occurred in the uninoculated, but none among the inoculated. The figures were inadequate to enable one to form any opinion regarding the value of inoculation, but were sufficient to show the innocence of the method, and to justify more extended trials. The first anti-typhoid inoculations on a large scale were made amongst British troops in India (Bangalore, Rawul Pindi, Lucknow), when the Plague Commission, of which Professor Wright was a member, was in India, November, 1898, to March, 1899. These inoculations were voluntary, at private cost, and without official sanction; though the original proposal for them, in 1897, had come from the Indian Government. The results of these early Indian inoculations were as follows:—

Number under Observation.	Cases.	Deaths.	Percentage of Cases.	Percentage of Deaths.
Inoculated 2,835	27	5	0.95	0.2
Uninoculated 8,460	213	23	2.5	0.34

On 25th May, 1899, the Indian Government made application to the Secretary of State for India that the inoculations should be sanctioned, and should be made at the public cost. The application contained the following:—"The anti-typhoid inoculations have been, we believe, on a sufficiently large scale to show the actual value of the treatment, while the results appear to afford satisfactory proof that the inoculations, when properly carried out, afford an immunity equal to or greater than that obtained by a person who has undergone an attack of the disease; further, the operation is one which does not cause any risk to health."

11794. (Sir William Collins.) You do not give the case mortality in those Indian cases. I see in the

inoculated there were 27 cases with 5 deaths, that is about 1 in 5. Then in the uninoculated 213 cases and 23 deaths; that is about 1 in 9, is it not?—Yes; I have not put the case mortality down.

11795. You have given it in so many figures, that I thought you might have added it there?—I have not done the arithmetic, you mean. It shall be done. Sanction for anti-typhoid vaccination having been given, a number of men from several regiments were inoculated. The regiment in which the largest number of men were inoculated was the 15th Hussars, which remained in India. The incidence of typhoid in this regiment from the 22nd October, 1899, to 22nd October, 1900, is given below:—

	Strength.	Inoculated.	Cases.	Deaths.	Not Inoculated.	Cases.	Deaths.
Officers	22	19	0	0	3	0	0
N.C.O. and men	481	317	2	1	164	11	6
Women	36	24	0	0	12	0	0

From which it is seen that the incidence of typhoid amongst the uninoculated was 5.8 per cent., and amongst the inoculated 0.55 per cent., or less than one-tenth.

11796. Where is the reference to that published? Is it in "The Health of the Army" Annual Reports?—Those statistics were published in the medical papers, "Lancet" or "British Medical Journal," and were collected together in "The Practitioner," March, 1904.

11797. Would they be in some official publications?—I do not know whether they were in an official publication. The Army Medical Department supplied Wright, while typhoid inoculation was going on with the figures, which he published from time to time in the "Lancet," I think.

11798. I do not think they appear in the annual Blue Book, entitled "The Health of the Army," do they?—I do not think so.

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11799. (*Chairman.*) You proceed to give the results of anti-typhoid inoculation amongst British troops during the South African War?—The statistics concerning the campaign in South Africa, which have been collected together by the Army Medical Department,

indicate a substantial diminution both of the incidence and deaths from typhoid fever amongst the inoculated. The following instances are taken from these reports:—

GARRISON OF LADYSMITH.

—	Number.	Cases.	Deaths.	Per cent. Incidence.	Case Mortality per cent.
Uninoculated - - - -	10,529	1,489	329	14.14	22.1
Inoculated - - - -	1,705	35	8	2.05	20.3

MANCHESTER REGIMENT, WINBURG.

—	Number.	Cases.	Deaths.	Per cent. Incidence.	Case Mortality per cent.
Uninoculated - - - -	547	23	7	4.2	30.4
Inoculated - - - -	200	3	0	1.5	0.0

7TH HUSSARS, SOUTH AFRICA.

—	Number.	Cases.	Deaths.	Per cent. Incidence.	Case Mortality per cent.
Uninoculated - - - -	244	20	3	8.2	15.0
Inoculated - - - -	307	7	0	2.9	0.0

Those are the extracted ones. There are a number of other figures collected. I chose those which seemed to give the clearest idea and were best capable of comparison between inoculated and uninoculated.

11800. (*Sir William Collins.*) Where are they published?—They are all published in "The Lancet."

11801. Is there no official publication of them?—I think not at present.

11802. (*Chairman.*) Is not the Army Medical Department an official body; do they not officially publish it?—I am sure that these figures have not been officially published; because when that Committee of Inquiry into Anti-Typhoid Inoculation was called together, we were supplied with a copy of "The Practitioner," March 1904, in which Wright had collected all these cases, and which were certified to by the statistical officer to the Army Medical Department as being correct. If they had had an official publication containing these figures they would obviously have placed it before us.

11803. At any rate, you have described what the source of them is?—Yes. More recently a carefully conducted test has been carried out under the auspices of the Anti-Typhoid Inoculation Committee, appointed by the Secretary of State for War. (An account of this outbreak was published in the *Journal of the R.A.M.C.*, May, 1907.) In August and September, 1905, a proportion of the 17th Lancers, whose total strength was 490 men and 24 officers and wives, were inoculated with anti-typhoid vaccine. This regiment embarked for India on September 6th, and proceeded to Meerut, where they went into the camp. The number of inoculated was 150, 127 of whom were inoculated on two occasions; 14 days elapsed between the two inoculations. In October, whilst the regiment was still in camp, enteric fever broke out, and 46 cases occurred. Of these 45 were amongst the uninoculated and 1 amongst the inoculated. The men, both inoculated and uninoculated, were all mixed up together in the camp and exposed to the same chances of infection. The value of anti-typhoid inoculation can no longer be regarded as doubtful, and considerable effort is being made by the Commander-in-Chief in India to secure the inoculation of as large a proportion as possible of the British Army in India.

11804. (*Dr. Wilson.*) That is not Wright's antitoxin.—It is essentially the same. The dose is different; it is much smaller. On experience it was found advisable to diminish the dose; it was as effective, and gave rise to less symptoms of soreness.

11805. (*Sir William Collins.*) Has that Committee reported?—No, it has made an interim report only.

11806. (*Chairman.*) Are these statistics taken from the interim report?—These figures of the 17th Lancers have been published in the *Royal Army Medical Corps Journal*.

11807. (*Sir William Collins.*) Have they been published in any Parliamentary Paper?—I do not think so.

11808. Not the interim report?—What became of the interim report I do not know. It went to the Army Council. Whether they presented it as a Parliamentary Paper, I do not know.

11809. Is the final report ready?—No; the investigation is still going on. This was an attempt to get an experiment that should be carefully controlled; about half the persons were inoculated, and about half uninoculated, and a special medical officer was sent out to watch them and to diagnose every case, so as to make sure that it was really typhoid. We had done that in the previous years, but we cannot ensure that the draft will go to a neighbourhood where there is any typhoid or any opportunities of contracting typhoid; so that, having made preparation for a test of this kind, one is never sure whether there will be any opportunity for its being carried out. In the case of the 17th Lancers they went straight away to Meerut, and encountered an epidemic of typhoid.

11810. (*Sir John McFadyean.*) I should like to ask whether, in view of the clearness of that result, you think it will be a moral proceeding to repeat that—that is to say, for the whole of the men to be sent out to have the advantage to be derived from this vaccination?—The morality question rests with the War Office. Our reference is to obtain information.

11811. (*Dr. Wilson.*) Vaccination is voluntary, of course?—Yes.

11812. Is there any inducement given to the men?—No.

11813. (*Chairman.*) You said as regards this anti-typhoid inoculation that, so far as the experiments go, you think they show very valuable results?—Yes, I think so.

11814. (*Sir William Collins.*) Has any order been issued by the Commander-in-Chief in India with reference to this?—He issued instructions that he would like as many men vaccinated as possible. There has been no order to say that men must be vaccinated. I doubt whether that is in his power, or whether it would be discreet for him to do so, even if he wanted to.

11815. But an order has been issued recently?—I can only tell you that a telegram came home from the Commander-in-Chief, saying that he would wish as many men coming out to India to be inoculated as possible; but that order did not come to the Committee.

11816. That is what you mean by a considerable effort being made by the Commander-in-Chief in India to secure the inoculation of as large a proportion as possible of the British Army in India?—Yes.

11817. (*Chairman.*) Now, you come to the importance of animal experiments in providing a pure food and water supply for the community. Those experiments for that purpose are conducted by various public bodies, are they not?—Yes.

11818. A certain number of public bodies all over the country?—Yes, public bodies are the great vivisectors in this country.

11819. I do not know that that would be accepted exactly as a doctrine by everybody—that they are the great vivisectors, but they are the great persons for making experiments on animals?—Yes.

11820. And you consider, I understand, that these experiments are necessary and of the highest value to the community?—Yes. Bacteriological methods are now largely employed as an aid in securing for the community a pure food and water supply. Animal experiments are necessary in many of these examinations, and many others would be impossible, but for knowledge previously acquired from experiments on animals. These methods are used not only to trace the origin and spread of outbreaks of disease due to contaminated food and water which may occur, but are constantly employed by municipal and other health authorities to test the purity of meat, milk, and water which is supplied to the public. To begin with milk, cheese and butter. In the examination of these products for tubercle bacilli, diphtheria bacilli, the Malta fever coccus, typhoid bacilli and allied organisms, animal experiments are had recourse to. The testing of dairy cattle with tuberculin may also be cited as a method utilised to secure a milk supply free from the infection of tuberculosis. In the case of meat, animal experiments are employed to discover the presence of anthrax bacilli and tubercle bacilli. In tinned meats and sausage poisoning the recognition that outbreaks of food poisoning (so-called ptomaine poisoning) are due to the bacillus enteritidis of Gaertner, and the bacillus botulismus, Ermengen, has been of use in indicating preventible dangers in connection with the consumption of certain foodstuffs, particularly those arising from the utilisation of infected meat for the manufacture of pies and sausages and for canning purposes. This knowledge depends entirely upon animal experiments. Since the causation of these outbreaks of food poisoning was understood sanitary inspectors have been alert to obviate this danger, and suspected carcasses or preserved foodstuffs are submitted to bacteriological examination, and, if necessary, condemned. In the case of trichinosis, the relationship of this disease to the presence of the parasite trichina spiralis in pork and hams, and the consequent necessity for the examination of the carcasses of pigs for the presence of the parasite was directly due to animal experiment. Water is examined for typhoid, anthrax and cholera bacilli and other organisms indicating pollution, such as bacillus coli communis, the recognition of all of which is either directly due to or based on animal experiments.

11821. Then you state the reasons why the varieties of animals employed in scientific experiments should not be restricted?—The following are, in my opinion, very strong reasons why the variety of animals employed for scientific experiments should not be restricted, and particularly why dogs should not be excluded from use for this purpose. (I.) Diseases peculiar to these animals, such as distemper and

epidemic jaundice (piroplasmosis) require investigation both in the interest of possessors of these animals and of dogs themselves. (II.) One case at least is known, and this is a very important one, in which dog and man harbour two stages of the same parasite echinococcus or hydatids. This parasite is not responsible for any great amount of damage in Great Britain, but in some pastoral countries, notably Australia and Iceland, the proportion of the rural population which becomes infected with hydatid cysts is very considerable. In Iceland it has been estimated by the chief medical officer that one-seventh of the entire population were affected with echinococcus. It is also responsible for a similar cystic disease in sheep. In Australia the disease is very prevalent amongst the pastoral population, and I do not think I am exaggerating when I say that in at least 1-20th of the cases operated upon for tumours a hydatid cyst is discovered. The life history of the echinococcus occurs in two stages. One stage is a cyst which occurs in man, sheep or ox, and occupies almost any situation in the body, but is more commonly found in the liver, lungs, brain or bones. This cyst often reaches the size of a man's head. The other stage is a small tape-worm a quarter of an inch in length, which inhabits the intestine of the dog. This small worm is the sexual stage, and produces quantities of microscopic eggs. These are swallowed by man or beast in drinking water which has been contaminated with the excreta of infected dogs. The eggs hatch in the alimentary canal of man, ox or sheep, and the embryo emerges and finds its way to various situations where it forms a cyst. The causal relationship of a minute worm in the intestine of the dog to a gelatinous cyst containing a gallon of fluid in the chest of a man, could, in this case, only have been ascertained by experiments upon the dog. (III.) Diseases common to man and dog have been fruitfully studied upon the latter, e.g., rabies and ankylostoma. (a.) Pasteur's investigations upon rabies which led to the discovery of a means of preventing the occurrence of this disease in persons infected by mad dogs, were entirely conducted upon these animals, and could not have been arrived at otherwise, for the dog was the only other available animal in which the symptoms and clinical course of the disease were at that time recognised. (b.) Ankylostoma or miner's worm is a disease seriously affecting the health of miners in many parts of Europe and Great Britain. It is due to multitudes of small parasites which attach themselves to the intestinal wall and suck the blood of their host. Until recently the way in which man became infected with these parasites was not understood. As the worms are found in the small intestine it was naturally presumed that the eggs were in some way introduced by the mouth, either in drinking water or food. All attempts to stay the extension of this disease by attention to the food and water were, however, without avail. In 1904 Loos (*Brit. Med. Assoc.*, 1904, *Mining J.*, Aug. 6, 1904), of Cairo, succeeded in demonstrating the precise channel through which these parasites affect an entrance into the human body, and succeed in establishing themselves in the alimentary canal. These experiments were made upon dogs. The ova of ankylostoma pass out in the dejecta of infected individuals. The ova hatch in the fæces and active small embryonic worms emerge. These, when placed on the skin of a dog were found to burrow through the epidermis, and were tracked into the small veins and lymphatics of the neighbourhood. Through either course they were carried by the blood stream to the venous side of the heart, and thence into the capillaries of the lungs, where they became impacted. Then they forced their way into the air cells of the lungs, and upwards into the small air-tubes, giving rise at the same time to a certain amount of catarrhal exudation. This exudation being swept upwards by the ciliary movement of the cells lining the air-tubes, the embryos arrived, after this complicated journey in the mouth of the animal and were swallowed. This demonstration of Loos (*Centrabl. f. Bakt.*, Vol. 33, 1903, p. 339) of infection through the skin was subsequently confirmed upon man by Calmote (*l'Ankylostomiasse (Anémie des Mineurs) Biologie, clinique, traitement*, &c., Paris, 1905) in France, and Boycott (*J. of Hygiene*, 1905) in this country.

11822. In the course of that journey through the man, or through the animal, do they do no moral harm to them?—No, no appreciable harm.

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11823. Then they got into the mouth, and, I suppose, passed out in the fæces ultimately?—Then they go into the intestine when swallowed, and then they develop into full-grown worms, which suck blood.

11824. And then they become mischievous?—And then they become mischievous. The dog and certain species of monkey are the only animals known to be capable of occupying the position of host to the human ankylostoma. The monkeys not being available, these facts could not have been ascertained without experiments on dogs. (IV.) A much more weighty objection is that physiology in Great Britain would be very seriously handicapped if, for any reason, dogs were not available for animal experiments. This would be a calamity, for pathology and medicine are founded upon experimental physiology, and any hindrance to the development of physiological knowledge must produce a corresponding diminution in the rate of advance of these medical sciences. I cannot better express my opinion on this subject than by quoting from the evidence given by Charles Darwin before the last Royal Commission on Vivisection: "I am convinced that to hinder the progress of physiology would be to commit a crime against humanity." I realise that physiological knowledge would not cease altogether to progress because an impediment were placed upon its development in this country, but am sufficiently patriotic to wish that my countrymen should maintain that honourable place as workers in this field of science which they at present possess. The physiologist must have access to animals of varying size. For some problems experiments upon small animals are more suitable, but many important functions of the body can only be investigated upon an animal large enough to permit of the accomplishment of the necessary experimental manipulations—e.g., the physiology of the circulation, respiration, and nervous control of the same. Physiological facts derived from experiments upon animals can only be applied to man when the mechanisms in the two cases present close similarity. In the case of investigations upon the digestive system it is necessary that experiments be performed upon an omnivorous animal. The dog, as domesticated, is omnivorous, and can be obtained of sizes varying within a considerable range, and is, therefore, well adapted for such investigations. As well as possessing the advantages above indicated, an experimental animal must be accessible to the physiologist from the point of view of price. Monkeys and apes can, on this account, only be employed for special researches, such as those upon the nervous system, where their much greater similarity to man in regard to nervous organisation renders them preferable. Owing to the fact that dogs are not utilised as food in this part of the world there is, in our large towns, a continual supply of valueless and homeless dogs, which can be procured at a moderate price. Lastly, it is desirable that an animal be of such a character that it can be kept in health and looked after in the adjuncts of a physiological laboratory. Physiological laboratories are, as a rule, in cities and attached to universities, and not usually supplied with any stable accommodation.

11825. (Chairman.) Then the last head of your evidence is as to demonstrations upon living animals in teaching. That we have heard a great deal of, and, of course, it is a very important question?—The study of the structure and arrangement of dead organs and tissues, even when coupled with the best of descriptions of the functions of such organs and tissues, is inadequate to supply the student with a true conception of physiological processes. The only method with which I am acquainted whereby one is enabled to explain a fact or process is by reference to other facts or processes which are already part of the experience of the pupil. Physiological phenomena are so peculiar, and often differ so essentially from those which have already become part of the experience of the student during his study of chemistry and physics, that it is impossible to convey more than a distorted conception of most processes occurring in the animal body without some direct appeal to experience. I am confident that no one ever realised many of the most elementary physiological facts without witnessing experiments upon living animals. It is not possible, nor is it necessary, that every experiment which has contributed to present knowledge of physiology should be reproduced for the benefit of the student, for when he has

seen and appreciated some few fundamental experiments it is possible to assist him to an understanding of further physiological phenomena by appealing to his previous experience. The opinion expressed above was forced upon me by my own experience as a student. I first studied physiology at a medical school, where I had no opportunity of witnessing physiological experiments. My conceptions of physiological phenomena were drawn from examination of dead animals and descriptions conveyed by word of mouth and printed books. This means of study energetically pursued enabled me to reproduce descriptions of physiological processes to the satisfaction of my examiners and myself. Subsequently I had the good fortune to study in the Physiological School of the University of Leipzig, and to see experiments, when I found that the conceptions which I had formed of what took place in physiological phenomena were distorted, and bore little resemblance to reality. The class of physiological experiments which it is necessary to show to students involves no suffering, as they are all conducted under anæsthesia. A mongrel which nobody will provide for, and which would otherwise be destroyed by the agency of the police, is drugged to unconsciousness, and his exit from this life is made an opportunity for the conveying of cardinal knowledge of function to the student, which knowledge cannot be attained in any other way. It has been suggested that the witnessing of physiological experiments would have a brutalising effect upon the student. I have never discovered this. When professor at Melbourne I performed from thirty to forty experiments upon living animals each year during my course of lectures. This class included six or eight young ladies. What I did notice, however, was that when confronted with experiments on animals the class became interested, earnest observers, and assumed the attitude of scientific searchers after truth.

11826. (Dr. Wilson.) May I take it that as Director of the Lister Institute you superintend generally the bacteriological methods of diagnosing infectious disease and the preparation of the various sera, and so on?—I have merely a general administrative supervision. I have skilled bacteriologists and experts in other departments whose special duties are of a particular nature.

11827. May I assume that the experiments on animals entailed in these methods of research and for the preparation of these preventive or curative substances are almost exclusively confined to inoculations or injections?—That is so in the Lister Institute—almost exclusively.

11828. Whenever diseased conditions are produced in animals by these experiments, do you concede that the element of pain can never be eliminated altogether?—I imagine that it is not entirely eliminated, either the element of pain or the want of feeling of well being.

11829. Discomfort?—I do not know whether it is. I certainly think that in some cases there is discomfort.

11830. And in some cases where death ensues after injections there might be a good deal of pain, of course?—In some cases there may be.

11831. Would you kindly tell me the names of the substances which you prepare as toxins and sera in the Lister Institute?—Do you mean for commercial purposes?

11832. For commercial purposes?—There is diphtheria antitoxin, tetanus antitoxin, dysentery antitoxin, and Yersin's anti-plague serum.

11833. Do they prepare any vaccine lymph at the Institute?—Yes, and vaccine lymph.

11834. All these are, as you say, prepared for sale?—They are prepared for sale.

11835. Could you give me any idea of the approximate number of doses of these preparations which are sold during the course of a year. I suppose that would not come within your knowledge?—I can ascertain it for you. I could not tell you off-hand.

11836. May I inquire how much one of these large doses of anti-diphtheritic serum costs—how much is charged for it?—These are sold to the public through our wholesale agents at the rate of 2s. 6d. for 2,000 units.

11837. So that for 20,000 units?—It would be ten half-crowns.

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11838. It would be 25s. for a single dose of the anti-diphtheritic serum?—It would be ten half-crowns; that would be 25s. for the amount that is the average dose administered by the Metropolitan Asylums Board people, who have the very greatest experience in this matter.

11839. And in a case of illness of average severity, how many of these doses do you think would be given?—Not more than 20,000 units altogether.

11840. During the whole course of treatment?—Yes, certainly.

11841. What would be the initial dose then?—It might be given in three or four doses within the first 24 hours; that is the usual practice, I believe.

11842. But is it not the fact that serum is given for several days in some cases?—It may be; but that is not the best way to give serum. The best way to give serum is to get a much in as possible as soon as possible.

11843. So that it may be taken as an estimate that for an average case of diphtheria about 30s. would have to be expended in treatment with this serum?—I think so.

11844. I hope you will not consider this an invidious question, but may I inquire whether you yourself and the whole of the expert staff are paid by fixed salaries, or whether the salaries depend in any way upon the amount of these products which are sold?—No, the salaries are fixed, fortunately; because the results from the sales of these products are not very remunerative, and under any circumstances it would in no way affect the salaries of the staff. As a matter of fact they do not quite pay expenses.

11845. Although I have no doubt that the horses on the Institute farm, which are used for the production of serum, are carefully vetted as to soundness, is it not the fact that these animals do not come under the Vivisection Act at all?—They do not come under the Vivisection Act as long as we are not trying to find out anything new.

11846. Nor, of course, do any of the horses used by the large manufacturing chemists for the production of sera?—Provided they are not attempting anything in the nature of an experiment. If they are only manufacturing serum for commercial purposes they are, I understand, without the scope of the Act.

11847. But as they have to be frequently injected and repeatedly bled, and the greatest care, of course, must be taken as regards their condition of health, do not you think that they should come under Government supervision?—I see no objection at all. It seems rather an anomaly that you should be able to bleed a horse for commercial purposes without supervision and not for experimental purposes.

11848. And that would apply to the calves you use for calf lymph?—Yes, for vaccine. I gather that it was merely a question of expediency in framing the Act—the difficulty to draw the line.

11849. In the production of sera in the animals themselves do not you think that, as much depends upon the purity and standardisation of all these various sera and vaccines, all establishments of the kind should be more or less under Government control?—There is no control of it in this country. In most foreign countries, there is a Government control and an establishment for standardising such preparations. But that is apparently not the way we proceed in this country.

11850. Before dealing with the results gained from all these newer methods of prevention and cure, may I ask whether you conceive that in this country, at all events, many endemic or epidemic diseases have either been banished altogether or kept under control without having recourse to experiments on animals?—I cannot remember any just now. I think it is possible.

11851. Plague, for example—we have heard so much about that?—Yes, plague disappeared from this country for reasons we do not understand in the least.

11852. We got rid of it without any recourse to experiments on animals?—Yes.

11853. And was not leprosy also common in this country?—Yes.

11854. And coming to more recent times, has not cholera been practically banished?—Yes. I do not

know that cholera was ever very common in this country.

11855. There have been several epidemics about half a century or more ago. And typhus and relapsing fever have been practically banished from this country, with the exception of a few outbreaks in poor and dirty courts?—Yes.

11856. And even typhoid fever has been vastly reduced, has it not, both as regards the mortality and prevalence?—Yes. I am not prepared to say how far the knowledge obtained by experiments on animals has not contributed to that reduction in the case of typhoid fever.

11857. But I am talking of prevalence before that time?—Yes, the prevalence had diminished before. I admit that.

11858. And even with regard to phthisis, was not the mortality from that disease reduced by half, at least, before Koch discovered his bacillus?—Yes, that is so.

11859. (Dr. Wilson.) And now I come to a disease to which I do not find any reference in your admirable and compendious *précis*—I mean small-pox. I presume you do not question the value of vaccination as a protective against small-pox?—No.

11860. But Jenner's great discovery that cow-pox was a modified form of small-pox was made through induction, was it not, and not through any experiments upon animals?—I think you will find that he took advantage of the fact that cow-pox existed in milch cows, and was associated with cow-pox in the milkers.

11861. (Sir William Collins.) Can you give the reference to that?—No, I cannot now.

11862. (Chairman.) I thought it was only that they got pimples on their hands?—Anyhow, he took advantage of observations of conditions that were ready made. There were no experiments on animals in that case.

11863. (Dr. Wilson.) Though calves are now almost exclusively used for the production of lymph, vaccination until very recent years was largely carried out from arm to arm, was it not?—I believe so.

11864. And could be so carried out still if all children were believed to be healthy?—Yes.

11865. But did not Pasteur found his methods of protective inoculation on Jenner's discovery, and are not all these newer protective methods to which you refer more or less based on that discovery?—I presume that Jenner's work was quite familiar to Pasteur when he discovered that an old culture of chicken cholera was capable of conferring a certain amount of immunity; but I believe it was not due to Jenner's work that Pasteur discovered that fact. I believe it was purely accidental.

11866. But do not you think he was influenced at all by Jenner's discovery?—I should imagine that he would probably have been influenced in drawing his conclusions by his knowledge of Jenner's discovery.

11867. Is not vaccine lymph, whether it is obtained from the calf by vaccination, or from arm to arm, as in human lymph, believed to be a modified or attenuated form of the virus of small-pox?—Yes.

11868. And is not that the reason why it is used as a protection against small-pox, because it is believed to be a modified or attenuated form of the small-pox virus?—I imagine that is why Jenner first used it.

11869. And is not vaccinia, or the conditions following vaccination, whether in the cow or calf or human being, also believed to be a modified form of small-pox?—Yes.

11870. So that as one attack of small-pox renders a person immune against a subsequent attack, so vaccinia, or the conditions induced by vaccination, more or less effectually protects against small-pox, is not that so?—That is so.

11871. But is not this protective vaccine manufactured, so to speak, entirely within the tissues of the living body, and cannot be cultivated outside the body, whether it be of the calf or the child?—I am afraid I do not quite understand that question.

11872. That the virus of vaccinia, the lymph, cannot be cultivated, as you do other toxins?—That is so; it has not been cultivated.

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11873. It cannot be cultivated outside the living body. Is it not entirely produced in the tissues as in the arm under the skin?—It has not so far been cultivated outside the living body.

11874. In this respect, then, does it not differ vastly from any other vaccines you name, such as tuberculin, mallein, and Haffkine's plague vaccine or Wright's typhoid vaccine?—I will not admit that it differs vastly; it differs to the extent you mentioned—that the one has not been cultivated and the other has.

11875. And all these newer vaccines are prepared, of course by cultivating the so-called specific microbe of the disease on various organic media or substances outside the living body?—Yes.

11876. Is it not the fact that while a calf which has been successfully vaccinated is rendered completely immune against a second vaccination, the serum of successfully-vaccinated calves absolutely fails to confer any immunity at all on other calves?—I think that that is to a certain extent true, but not absolutely true. Experiments have been made showing, first, that the serum of a vaccinated animal will destroy vaccine lymph when mixed with it; and, secondly, that in some cases it has been found difficult or impossible, I believe, to successfully vaccinate an animal that has had several injections of the serum of a calf that has been vaccinated.

11877. But my question was whether it has not been proved by experiments that the serum of a successfully vaccinated calf will not protect another calf against vaccination?—Yes.

11878. Does not this fact alone, which you admit has been established, cast very logical doubts upon the protective value of the various sera you have mentioned?—I do not think so, in the least.

11879. Although it must be admitted that there is a specific virus, as I call it, for all these infectious diseases, it does not follow, does it, that the micro-organism which is characteristic of any particular infectious disease, is the actual *causa causans* of that disease?—I am afraid I do not understand the distinction.

11880. You believe that the microbe is the cause, wherever you find it?—I believe in the case of a large number of infectious diseases, many of which I have mentioned in my evidence, it is undoubted that the microbe is the essential cause of the disease.

11881. Take phthisis, for example. It is now admitted, is it not, that the tubercle bacillus is not found in the early stage of the great majority of cases of the disease?—Is not found where?

11882. In the sputum?—I do not admit it.

11883. Then is not the typhoid bacillus now found in the faeces of healthy persons?—It has been found in the faeces of healthy persons, especially in those who have suffered from typhoid and are known to have suffered from typhoid.

11884. Has it not been recently found in the jejunum, even in cases of typhoid further up the intestine?—I am not aware whether it is ever found in the jejunum or not. You can get it from the small intestine in cases *post-mortem*; but it is very difficult to isolate it.

11885. Has the typhoid bacillus ever been found in polluted air?—I should imagine not.

11886. Or even in polluted water?—It has been found in polluted water, certainly.

11887. But very rarely?—Very rarely, yes.

11888. Then as you admit that typhoid fever cannot be produced by inoculation in animals, how can it be said that the typhoid bacillus is the exciting cause of the disease? You cannot by inoculation produce typhoid fever in animals?—It is true that you cannot completely reproduce the disease by feeding with the bacillus of typhoid. The evidence so far of the typhoid bacillus being the cause of typhoid fever is not absolutely complete; but its association with the disease is so constant, and the fact that it can always be recovered from the organ of patients who have died of the disease (and, indeed, it was in the first instance discovered in the organs and not in the alimentary canal) renders it so extremely probable that it is the cause of the disease as to leave little room for doubt. And, in addition to that, you have now the evidence from agglutination and the bactericidal

action of the sera of patients convalescent from typhoid fever, which is entirely specific for that organism.

11889. But is not this reaction sometimes found in respect to other diseases?—It has never been found in respect to other disease to anything like the same extent or the same dilution as for typhoid.

11890. But it has been found; so it is not an absolute test?—A slight specific reaction has been found in the serum of patients which are not known to have suffered from typhoid fever.

11891. What about para-typhoid?—The serum of para typhoid cases reacts in a high degree to the particular para-typhoid bacillus.

11892. But there is at present a great deal of confusion between the two, is there not?—I think that confusion is being rapidly cleared up by bacteriological investigation.

11893. Now, take the bacillus of diphtheria; you have admitted that that is often found in healthy throats?—Yes.

11894. And is it not often confounded with what are called pseudo bacilli, like Hoffman's bacillus?—I do not think it is often confounded nowadays.

11895. Do you think mistakes in bacteriological examination cannot be so rarely made?—I think they are rarely made nowadays.

11896. If you inject the toxin manufactured or produced from the diphtheria bacillus into a horse, are any of the prominent symptoms of the disease, as seen in the human patient, developed in the horse?—Do you mean in the course of immunisation?

11897. Yes. You inject the cause of the disease, and you believe that you produce the disease in a modified form, do you not? You never get diphtheria; you never get the throat manifestations in the horse?—No, not when you inject it under the skin.

11898. Of course, if you are to inject any stuff into the throat it will produce irritation?—Not any stuff.

11899. I will not say any stuff; I do not mean it in any sense disrespectful to the serum—but any injurious material?—If you inject any irritant into the throat it will produce inflammatory changes.

11900. Can it be said that any of the diseased conditions produced in animals by injections of toxins derived from so-called specific microbes, bear any close resemblance as regards the incubation period, for example, and train of symptoms, as in diseases propagated by infection on the usual natural lines. Do you get the real disease with it?—You get the real disease in a modified form, owing to the fact that you inject the poison all at once instead of having to wait for it gradually to be developed and be absorbed day by day.

11901. But you are never certain that some of these induced diseases have periods of incubation as in human beings. You do not get these illustrated in animals?—If you inject a toxin directly into an animal you cannot expect to have the same period of incubation as when you infect it with a disease.

11902. But if you inject vaccine lymph into the calf you get always the period of development; that is to say, a vaccine lymph vesicle is there on a particular day after?—But I would point out that the two conditions are not analogous. In one case you are injecting a poison, in the other case you are injecting a living organism which has to grow.

11903. But in injecting animals with toxins you do not, of course, inject the organisms?—No.

11904. No bacillus has been discovered yet in small-pox?—No.

11905. Has any so-called specific microbe or micro-organism been discovered, apart from small-pox, for scarlet fever?—No.

11906. Or measles?—Measles is doubtful.

11907. In whooping cough?—There are good bacteriologists who claim to have discovered the organism responsible for whooping cough within recent years.

11908. But there is not general agreement upon it?—It is not established at present.

11909. Nor, of course, in hydrophobia has any specific microbe been discovered?—No.

11910. With regard to hydrophobia, compared with other prophylactics, does it not seem very incongruous

that Pasteur should have selected the spinal cord of an assumed infected rabbit as his vaccine against hydrophobia? That is a very singular instance, is it not, of the substance that is used?—I think I am right in saying that in the first instance Pasteur used the saliva of dogs, and subsequently used the spinal cord; and I think the train of reasoning was quite clear and logical which led him to expect that he would find the virus of hydrophobia within the nervous system.

11911. But as the virus is found in the saliva of dogs, would it not have been just as logical to try to discover it in the salivary glands, for example?—He did.

11912. And failed?—He found it unsatisfactory.

11913. Although the saliva itself contained the virus?—Yes.

11914. I see in your *précis* that you have given statistics to prove the success of the Pasteur treatment. But is it not a fact that all deaths are excluded which either occur during the treatment or within 15 days after it has been completed?—Yes, they are all excluded. May I say why?

11915. Yes; I know why?—I might put why in evidence, perhaps.

11916. Certainly. I was going to ask you has it not also been made clear by statistics that a large number of deaths have occurred after Pasteur's treatment? I cannot say how many just now, but I know that in 1898—that is nearly 10 years ago—there were over 800 fatal cases which have been collected after treatment?—I am afraid I do not quite know where I am. Am I to answer that first question?

11917. Yes, certainly. Give your reason?—The reason why they excluded all cases which occur within 15 days of the termination of the treatment was that Pasteur found that the incubation period after the direct access of virus into the central nervous system of a dog was 15 days, and he did not imagine that his treatment would be of avail when the virus had once reached the central nervous system; consequently, he never expected or claimed that it would be of any value when once that had taken place.

11918. He did claim it in the first instance, did he not? He was surprised that these deaths occurred?—I do not think he ever claimed it. Anyhow, that is why they were excluded. That was what he considered to be the limits of his treatment—that if the virus had once got into the central nervous system he did not expect that he would be able to counteract it.

11919. But was not that explanation given after deaths had occurred during treatment, and within a short period after it?—I think very likely.

11920. But this differs, then, vastly from, for example, the serum treatment in diphtheria, does it not?—It has nothing in common. The principle of that is entirely different. In the serum treatment of diphtheria the serum contains a body which neutralises the toxin of diphtheria—it combines with it chemically, and neutralises it. In the case of hydrophobia it is a vaccination. You are endeavouring to accustom the tissues of the animal to a resistance against the virus of hydrophobia by educating it first of all with a mitigated virus, and gradually increasing the severity of it. It is a process of education of the tissues of the individual. The other is a mere chemical reaction.

11921. There is no certain incubation period for hydrophobia in the human being, is there?—No. The onset of hydrophobia occurs, in the larger proportion of cases, in the second month after the infection; but it may occur in the first month, and it may occur as long as one year afterwards, and I believe cases have been recorded of even longer incubation.

11922. And when injected into animals, what is the incubation period?—It varies with the virulence of the virus injected.

11923. So that you admit, I suppose, that statistics of this description have to be very carefully scrutinised before any such Commission as this?—Statistics of every description have to be very carefully scrutinised, I find.

11924. Of course, as you have said, the disease has been effectively stamped out in this country without having recourse to Pasteur's treatment?—Certainly.

11925. Now, with regard to Haffkine's prophylactic against plague, can it be said that it has had any marked effect in stemming the spread of the disease in India?—I think the figures which I have given show that it has had a marked effect.

11926. But in a very limited number of cases compared with the millions of India?—It has not been attempted in the majority of instances.

11927. Is it, or is it not, within your knowledge that within the last few years an epidemic of plague was stamped out in Egypt without having recourse to Haffkine's vaccine or any other prophylactic?—There have been several small epidemics in Egypt, and whether Haffkine's vaccine was used or not I cannot tell. That is not the only way in which you can operate.

11928. Is not the progressive improvement of sanitary conditions the only effective basis on which prevention of the disease can be carried out?—Provided we agree as to what sanitary conditions mean, yes.

11929. Is it not the fact that before Haffkine's treatment became the vogue the mortality from the disease among the European population in India was comparatively slight?—Do you mean the case mortality or the general mortality?

11930. The general mortality?—The general mortality among the European population in India has always been slight.

11931. So that any statistics applied to Europeans with regard to the effect of Haffkine's vaccine on natives would be rather misleading, would it not? You cannot compare them exactly in the same line?—I do not know that they would be misleading. They would be a small number of figures I take it.

11932. But a vast number of Europeans did go in for it voluntarily; I think Lord Curzon himself, if I remember aright, was vaccinated?—Yes, I think so.

11933. But before treatment was introduced cases of plague were comparatively few amongst Europeans?—Yes.

11934. Now there is another point. Although the isolated groups of statistics that you give appear to show that the vaccine may possess some value, inasmuch as vaccination is voluntary, and not compulsory, would not those vaccinated, believing in the vaccine, possess, as it were, more physical resistance to the disease on account of that belief?—I see no reason to suppose that that would be the case.

11935. You do not believe in faith cure—in faith being an agent?—Yes, I do. I believe in faith being an important agent. But this is not cure; this is a question of incidence.

11936. Would they not be naturally inclined to take other reasonable precautions if they went in for the treatment?—I see no reason to suppose so.

11937. But they volunteer for the vaccination, do they not?—Yes.

11938. Would you not think that they would also be prepared to take other careful precautions?—No. On the contrary, one might imagine that those who had been vaccinated would be the most careless.

11939. But they know that a good many do not escape. That element would not come in. Have not somewhat disappointing results from the use of Haffkine's vaccine induced Dr. Klein to prepare a vaccine of his own?—Dr. Klein has, I believe, prepared a vaccine.

11940. I see that you admit that attempts to produce a serum for the cure of the disease have been disappointing; but is not Yersin's serum largely relied on in the eastern dependencies of France?—It may be so; I believe it is much more used in French territory than it is in other territory.

11941. And has not Lustig's serum also found staunch advocates?—Lustig's serum has been advocated.

11942. Now with regard to Professor Wright's anti-typoid vaccine, was not its success in affording protection to our soldiers during the South African war seriously called in question?—It has been called in question.

11943. Were not the statistics also scrutinised by the well-known statistician, Dr. Karl Pearson?—Yes,

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11944. And characterised by him as somewhat misleading?—I think he disagreed as to the extent to which they justified the conclusions that Wright had drawn from them. It was a question of the extent.

11945. But they appeared to him not to support the contention that it had acted as a protective?—No, if I remember rightly they appeared to him to be less conclusive. He was dissatisfied with the numbers and the discrepancies in certain cases. But I think Karl Pearson was taking a very high standard as to what would be required of a vaccine.

11946. He took vaccination into account in making comparisons, as I have been doing?—Yes.

11947. Is it not the fact that during the Russo-Japanese war there was an entire absence of any prevalence of typhoid fever among the Japanese troops, owing to the careful sanitary and other precautions which were taken, without the aid of any such anti-typhoid inoculation?—I have not seen any account of the amount of typhoid fever in the Russo-Japanese war amongst the Japanese troops; I cannot say.

11948. But it was very trifling, was it not?—I do not know; I have not seen any account of it.

11949. You do not know whether they used any prophylactic or not?—No.

11950. Because you have just said that they do use the anti-dysenteric serum—Shiga's serum?—Yes.

11951. I do not know whether this is within your knowledge. May I ask whether any of this anti-typhoid vaccine was used to stem the spread of typhoid during the Lincoln epidemic some two or three years ago?—Not that I am aware of.

11952. Did not Wright try it in the Maidstone epidemic?—That was the first instance in which it was tried.

11953. Are not these inoculations both with Haffkin's vaccine and with Wright's followed by somewhat painful symptoms?—I think the symptoms are quite trivial.

11954. But at the outset, from the original vaccine?—In the early stage the dosage was considerably greater than is necessary now.

11955. But how do you know about its being greater than necessary? Was not the dose lessened because of the unpleasant symptoms?—No; the dose was lessened on account of the investigations undertaken under the auspices of the Anti-typhoid Inoculation Committee.

11956. By experiments?—By experiments.

11957. Then the original experiments could not have been relied on, and were not so reliable?—It is always possible to improve things as you learn more and more about them. And, in the first instance, as I have pointed out in my evidence, Wright was suddenly confronted with the vaccination of a mass of troops in South Africa, owing to the great prevalence of typhoid fever there, and had to put a process into use which no doubt he would gladly have proceeded with more slowly and improved his methods.

11958. Would not a great deal depend upon the particular strain of bacillus that might be cultivated?—I think not.

11959. Would that apply to any serum that might be produced as regards the strain of bacillus that must be used either for producing the vaccine or the serum. Is it immaterial?—It is generally held that it is more advantageous to use a bacillus of considerable virulence. But those are points which are not definitely determined.

11960. Has not Professor Wright recently made rather a damaging pronouncement concerning the serum treatment?—I should think it is very likely.

11961. Does he not now strongly advocate treatment by vaccine and not serum, as indicated by what he calls the opsonic index?—I believe that is so.

11962. Do you think that this new departure in bacteriological treatment can ever become possible in general medical practice, even if it should be proved to be successful? Could it be possible for medical practitioners, I mean, to take this opsonic index into practice?—I think it would be difficult for busy medical practitioners. But, if it were found essential, it will always be possible to find people capable of doing it.

11963. I come now to tetanus. Is it not the fact that the treatment of tetanus by anti-tetanic serum has fallen into disrepute?—I would hardly agree with you

that it has fallen into disrepute. It has not justified the expectations.

11964. But it was first tried by injecting it under the skin, subcutaneously, was it not?—If I may put it this way: although I consider that you are treating tetanus at a very late stage of the disease by the time you have got symptoms of spasms, it would still be my duty to administer the anti-toxic serum.

11955. Then would you administer it by injecting it under the skin, or under the dura mater after trephining the patient, as has been done in some cases?—I should inject it intravenously. I have done so on more than one occasion.

11966. And not under the skin or under the dura mater?—No.

11967. You referred to puerperal fever. Has not the anti-streptococcus serum treatment for puerperal fever also been more or less a failure?—It has been unsatisfactory, owing to the fact that it is highly specific, and puerperal fever is caused by a great variety of different strains of streptococci. You are never sure that the serum will be made from an animal immunised with an homologous strain.

11968. But were not great expectations held out with regard to it at first?—I cannot help that.

11969. Can it be said that though pneumonia is one of the most prevalent of diseases, the anti-pneumonic serum has in any sense become popular as a remedy?—No, I think not.

11970. Does it not appear somewhat singular that anti-tetanic serum, for example, which was first used as a remedy should recently be almost exclusively used as a prophylactic?—No; it seems to me to be perfectly clear why it should be so.

11971. Why was it not so used in the first instance? They started it as a cure, did they not, and not as a prophylactic?—They did not find out the limitations until they had used it.

11972. That is to say, they did not use it as a prophylactic until they found that it failed as a cure?—No, that is not quite so; because it was used as a prophylactic in animal experiments in the discovery of it, the working out of the process.

11973. Even as a preventive of tetanus in animals, it was not used for some time?—I think it was; I think that was one of its first virtues that was discovered—its prophylactic value.

11974. It was not used as a prophylactic in tetanus at first?—I am not aware whether that was the case in practice or not.

11975. But in spite of the statistics supporting its claims as a preventive, it does not follow, does it, that the great majority of men or animals, in which it is used, would have developed tetanus if it had not been used. You cannot say that?—If you will leave out the first part, I could understand it.

11976. Very well, I will put it in this way: that we will suppose horses or people have abrasions, and that you use this serum for all of them to prevent the disease; it does not follow, does it, that any or many of them would have had the disease if they had not been injected?—It does not follow that every horse that has abrasions will become infected with tetanus. I prefer to put it in that way, if I may.

11977. May I ask whether anti-tetanic serum was used to prevent the disease occurring among the wounded during the South African War?—Yes.

11978. To a large extent?—There were only three cases of tetanus.

11979. But was that due to the use of the serum or to clean surgery?—It was due to clean surgery.

11980. And you cannot say whether the serum was largely used or not?—Whether the serum was used in those cases or not I am unable to say.

11981. But I mean generally amongst the wounded?—I do not think so. There was a large amount of serum taken to the South African War, but I do not suppose it was used. As a matter of fact they had no tetanus except three cases, and it would not have been generally used.

11982. You point out that in the Franco-Prussian War there were 35 per cent. cases amongst the wounded?—Yes.

11983. And that was before the days of clean surgery?—It was before they knew how tetanus was transmitted, or what its causation was.

11984. So that after all is not the chief preventive against tetanus, to ensure that wounds are washed clean and kept as clean as possible?—Yes.

11985. Now I come to the much vexed question of the serum treatment of diphtheria. Of course, the anti-diphtheric serum is believed to be the most successful of all sera in treatment, is it not?—Certainly.

11986. And that is not quite a harmless product, is it?—It is not quite harmless.

11987. It produces rashes in a great many cases?—Yes.

11988. And other disagreeable symptoms?—The most disagreeable symptoms are the rashes. They keep the child awake at night.

11989. Are not medical men encouraged to use it in all suspected cases of throat illness, and not to wait for the results of the bacteriological testing for the bacillus?—I think that is quite a wise procedure.

11990. But if the bacteriological testing fails to confirm the suspicions of the medical attendant and the case does not turn out to be one of diphtheria, would you not call this somewhat haphazard treatment and scarcely fair to the patient?—No. I should consider it had been done in the best interests of the patient, for the reasons given in my evidence as regards the necessity of the earliest possible treatment by all antitoxins.

11991. If it does not turn out to be diphtheria?—No harm is done.

11992. But you admit that it causes rashes?—The patient does not die of the rashes.

11993. How long does it generally take before the medical man receives the bacteriological report, after forwarding a swab from the throat of a suspected case?—Fourteen or fifteen hours.

11994. But supposing you have to send to the country by post?—You can telegraph the result.

11995. But the swab has to be sent?—Say from 14 to 24 hours, in this country.

11996. But as there are several other bacilli closely allied to Loeffler's bacillus, can the bacteriological testing be regarded as absolutely reliable without having recourse to the guinea pig?—Not absolutely; but it is a near approximation to the truth, which is of great value for practical purposes.

11997. How long would it take to complete a test for injecting a guinea pig?—About four days.

11998. In relying upon the bacteriological testing, is not the medical attendant placed in a somewhat invidious position; does not the delay in waiting for the report rather militate against the chances of cure?—I have just previously stated that I thought the injection of the serum in the case of a patient that was suspected to have diphtheria was the best treatment in his interest.

11999. But then that would be dispensing with the testing altogether?—No, because you can inject one dose. The earlier you can treat with antitoxin the less is necessary. You can give a moderate dose of antitoxin and then if the diagnosis is confirmed on the morrow by bacteriological examination, you can give more. If it is not confirmed you would give no more.

12000. But then you must admit that this waiting for the report of the bacteriological testing does militate somewhat against the chances of cure?—I advise not to wait.

12001. Then the testing could be dispensed with?

12002. (*Sir Mackenzie Chalmers.*) Are there not in a great many cases well-known marks which no medical man can mistake?—Yes.

12003. When you see the false membrane and the other symptoms?—Yes.

12004. (*Dr. Wilson.*) But the false membrane does not appear within a day or two, does it?—Not as a rule.

12005. Now the success of the diphtheritic serum treatment in this country is based, is it not, mainly on the cases admitted into the London isolation hospitals before and since the introduction of the serum

treatment?—No; I have given a large number of tables from many other places.

12006. I am going to refer to some of those. You have admitted, I think, that if, for example, the type of the disease had become much milder in London since the serum treatment was commenced, that would influence the statistics somewhat, would it not?—I have stated that in my evidence about it.

12007. It would lessen their value. I think you have also stated that the bacteriological testing rather tends to make the cases appear more numerous, because slight cases are notified now which would not have been notified in old days?—I have suggested that that may very possibly happen.

12008. But if the death-rate per thousand of population has gone up in spite of the antitoxin treatment during the past ten years as it has, that surely could not be regarded as a statistical result in favour of the serum treatment, could it?—It could not be regarded as a statistical result in favour or against.

12009. Granting that the serum does assist in freeing or loosening the diphtheritic membrane owing to the reaction which it produces, and its effect on the circulation and minute blood vessels under the membrane, can it be said with certainty that its after effects are so beneficial as they are believed to be? I admit that it may have an effect?—I do not know how beneficial they are believed to be.

12010. You believe that the results of the treatment are beneficial, and you gauge them by figures?—Yes.

12011. For example, in your *précis* you state that paralytic symptoms are more common now than they were before the serum treatment?—Yes.

12012. And you hold that that is an argument that severe cases survive which would have proved fatal had not the serum been used?—I suggest that that is a possible explanation.

12013. Might I not also suggest that a possible explanation may be that, admitting that the serum may possess some action in assisting in loosening the membrane, its after effects may increase the number of paralytic cases?—You may hold that opinion, Sir.

12014. But is it not as logical as the one you have mentioned?—I have adduced some reason for my opinion; you have not done so yet for yours.

12015. I will not labour this point further. Though the general death-rate from the disease has gone up, the case mortality rate in the county of London, according to the statistics which you furnish, has gone down from over 20 per cent. in the pre-antitoxin days to about 10 per cent. in the last few years. That is correct, is it not?—That is correct.

12016. I see also that you refer to the statistics collected by Dr. Armstrong, Medical Officer of Health for Newcastle, in various towns in the north, centre, and south of England. You give that table?—Yes.

12017. These statistics show, do they not, that although there has been a reduction in the case mortality rate, it is not nearly so marked as it is in London?—That is so.

12018. And you attribute this, do you not, to the fact, or to the assumption, rather, that the antitoxin was not so freely used in these other towns as in London?—I think that is a probable interpretation.

12019. But is it not the fact that almost all medical men now use the antitoxin?—I think the majority of medical men use the antitoxin. But of that I have no way of judging except by the amount of antitoxin which is sold.

12020. Now I have here a very short extract from a paper recently published, which was read at a sessional meeting of the Sanitary Institute by Dr. Hibbert, who is Dr. Armstrong's deputy in Newcastle, and is also physician to the isolation hospital. In that paper he deals with the statistics which you have collected, but groups them in a different form. For example, during the five years from 1900 to 1904, in eighteen northern towns the average case mortality rate was 24·7 per cent., in five midland towns it was 15·7 per cent., and in eight southern towns 13·1 per cent.; and in London you say it is now 10 per cent.?—Yes.

12021. That table shows, does it not, that even during the years 1900 to 1904 the case mortality rate of diphtheria was even higher in the eighteen northern towns than it was in London before the serum treatment was introduced. For example, before the serum

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treatment was commenced in London it was about 24 per cent. ?—Yes, 20 to 24 per cent.

12022. And in the eighteen northern towns during these recent years, 1900 to 1904, it was as high as 24·7?—No, not quite; 18 in 1904 and 22·7 in 1900.

12023. Now he also makes these remarks in his paper:—"I may add that at the City hospital here" (of which he is physician), "where antitoxin in doses of from 6,000 to 12,000 units is given as a routine treatment, Dr. Harris and myself have not had evidence that antitoxin is the definite curative agent which it has apparently proved itself to be in the hospitals of the Metropolitan Asylums Board. There is, however, this fallacy as regards diphtheria statistics, that at least one southern town" (he does not name the town) "includes amongst its cases of diphtheria those 'contacts' in whose throats the bacillus is found, but who do not show any clinical symptoms of the disease." Does not this statement somewhat qualify your inference that the much higher case mortality in the northern towns is probably due to the fact that antitoxin is less used there. You see that they use it freely?—I am unable to say unless I have more data.

12024. I am quoting from this paper?—I am not complaining of the data that you have given me. I do not think the statement of this gentleman necessitates a modification of opinion on my part.

12025. But no medical practitioner, so far as I know, dare neglect the use of serum nowadays?—I am very glad to hear it.

12026. In country districts the parson would be down upon them, and the country squire would be asking why the serum was not used. As you quote several diphtheria statistical tables from Continental records, as well as from American records, I will now ask your opinion about some records which are given by the Medical Officer of Health of the City of Toronto, in his report for 1904, based on the diphtheria cases which were treated in the city hospital for ten years. It appears that there have all along been doubts among medical men there concerning the efficacy of serum—that is, in Toronto, and groups of unselected cases were treated in the hospital, some with antitoxin and some without, during those ten years; and this is a summary of the results for ten years. He says: "Since 1894, when the first dose of antitoxin was given at the hospital, the mortality results have almost always been unfavourable. In every year, save one, the percentage of deaths in antitoxin cases has even been higher than the ordinary hospital rate, which is of course influenced by the increased antitoxin mortality. In 1894 the figures were respectively 21·4, against 14·07, and this state of things continued without much improvement until 1902, when the antitoxin was about 1 per cent. under the gross hospital rate." Then he gives the statistics for 1903:—

	Cases	Deaths	per cent.
Total hospital admissions	- 565	65	11·5
Antitoxin treatment	- 228	37	16·2
Ordinary treatment	- 337	28	8·3

But then he goes on to observe, "The statistics of the last decade, which can now be presented, embrace a sufficient number of cases on which to base a fair opinion of the value of antitoxin in hospital practice.

	Cases	Deaths	per cent.
Total hospital admissions	5,100	667	13·0
Antitoxin treatment	- 1,132	181	16·0
Ordinary treatment	- 3,968	486	12·2."

And then he says, "In a report of this kind any speculations as to the cause of the pronounced failure of a remedy from which so much was expected would probably be deemed out of place. A bare statement of fact is therefore presented, and this can, if necessary, be supplemented or substantiated by a large accumulation of original and classified evidence of unquestionable reliability." Do you accept that report?—Not without examination.

12027. (Dr. Gaskell.) Have you seen that report before?—I have not seen that report before.

12028. (Dr. Wilson.) You also believe that the serum is effective as a prophylactic?—Certainly.

12029. Would you recommend, then, that it should be used in every household, as they use vaccination, when a case of diphtheria crops up?—I should always

use it as a prophylactic injection to any of my own children if one of them took diphtheria.

12030. But doctors do not inject themselves who attend cases of diphtheria, do they?—I imagine not.

12031. Now with regard to the anti-dysenteric serum; is that used in India, do you know; has it been taken up in India?—No, it has not been used in India to any extent.

12032. (Mr. Tomkinson.) How long were you in Melbourne?—Seven years.

12033. And with the exception of your experience there you have performed no experiments of a physiological kind upon animals?—Yes, I held a licence before I went out to Australia, in 1889 and 1890 in this country.

12034. I thought you said that that was the only case; perhaps I was mistaken. You there performed 30 or 40 experiments on living animals?—That was in connection with my lectures each year.

12035. And more besides?—A great many more besides.

12036. But, with the exception of that experience at Melbourne, have all your researches in the pursuit of science and discovery been confined to inoculation?—Oh, no.

12037. With regard to tetanus and hydrophobia, there is no claim that any cure of either has been discovered, once the disease has been established?—No, I make no claim.

12038. I think you include tetanus among the diseases which are occasionally cured. You say, "Unfortunately, the useful application of antitoxin serum therapy is at present limited to a comparatively few diseases; the most important of these are diphtheria, tetanus, and epidemic dysentery"?—It is usefully applied to prevent tetanus during the period prior to the development of symptoms.

12039. As a preventive, of course, I quite understand it. I thought you meant that it was usefully employed in the cure?—No; in the case of tetanus it is a prophylactic.

12040. And you claim that the use of this antitoxin or serum, or whatever is applied to either man or horses, is a prophylactic against tetanus?—Yes.

12041. I suppose you admit that that to a certain extent is guesswork?—No, I do not admit for one moment that it is guesswork. I submit that it is capable of scientific demonstration.

12042. It is a negative proof that tetanus does not follow in certain cases in which it is applied?—You are referring, I imagine, to the statistical evidence that it occurs in a certain number of wounds of a particular kind, and if you give tetanus antitoxin as a prophylactic it does not occur at all. I do not consider that that is guesswork. But, moreover, it is founded upon absolute demonstration. You can take a hundred animals and infect them with tetanus, and if you apply the prophylactic immediately not one in that 100 will develop the disease. Whereas if you take 100 similar animals and infect them with tetanus, and do not apply a prophylactic, they will all die of the disease. I call that the simplest form of scientific demonstration, apart from guesswork.

12043. (Dr. Gaskell.) In that case you speak of its curing disease?—It is really curing disease, because you apply the remedy after the infection, and before the manifestation of the symptoms.

12044. (Mr. Tomkinson.) In your *précis* you describe the treatment of a horse for antitoxin, and the effect upon the horse. You say, first of all, "It is carefully tested as being a healthy horse"; and then, "The first injection of the toxin may be followed by swelling at the site of inoculation, loss of appetite, general malaise, and rise of temperature." Have you often studied horses under this treatment?—I have seen a large number of horses under this treatment.

12045. Does the horse seem to suffer very much?—No, I think not.

12046. Is there great wasting?—Not at all. The horses compare favourably with those in anybody's stable. They are all fat and shiny.

12047. Have you seen the picture of the Khartoum dog?—I have.

12048. Do you consider that that would be the average result of certain inoculations?—It does not represent the condition of our horses.

12049. I suppose not, not from the description you have given of the horse, I am glad to hear. I wanted to know whether there was any suffering in the horse approaching the suffering that that dog had obviously had to go through?—No, nothing of the kind. There is no evidence that the horse during the process of immunisation suffers more than the man does who drinks too much the night before, say, and has a headache and general unsteadiness in the morning, and is off his feed. I should think that would put it in a simple, practical way.

12050. I see that you are strongly opposed to a limitation of the choice of animals for purposes of experiment?—Yes.

12051. Because you say that dogs and monkeys, being in many respects nearest to man, are the most useful subjects for experiments?—Yes. I place particular emphasis on the accessibility of the animal for the physiologist.

12052. I suppose in all the experiments that you have performed of the minor kind, the animals have not been subject to anæsthetics?—Not in the inoculations.

12053. May I ask, are the laws regulating the practice of vivisection in the Colonies at all akin to ours?—They differ in different countries. In New South Wales, where I was for some years, there is no law.

12054. There is a law, I suppose, for the prevention of cruelty to animals?—Yes, but there is no vivisection clause in it.

12055. Then there is no immunity or permission given to vivisection?—No; it was not supposed that anybody would want to vivisection when they framed the law.

12056. And it would be a breach of the law prohibiting cruelty to animals?—No; there was nothing to prevent us performing any experiments we liked for scientific purposes in the University in New South Wales. There was no Vivisection Act.

12057. (*Sir Mackenzie Chalmers.*) There was no regulation?—There was no regulation of vivisection.

12058. (*Mr. Tomkinson.*) But there was a law for the prevention of cruelty to animals in New South Wales?—Yes. If anybody had supposed we were cruel they might have prosecuted us under that Act.

12059. (*Sir Mackenzie Chalmers.*) Nobody thought of that?—Nobody did.

12060. (*Mr. Tomkinson.*) Was there a Vivisection Act in Melbourne?—In Melbourne there was a clause in the Cruelty to Animals Act allowing the Governor in Council to give a licence to such persons as he thought advisable to perform experiments on animals; and that licence in my case included myself and every thing done in my laboratory for which I was responsible.

12061. Were you the only person to whom it was granted?—No; there were some few other persons. The pathologist and the bacteriologist and others in connection with the scientific Departments of Agriculture and Stock also held licences.

12062. Was the use of anæsthetics imposed by that licence?—Not necessarily. The licence was a comprehensive one, and in returns one had to fill in what one did.

12063. And, as a general rule, did you use anæsthetics?—Not for inoculations; always for physiological experiments.

12064. Did you find it easy, or at all events, possible, to keep the animals perfectly anæsthetised for a considerable time—as long as you wanted?—All day if one wished. There is no difficulty.

12065. (*Dr. Gaskell.*) Tetanus is a very painful disease, is it not?—I imagine so.

12066. With regard to the production of antitoxin for tetanus, is any pain connected with it?—No, not the proper production of antitoxin.

12067. (*Sir Mackenzie Chalmers.*) In the commercial production?—No.

12068. (*Dr. Gaskell.*) Would you tell us why there is no pain connected with it, because it has been put before us that there is pain connected with the pro-

duction of antitoxin?—Because you never produce tetanus when you are immunising a horse.

12069. And short of producing tetanus, what is the case?—Nothing more than a slight rise in temperature and general *malaise* following each injection, which I have referred to in my evidence.

12070. I wanted to bring out simply that that applies to tetanus as well as to other things?—That is so.

12071. There is another question that I want to know about, too, and that is, what precautions are taken nowadays with respect to plague antitoxin—firstly, as to being sure that it is free from other deleterious products; and, secondly, as to standardising it. It has been said that the plague antitoxin has been out of favour in India, and would not be recommended on account of the want of precautions being taken in preparing it, and also the inefficiency of standardisation. Can you give me any information upon that subject?—The same precautions exactly are taken as with every other form of antitoxin to render it sterile and free from any contamination whatever. The reason why the plague serum is out of favour, as you phrase it, is, that it is not of very great value, unfortunately. Although it is the only rational remedy we have, it is impossible at present by injecting plague bacilli into the horse to make it respond by the manufacture of a sufficient amount of immunising bodies to be of great practical value. It is very weak compared to such sera as diphtheria and tetanus, and to that extent it is a disappointment.

12072. (*Sir Mackenzie Chalmers.*) You are referring to Yersin's serum?—Yes.

12073. (*Dr. Gaskell.*) In reality when bottles are sent out you cannot be quite sure that the standard dose put upon them is accurate. Is not that so?—I do not think there is any difficulty about that, because when you have a remedy that is too weak you give as much as you can of it.

12074. (*Sir Mackenzie Chalmers.*) You have told us that the work of the Lister Institute consists in preparing commercial sera and antitoxins. Is there any scientific work done there as well?—Yes, certainly. The preparation of the antitoxins that are of established value is only a very small part of the functions of the Institute. Practically the whole energies of the Institute are devoted to research work.

12075. How many people are working there?—I think there are about 25.

12076. Are any special lines of research going on now?—Yes, everybody is doing something or other.

12077. Merely inoculation work dealing with infective disease?—No; there is a chemical side to the Institute, where various chemical problems connected with biology are investigated.

12078. Does that involve animal experiments?—That may or may not be concerned with animal experiments. Often it is not.

12079. May I take it generally from your evidence that in your opinion research on the lines of the Institute cannot be carried on without animal experiments?—Most certainly. The Institute would have to go elsewhere if animal experiments were not permitted by law. It would have to go to one of the Colonies or America.

12080. You yourself have been on this Plague Commission, I think?—Yes.

12081. And you have been in India?—Yes.

12082. And were working there. What is the number of persons on the Plague Commission?—I think there are eight doctors.

12083. How many assistants?—I think, taking the minor assistants, it must amount to one hundred.

12084. Are all those people continually exposed to the virus of plague?—Yes, more or less.

12085. What precautions have been taken to secure them, if any?—They were inoculated at regular intervals.

12086. With Haffkine's prophylactic?—With Haffkine's prophylactic.

12087. Were they inoculated once a year?—They were inoculated more frequently than that, I think.

12088. Among your eight doctors who are continually handling this virulent material, how many cases have you had of accident?—None amongst the doctors.

Mr. C. J. Martin,
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12089. Yet from the nature of your experiments I suppose you are continually in contact with the poison?—Yes. They take reasonable precautions so as not to get it inside themselves.

12090. One of them, at any rate, has had some abortive attacks of plague, has he not?—One of them suffered from what is called *pestis minor*. I did not mention that.

12091. However, no effects followed. The prophylactic was effective?—Yes.

12092. How far was it effective among the native assistants?—To my knowledge there have been three cases of plague amongst them.

12093. Fatal or non-fatal?—They were non-fatal.

12094. Therefore amongst several hundred people in continual contact with this disease, the Haffkine vaccine has been a successful prophylactic?—I can only say that they did not die of plague.

12095. And they had every opportunity?—How far that is to be attributed to Haffkine's prophylactic can only be found out by dealing with a larger number of figures.

12096. The statistics are too small?—They are too small for me to attach importance to.

12097. Still, from your own experience, do you think it is right or superfluous to inoculate with Haffkine's vaccine these people specially exposed to the plague?—I can best answer that by saying that I have been always careful to inoculate myself.

12098. More than once?—Yes.

12099. It produces, I suppose, for two or three days a great discomfort?—Not at all. In my case it produced no discomfort, because I went about it in a gradual way, taking a very small dose in the first instance, and the next week a little more, and so on, which is the best way to immunise anybody.

12100. Just as you would a horse?—Yes, just as you would a horse; the dose was small to begin with, and gradually multiplied. I was never in the least incapacitated either in the way of appetite or my ordinary occupation. I had a slight soreness.

12101. When a strong prophylactic dose is given there is generally for two days discomfort, is there not?—Yes, but it is very seldom that it would incapacitate a European from going about his ordinary business. Occasionally it happens that a man for some reason or other is distinctly invalided, say, for 30 hours; but that is unusual.

12102. It does not take place if the inoculation is carried out gradually?—No, it does not take place then.

12103. Your experiments in India with plague have been animal experiments, of course?—The experiments have been entirely on animals.

12104. And you have as the result of those experiments discovered, at any rate, the common mode of transmission of plague?—Yes, I think so.

12105. Namely, by the rat flea, as you say?—Yes.

12106. You would hardly say that that is the only mode?—It is not the only possible mode; but the evidence adduced by the Commission is such as to lead one to regard it as the only serious mode of epidemic spread.

12107. Therefore, at any rate, you have accomplished this by your experiments—that you know the evil to be dealt with?—Yes.

12108. Though you have not yet succeeded in discovering anything like a remedy after the outset of the disease?—No, not yet. Whether it is possible to satisfactorily interfere is another question.

12109. Still is it or is it not an important step to know the means by which a disease is communicated from one person to another?—I think it is an essential step.

12110. And that you say cannot be obtained without the use of animals for experiment?—No.

12111. Incidentally, if the rat flea is the ordinary way of communicating the disease, how is it that doctors continually handling these animals in transferring the fleas from one to another, were not bitten?—I imagine they were occasionally bitten. For one thing, they wore top boots.

12112. But in combing out the fleas, to take them

from one animal to another, I mean?—The animal was chloroformed and the fleas were chloroformed. Otherwise it would have been impossible to have dealt with them; the Commission would have been exterminated.

12113. But you think there must have been occasional bites, and do you attribute the immunity to the prophylactic or not?—I attribute some of the immunity to the prophylactic.

12114. (*Chairman.*) Why did you say the flea was chloroformed?—The animal and the flea. If they were getting fleas off the animal they would put the animal in a receptacle with chloroform vapour, and that quiescent sent the animal to sleep and the fleas to sleep, and one could then pick them off at one's leisure.

12115. (*Sir Mackenzie Chalmers.*) And then apply them to another animal?—The fleas recovered from the anaesthetic in a few minutes, and they could then be dealt with as wished.

12116. Is it known at all how a flea biting either an animal or a human being can deposit the plague bacillus?—It is known that the flea whilst biting commonly discharges from behind.

12117. In the process of biting?—Nearly all suctorial animals whilst sucking blood at one end discharge the contents of the intestine at the other, and in that way they can discharge infection.

12118. Then the discharge of the intestine gets on to the puncture?—Yes.

12119. And that is the mode of entry?—That is the probable mode. It itches and you rub it.

12120. It is exactly the same with an animal?—Yes, the same with an animal.

12121. (*Chairman.*) Is it the discharge that gets into the wound?—I can only say that that seems the probable way in which infection actually takes place. The fact of the soiled proboscis is not excluded, but as infection may take place as much as fifteen days after the flea has fed upon the infected animal, during which fifteen days it has been fed upon the blood of a healthy animal every day, that practically excludes the soiling of the proboscis as the means of inoculation.

12122. (*Dr. Wilson.*) Does that apply also to the mode of infection conveyed by the mosquito in malaria?—No, it is more elaborate and more perfect in the case of the mosquito; because in the malaria parasite you have two stages, one which lives in the mosquito's body, and one which lives in the blood of man. The stage that lives in the mosquito's stomach ultimately breaks up into a large number of eggs, so to speak, which get into the salivary glands of the mosquito and are directly discharged with the saliva when biting.

12123. (*Chairman.*) But that discharge which you speak of from behind is common both to the flea and the gnat?—Yes.

12124. Only it is deposited further off the wound in the one case than in the other, I suppose. The gnat's body is very perceptibly longer than the flea?—Yes.

12125. (*Sir Mackenzie Chalmers.*) The gnat actually injects the poison?—Yes.

12126. The flea merely deposits it close by?—That is what we imagine is the usual case.

12127. There is no reason for thinking that the mandible of the flea is charged with infection?—There is the reason for not thinking it that I have just mentioned—namely, that the infection will remain for fifteen days, and the flea, being fed upon a healthy animal each time, would wash its proboscis, which practically excludes this mode of infection from practical politics.

12128. By means of experiments on animals have you discovered at all the length of the prophylaxis conferred by Haffkine's vaccine?—I do not think that there are very accurate data. The Commission has not concerned itself with that problem. There is certainly some immunity left in animals, and also apparently in man after twelve months from some of the statistics which were collected by Bannerman.

12129. Can you draw any general inference from the length of prophylaxis in an animal to the length of prophylaxis in man?—Only in a rough way.

12130. Is the incubation period the same in man as in animals?—Roughly speaking, everything takes place more rapidly the smaller the animal.

12131. The guinea-pig displays plague sooner after inoculation than the monkey, for instance?—Yes, all processes take place much more rapidly in small animals, so that everything goes a little quicker.

12132. (*Chairman.*) How about the flea. What is the period of prophylaxis?—The flea does not suffer.

12133. (*Sir Mackenzie Chalmers.*) The flea takes its plague as a food?—The flea is a mere passive transmitter.

12134. Is the Commission still at work, or has it completed its work?—It has completed what it undertook to do in the first instance, and it is now resting during the hot weather. The Committee have suggested to the Government of India certain other matters which it considers it would be advisable to investigate, and has expressed its willingness to do so if it is wished.

12135. It has issued one report?—It has issued an interim report: A second will be ready to-morrow, and there will be about three others, which will be more bulky, which deal largely with epidemiological matters and the analysis of a very large amount of evidence, which takes a long time.

12136. But, in your opinion, is it still important that further experiments should be made; or do you think

that experiment has done its work, and it is no longer necessary to use animals for plague experiments?—I would not say that for one moment. I do not know what may be necessary to-morrow or next month. I do not know how knowledge is going to grow.

12137. There is only one other question that I think I need trouble you with. As regards the Khartoum dog which was referred to, I imagine that people who die of trypanosomiasis would be attacked exactly the same as that dog, and look as miserable in a photograph?—Yes, the people who die of sleeping sickness very closely resemble that dog.

12138. It is not a very suffering disease in the later stages, is it?—No; but that particular trypanosomiasis was a disease of mules, which was being investigated.

12139. For the purpose of finding a cure and preventing these animals from suffering from it?—First of all for ascertaining what the disease was, and then if possible how one could best prevent it by sanitary means, or cure it if possible; and dogs were used rather than mules, as being convenient and cheap in Khartoum.

12140. The only means of eventually combating the disease being to get exact knowledge as to its nature and its mode of transmission as your first starting point?—That is the case.

*Mr. C. J.
Martin,
M.B., D.S.,
F.R.S.*

10 July 1907.

TWENTY-SEVENTH DAY.

Wednesday, 17th July 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.

Sir W. S. CHURCH, Bart., K.C.B., M.D.

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.

Sir J. McFADYEAN, M.B.

Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.

Mr. W. H. GASKELL, M.D., F.R.S.

Mr. J. TOMKINSON, M.P.

Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. C. J. MARTIN, M.B., D.SC., F.R.S., recalled; and further Examined.

Mr. C. J. Martin, 17 July 1907. 12141. (*Sir Mackenzie Chalmers.*) There was one question that I forgot to ask you last week, arising out of your plague experiments in India. If I understood you aright, your experiments established that the ordinary bubonic form, excluding the pneumonic form, is not infectious?—Our experiments did not establish that. It was established by the clinical experience of the plague in India.

12142. It is quite consistent with your experiments with respect to infecting animals, when you excluded the fleas?—Yes.

12143. It had been established before, by clinical experience, that plague is not infectious and barely contagious?—In order that there should be no misapprehension as to what is meant by the words infectious and contagious, I might say that the evidence from the experience of plague in hospitals in India and in houses in India is that it does not spread from one patient to another, provided that it is confined to the bubonic form.

12144. Your experiments with animals confirmed that?—Our conclusions, from our experiments, are consonant with that experience.

12145. You would not put it higher than that?—No.

12146. You have been a professor of physiology?—Yes.

12147. But you are yourself a medical man, holding medical degrees?—Yes.

12148. A question has been raised as to whether licences and certificates under the Act ought to be granted to people who have no medical qualifications. What is your opinion on that?—I think there is no reason whatever why those persons should be excluded, provided that they are suitable investigators.

12149. Have you the same guarantees for character and humanity in their case?—I think so.

12150. You think that persons who are what I may call pure physiologists ought to have licences?—I think the pure physiologists are quite as humane as the doctors.

12151. But have the certifying authorities the same guarantee in that case? That is rather a difficulty. Of course, in the case of a medical man, he belongs to a great profession, and you have a hold on him that you have not in the case of men belonging to no recognised profession?—I think that the certifying authorities can easily acquire the necessary information.

12152. There are, I believe, distinguished physiologists who are not medical men?—The Professor of Physiology at Cambridge is not a medical man—one of the most distinguished physiologists in this country.

12153. Generally have you any alterations that you would suggest, from your experience, in the existing law and practice?—On the whole I am content with the working of the present Act, but I would take exception to the recent action of the Home Office in not extending a licence to foreign scientists wishing to work in our laboratories.

12154. Would you kindly give us your grounds for that; it is an important question?—First of all, we were all of us indebted years ago to foreign laboratories for the hospitality they afforded us; and there is a general brotherhood amongst scientific men in Europe and America, so that if one wishes to learn something in particular one goes to the man who has devoted his life to that particular subject, and one can go to his laboratory and learn his methods and whatever he is able to teach one. That recent action of the Home Office has prevented any foreign scientist from coming to England to learn from us.

12155. (*Chairman.*) Do you think that it might lead to retaliation on the part of foreign laboratories?—No, I do not think it would lead to retaliation. I think there would be too much sympathy with our disabilities for that.

12156. (*Sir Mackenzie Chalmers.*) May I put this to you as a difficulty which the Home Office might feel? In the case of an Englishman he is well aware of the prevalent English feeling on the subject of experiments on animals, and there is certainly an idea that foreigners are less careful of animal suffering than Englishmen?—I think that difficulty could be got over by placing greater responsibilities upon the heads of laboratories.

12157. That is what I wanted to know. Can you develop that at all; can you suggest what form it should take?—The form of licence that I had in Victoria grants that in principle. That licence included the head of a laboratory and such persons as he made himself responsible for; and he returned to the Government everything.

12158. He reports everything?—He reports everything.

12159. He is responsible for the whole of the work done in his laboratory?—He takes that responsibility; the head of the laboratory must take that responsibility.

12160. You think that under those conditions there

would be no objection to granting licences to foreigners who had proved character and qualifications?—None whatever.

12161. You would make the English head of the laboratory responsible for all the work done in his laboratory, and would only allow the foreigner to work under that particular specified licensee?—I think that would get over the difficulty. In the case of my own laboratory I certainly regard myself as responsible for the carrying out of the Act.

12162. That the law is honestly and carefully observed?—Yes, in my own laboratory.

12163. Whether you are experimenting yourself or anyone is experimenting under your supervision?—Certainly.

12164. (*Sir John McFadyen.*) I want to direct your attention to Question 11924 of your evidence, given on the last day. It was put to you in the form of an affirmation, but I take it that it was intended as a question: "Of course, as you have said, the disease has been effectively stamped out in this country, without having recourse to Pasteur's treatment?" and you in answer to that question said, "Certainly." On reading it over it seemed to me that there must have been a misunderstanding, either on the part of Dr. Wilson, in putting the question, or upon your part in assenting to it; because it appears to imply either that it had been claimed by somebody that the application of the Pasteurian treatment to human beings might be expected to have the effect of exterminating rabies in dogs; or alternatively it appears to imply that the Pasteurian treatment was never applied to people in this country who were bitten by rabid dogs. I want to know from you now whether you are prepared to answer the question "Certainly," taking it in either of those senses?—When I said "Certainly," I meant that the Pasteur treatment had nothing to do with the stamping out of rabies in dogs.

12165. But it is qualified by the expression "in this country"?—But rabies is essentially a disease of the dog, and the treatment of the men who are the victims of the dogs' bites by Pasteur's treatment will not affect the disease amongst the dogs. That is why I said "Certainly."

12166. But have you ever heard it suggested that the Pasteurian treatment, whether it is good or bad, can have any influence upon the spread of rabies or the continuance of it among dogs?—No.

12167. You would not assent to the statement that in this country people have not derived any benefit from the Pasteurian treatment?—No.

12168. Do you believe, on the contrary, that some of them have derived benefit?—Certainly.

12169. Then I want to direct your attention to a question or two on the preceding page, beginning at Question No. 11914. These questions were with regard to the reason why, in the statistics purporting to set forth the failures of the Pasteurian treatment, those cases were excluded in which the disease developed within fifteen days after the treatment had been completed; and you have given the reason which to you appears to justify the exclusion of these cases in answer to question 11917. But the question I want to ask you is this: Are the available statistics greatly affected, supposing these cases are included among the failures; or could you give us any evidence showing what the percentage is in which the disease develops in persons subjected to the Pasteurian treatment, excluding no cases at all?—If these cases were not excluded the mortality would be about doubled, or a little more than doubled, so far as I can judge. I can only remember the figures in round numbers for one Institute, but that is a large institute, and included about 30,000 cases. The mortality given in the usual way, excluding persons who have died within fifteen days of the treatment, was about 0.45 per cent., and, taking the total mortality, it was about 1 per cent. (See note after Question 12173 below.)

12170. As compared with the mortality among non-treated persons?—In the figures that I collected and gave in my evidence that mortality would appear to be about 17 per cent. I can put in the exact figures, of course, if it is wished, but that is as nearly as I can remember.

12171. I think it would be as well to calculate out the figures showing what the calculated mortality for bitten persons was?—Not for every institute, I trust.

12172. The larger the number of statistics you put in, the more convincing it will be. Probably 30,000 cases would suffice for the one institute?—Yes, I can put my hand on 30,000 cases from one institute; but to put in the total statistics for 150,000 cases would be a great labour.

12173. I think we would be content with 30,000?—From 1890 to 1905 28,033 persons underwent Pasteur anti-rabies inoculation at the Buda Pesth Pasteur Institute. (Report of the Institute, 1905, by Dr. August v. Szekeley, director Centraabl. f. Bakt. Part 1. Referate vol. 40, p. 34, 1907.) Of these 159 died during or within 15 days after the treatment and 129 subsequently. The total mortality was 1.02 per cent. and the mortality, excluding the 159 cases from the statistics, 0.46 per cent.

12174. (*Sir Mackenzie Chalmers.*) Have you statistics of the Kasauli Institute?—Not with me; but they are given in the report of the Sanitary Commissioner for India year by year.

12175. They show the number of people treated, and the number of failures, but they do not show at what date the treatment was commenced?—No, they are not at all full in that Report.

12176. (*Sir William Collins.*) The evidence which you have been so good as to give the Commission has dealt mainly with the causation and control of infectious and infective diseases, has it not? So far as our knowledge and our power in regard to them have, in your opinion, been the result of experiments upon living animals?—Yes.

12177. Do you think that in the evidence which you have given to the Commission you have had due regard to what might be called the failures as well as to the successes of bacteriology?—I have not been concerned with the failures of bacteriology. There are failures in every form of scientific inquiry.

12178. For instance, I notice on page 1 of your *précis* you tell us that Pasteur's methods have led to the discovery of the causation of most infectious diseases to which man is liable?—I think that is so.

12179. Would you tell me, for instance, in the case of scarlet fever, have we identified the cause of it?—No.

12180. Or of measles?—No.

12181. Typhus?—No.

12182. Small-pox?—No.

12183. Chicken-pox?—No.

12184. Whooping cough?—Yes, that is probably correct.

12185. Would you name the organism?—No. I will name the authors—Bordet and Gengou.

12186. Are they the only experimenters who claim to have identified an organism as the cause of whooping cough?—I should think not.

12187. Do you regard their organism as established?—No, not as yet, but as very probable.

12188. Influenza?—Yes.

12189. Do you think that is established?—I think that is established.

12190. Would you name the author who has identified the true cause of influenza?—Pfeiffer.

12191. Have other authors claimed to have established other organisms as the cause of influenza?—No doubt.

12192. Mumps?—No, I think not.

12193. You would, I suppose, hardly have the Commission believe that sanitary science did not exist before the dawn of bacteriology?—No.

12194. You would hardly put aside as worthless the work of Chadwick, and Farr, and Simpson, and Radcliffe, and Dr. Kay, and Southwood Smith, and others?—No.

12195. Have there been means of stamping out or reducing mortality from diseases whose cause has not been identified as a particular organism?—Yes.

12196. Such as typhus?—Rabies.

12197. Typhus?—Small-pox.

12198. Do you agree that typhus is one of them?—Yes.

12199. And cattle plague, for instance; was that successfully dealt with before the identification of a par-

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ticular organism as its cause?—No organism has yet been identified as the cause of rinderpest, I presume you mean?

12200. You would dispute any contention to the contrary?—Yes.

12201. And I think you intimated, if you did not actually state, that puerperal fever, by the reforms introduced by Semelweiss, was dealt with satisfactorily as regards its prevention before the identification of any particular organism as its cause?—I endeavored to point out that it was not dealt with satisfactorily, because persons would not believe in its infectivity until they had evidence of a specific cause.

12202. Was it not claimed in Vienna so long ago as the sixties of last century that by the adoption of Semelweiss's views the mortality from puerperal fever had been reduced, if not abolished, in lying-in institutions?—Semelweiss was practically persecuted in Vienna, and driven from his post, and died in a lunatic asylum, from suicide, I believe.

12203. Quite true; and a monument in recent years has been erected to his memory in Buda Pesth?—I think so.

12204. But if you consulted a letter written by himself to the "Medical Times Gazette" in 1861, you will find that there were those who recognised the truth of his doctrine even in Austria during his lifetime?—No doubt.

12205. And before the identification of an organism as the cause of puerperal fever?—Certainly.

12206. Now as regards tubercle, was it not at one time contended by the Medical Department of the Privy Council that Dr. Burdon Sanderson had established the cause of tubercle? You quoted several authors, such as Klencke and Cohnheim, but I do not see a reference to Sir John Burdon Sanderson?—I think, if I remember rightly, that Dr. Sanderson's (as he was then) observations were in the direction of showing that tubercle was not a specific disease.

12207. That it was due to what he called the common septic ferment?—Yes. But Sir John Burdon Sanderson was mistaken, and, of course, he fully realised that later.

12208. And did not Sir John Simon, in reporting his researches to the Privy Council, state in regard to these matters that he thought they might "now be taken as settled"?—That I do not know; but if so, Sir John Simon was also mistaken.

12209. You know Koch's work on the cure of consumption?—Do you mean on the introduction of tuberculin inoculations about 15 years ago?

12210. It was a work entitled "The Cure of Consumption," translated and published in this country in 1890?—I am not familiar with that pamphlet, but I was in London at the time when Koch's treatment was being extensively applied in some of the London hospitals.

12211. It was claimed in that work that "Phthisis in the early stages can be cured with certainty by this remedy"—that is, tuberculin. Is that established?—No, that is wrong, I imagine.

12212. Is tuberculin recognised as a cure for consumption?—The revival of tuberculin as a means of treatment has occurred within recent years, and I believe is giving beneficial results under modified conditions with regard to dosage.

12213. Do you remember Koch's last statement when in this country in regard to the prevention of consumption?—Koch has made a good many statements.

12214. Did he not rather advocate what might be called the sanitary mode of prevention rather than any method of inoculation?—I think prevention is always better than cure.

12215. But was the prevention to be by the wholesale inoculation of tuberculin or by the rooting out of insanitary dwellings, in Koch's opinion?—By preventing infection by all means.

12216. The passage to which I was referring is "That above all other considerations he placed 'the domestic conditions under which the poor everywhere, but especially in great cities, have to live. It is,' he said, 'the overcrowded dwellings of the poor that we have to regard as the real breeding places of tuberculosis; it is out of them that the disease always crops up anew, and it is to the abolition of these conditions

that we must first and foremost direct our attention if we wish to attack the evil at its root, and to wage war against it with effective weapons'?"—I think that is quite sound.

12217. That was in 1901; I think it was his last visit to London. I notice also in this work of Koch's on the cure of consumption that he makes this rather remarkable statement: "As regards the effect of the remedy" (tuberculin) "on human beings, it was evident at the very beginning of the experiments, that in one very important point the effect of the remedy on man is entirely different from that on the guinea pig, which is the animal usually experimented upon," and he goes on to say: "Here, again, is a fresh and conclusive proof of that most important rule for all experimentalists, though an experiment on an animal gives no certain indication of the result of the same experiment upon a human being"?—I think that is true—"no certain indication."

12218. You would agree with that opinion?—I agree with "no certain indication."

12219. Do you agree with the opinion of Koch in regard to the non-communicability of the tubercle of animals to man?—No.

12220. Did he claim to have established that as the result of experiments upon animals?—I do not see how he could establish the non-communicability of bovine tubercle to man by experiments on animals.

12221. Did he claim that the tubercle of animals could not be conveyed to man?—It was his opinion that it was anyhow an insignificant way by which human beings become infected with tubercle.

12222. Has that been disproved?—I think it has been disproved by the Royal Commissions on Tuberculosis both in England and in Germany, and by other individual workers.

12223. Has that been the result of experiments on animals?—Yes.

12224. Then the disproof has been the result of experiments on animals, in your opinion?—Experiments upon animals have been necessary to obtain data for coming to that conclusion.

12225. Did not Koch claim that it was from experiments on animals that he got the data necessary to come to his, in your opinion, erroneous conclusion?—I do not think so.

12226. Did he not use experiments upon animals in the course of his investigations?—Certainly.

12227. Have you looked into the statistics of tetanus as to whether the death rate is going up or down?—No.

12228. Do you remember Kanthack's paper with regard to tetanus; did he not collect some cases?—I do not remember it.

12229. I think you spoke in one place of the cause of syphilis having been established by experiments on animals?—Did I?

12230. Yes, on page 13 of your *précis*?—I do not think it amounts quite to the statement that you have put in my mouth.

12231. I am very anxious that you should make your own statement, and if I have put one into your mouth I apologise for having done so. Will you make your own statement? You speak of "many cases in which bacteriological diagnosis of a disease" has been arrived at by experiments on animals, and in that category you mentioned syphilis?—Yes.

12232. I was under the impression that you implied that syphilis could be diagnosed by bacteriological investigation upon animals. If I am wrong in that contention, will you be so good as to correct me?—You are quite wrong.

12233. Then, perhaps, you will state what is your contention?—The contention in that statement is that syphilis can be diagnosed by the recognition of the spirochæta pallida.

12234. Has not the spirochæta pallida been claimed as the cause of syphilis?—Yes.

12235. By Schaudin?—Schaudin himself did not claim it as the cause. He was the discoverer of it in syphilitic material. His partner, Professor Hoffman, suggested it as the cause. It has been extensively investigated by hundreds of persons in every country during the last two or three years, and has been found

to be almost invariably associated with syphilitic lesions of different kinds.

12236. Then am I right in thinking that, if not by you and possibly not by Schaudin, at any rate, by some observers, the spirochaeta pallida has been claimed as the cause of syphilis?—By hundreds. And strength has been given to that claim by the fact that material containing the spirochaeta pallida can be inoculated into apes and reproduce the disease.

12237. Up to a few years ago was not Lustgarten's bacillus claimed as the cause of syphilis?—Lustgarten claimed the bacillus as being the cause of syphilis, but he was mistaken and was speedily shown to be mistaken.

12238. For many years Lustgarten's bacillus was mentioned in text books as the cause of syphilis, was it not?—That may be so.

12239. You remember Mr. Watson Cheyne's remark in regard to that, in which he said, "But lately Alvarez and Favel state that they have found bacilli in the smegma taken from the healthy prepuce which stain almost in precisely the same manner as Lustgarten's bacillus and as the tubercle bacillus"?—That was one of the observations which showed that Lustgarten's contentions were not justified.

12240. Do you agree with Mr. Watson Cheyne in that statement?—Yes.

12241. You have given the figures for deaths from hydrophobia in Paris in your *précis*. Have you looked into the statistics for this country?—No, I have not looked into the statistics for this country. So far as I know, anti-rabic inoculation has never been performed in this country.

12242. But the Registrar-General for England and Wales publishes the figures of the deaths from hydrophobia?—I believe so.

12243. I have them before me from 1886 to 1905 (the same years that you give for Paris). I find (page 48), beginning with 1886, there were 2, 2, 4, 8, 4, 1, 1, 1, 2, 3, 1, 0, 1; and then since 1899, none—that is, of females—and of males for the same years (page 34), 24, 27, 10, 22, 4, 6, 5, 3, 11, 17, 7, 6, 1, 0, 0, 2, 0, 0, 0. That makes 175 in the same years that you have given the figures for Paris as 124, I think. One is comparing England and Wales, of course, with Paris. Has it occurred to you to make any remark upon those figures?—I cannot see that there is any connection between those two sets of figures.

12244. Are you able to give any suggestion as to the reason why there has been hardly any fatal hydrophobia or rabies in man in this country since 1899?—I believe there has been no rabies in this country since about that time.

12245. Will you give us your opinion as to why there has been no rabies since 1899?—Because of Mr. Long's Bill to muzzle the dogs.

12246. The result of muzzling the dogs has been the abolition of rabies in dogs and the absence of hydrophobia in human beings?—Yes.

12247. I thought that would be the explanation that you would give me.

12248. (Colonel Lockwood.) And the restrictions on importation?—Yes, the quarantine restrictions.

12249. (Sir William Collins.) Are similar methods not practised in France?—No, people interested in rabies in France have urged upon the French Government to employ some such means, and have pointed to England as the great example. In Germany muzzling orders and the destruction of stray dogs is carefully carried out, and one way in which they have diminished the number of dogs of doubtful ownership was by doubling the dog tax.

12250. At any rate, in this respect this country seems to have the advantage of France?—It has the advantage of France in this respect.

12251. (Sir John McFadyen.) Has not Great Britain in this connection got an immense advantage through being an island? Is it possible to establish a quarantine for dogs all round the French frontier, which would be absolutely essential to securing the result we have in this country, would it not?—Yes, it would.

12252. (Sir William Collins.) Now, as regards the curative value of diphtheria antitoxin, I see you have given various percentages of case mortality of diphtheria apart from the use of antitoxin?—The case mortality from antitoxin varies much in different years and in different epidemics.

12253. On page 20 of your *précis* I see you give 80 per cent. in one hospital in Paris, 60 per cent. in another, and then on page 21 you give 21.6 per cent. in 1892 in London, and, as you say, there are great variations in the case mortality apart from antitoxin?—Yes, quite so.

12254. On page 23 of your *précis* you give some Russian statistics—cases treated by antitoxin, mortality 14.6 per cent.; cases not so treated, 34.1 per cent. Have you looked into the figures of cases treated with and without antitoxin in London?—I do not know where to get any reliable figures that coincide in time for London. I doubt whether there are any available.

12255. The statistical report of the Metropolitan Asylums Board purports to supply them?—Coincident in time?

12256. Year by year. On page 309 of their Medical Supplement for 1906 I see cases treated with antitoxin, mortality 10.4 per cent.; cases not so treated, mortality 1.5 per cent. I thought you would desire to offer some explanation of that?—I am not aware of those figures. I have no doubt that there is a satisfactory explanation of them consistent with what I have put forth.

12257. You have given a statement on pages 23 and 24 of your *précis* as to the caution that is necessary in dealing with mortality statistics of diphtheria. Has the mode of diagnosing diphtheria changed largely since the introduction of the antitoxin treatment?—I imagine that a considerable number of cases are now diagnosed with greater certainty by bacteriological diagnosis of swabs from the throat.

12258. Prior to 1895 were any cases diagnosed in that way?—Yes.

12259. Will you give me the year prior to which no such case was diagnosed in that way?—Say 1833.

12260. When was the treatment by antitoxin and the diagnosis of it by finding the bacillus commenced?—The discovery of the bacillus was in 1882; it was established in 1882 and Roux's cases were treated in 1894.

12261. When would you say that the bacteriological mode of diagnosis was largely adopted?—As a routine practice in England about 1890—somewhere between 1890 and 1892, I imagine.

12262. And the antitoxin treatment commenced and became general in what year?—It commenced in this country about the middle of 1895.

12263. Has the introduction and the large use of bacteriological diagnosis been the means of bringing, as it were, under review a large number of cases which, prior to the use of the bacteriological diagnosis, were not called diphtheria?—I have indicated in my evidence that that is extremely probable.

12264. And that has to be taken into account in estimating the mortality?—Yes.

12265. Both case mortality and mortality per population, or not?—In estimating the case mortality.

12266. The case mortality only. Now, according to the figures published by the Metropolitan Asylums Board, the case mortality in 1888 was 59.3 per cent. for diphtheria, and then in the following years it fell to 40.7, 33.5, 30.6, 29.3, 30.4, 29.3. Those years would be prior to the antitoxin treatment, would they not?—Which was the last year?

12267. 1894—29.3?—Yes.

12268. There was a fall in the case mortality prior to the antitoxin treatment?—According to those figures, yes.

12269. And since 1894 the mortality has further declined from 22.8 down to 8.8 in 1906. I understand the point made by you, on page 23 of your *précis*, is, that if the cases of diphtheria are largely added to by the superaddition of a number of mild cases recognised by bacteriological diagnosis, that would have the effect of reducing the case mortality?—It would have that effect.

12270. Are you able to estimate the extent to which that cause is operative?—No.

12271. You yourself have written upon snake venom, have you not, in Clifford Allbutt's "System of Medicine"?—Yes.

12272. I see you state there, "The mortality in persons bitten by the larger snakes of India and South America would not, from the scanty records available, appear to be more than 30 per cent."?—I may have said

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12273. The mortality in persons bitten?—May I have the date; there are two editions.

12274. 1907 is the one this has been extracted from?—That is the last edition. I may have made that statement.

12275. You also, I think, in this paper, discuss the method advocated by Sir Lauder Brunton and Major Rogers?—I have no doubt.

12276. I find this passage, "Washing the punctures with any kind of solution is of course futile. Wall also considers that the application of any disinfecting agent to an incision through the wounds is almost useless; he found that such a method sometimes succeeded after the injection of the poison with a syringe, but was without avail after the natural bite of a snake. The same difficulty will be met with in the method lately advocated by Sir L. Brunton, Sir J. Fayrer, and Major Rogers, in which it is advised that an incision be made over the punctures with a lancet-shaped blade and crystals of permanganate of potash then be rubbed in. Major Rogers found this method successful in the case of cats, but in these experiments he knew accurately the direction taken by the point of the needle. In practice we find that it will only be by chance that the permanganate reaches the poison, which may be placed deeply in the areolar tissue and at some distance from the punctures." That was your opinion in 1907, I gather?—It is my opinion now.

12277. I thought you would probably give me that answer.

12278. (Chairman.) When you speak of the larger snakes in India and South America and the percentage of deaths, what do you include in the larger snakes, because some of the large snakes do not kill by biting?—No, the very large snakes are not poisonous.

12279. Do you include the cobra?—Yes.

12280. The rattlesnake?—Yes, and the fer de lance.

12281. The most deadly?—Yes.

12282. (Sir William Collins.) You think, then, that all this washing of punctures with any kind of solution is futile?—Yes.

12283. You attach some importance to the elastic ligature and to excision of the infected part?—Yes.

12284. Do you regard that as the only useful treatment that at present can be advocated?—No, I think you can get a certain amount of usefulness by the injection of antivenin—that is a serum.

12285. Do you agree that the difficulty with regard to that is to get antivenin, which is polyvalent?—Yes.

12286. Then as regards plague, you are a member of the Committee on Plague that is now engaged on its researches, are you not?—There is a Committee and a Commission.

12287. I am afraid I cannot discriminate; will you be so good as to discriminate between the Committee and the Commission?—The Committee is an Advisory Committee appointed by the Secretary of State for India, and the work has been done by a Commission.

12288. Is it a Royal Commission?—No, a Commission.

12289. Who appointed the Commission?—The Committee.

12290. The Commission has emanated from the Committee?—Yes, the Committee have placed their work in Commission. I was in the first instance, at the request of my colleagues on the Advisory Committee, in India for eight months organising things and starting the work, so that to that extent I was a member of the Commission as well as of the Committee.

12291. There was a previous Plague Commission, was there not, in 1901, which, I think, reported?—That was a Commission to take evidence. Ours is essentially a working Commission.

12292. Have you read the report of the previous Commission?—I have read most of it at some time or other. It is several large volumes.

12293. I notice that amongst their conclusions they state: "Plague has attacked persons who have undergone inoculation as many as four times in the course of two years previous to their attack," and, again, "as many as 8 per cent. of the inoculated population may suffer from plague, as was the case in Bulsar. Many varying influences have been at work in determining the rate of attack in different places,

and it is impossible to give a numerical expression for the measures of protection against attack which inoculation confers"?—That was the opinion of the Indian Plague Commission from the early records of inoculation, I imagine.

12294. Six years ago?—Yes, it is a long time ago.

12295. I think you investigated, or had something to do with the investigation, with regard to certain untoward results which followed the use of Haffkine's vaccine at Malkowal?—There were certain questions of scientific fact and opinion concerning that accident, which were referred to the Lister Institute.

12296. As we have had the matter mentioned, and you yourself, I think, reported upon it for the Lister Institute, perhaps you will be so good as to state the result of your inquiries as to the source of contamination of tetanus, from which, I think, 19 persons died who were vaccinated with Haffkine's material?—The expression of opinion of the Institute was, as briefly as possible, in the following words: "That the probabilities are in favour of the view that the tetanus impurity was primarily in the fluid, but that they did not feel justified in asserting this as a proved fact in view of the possibility of contamination at Malkowal."

12297. Did the conclusions of the Institute coincide with those of the Commission?—I am not quite sure what the conclusions of the Commission were. I think they substantially coincided.

12298. I have before me a report entitled "East India (Punjab) Inquiry into Deaths from Tetanus" (Cd. No. 106), which states: "With reference to the findings of the Malkowal Commission, the Institute" (that is, your Institute, the Lister Institute) "were asked to report, among other matters (1) on the comparative efficacy of the standard and the new fluids as a protection against plague; (2) on the comparative liability of each fluid to contamination; (3) on the probable origin of the tetanus virus in the Malkowal cases. Their Report on these questions reached India early in December, 1904. They summarised their conclusions as follows:—(1) The Institute sees no reason to differ from the conclusions of the Commission that the new prophylactic is not less efficacious than the old. (2) The Institute is of opinion that in the hands of more or less unskilled workers it is easier to ensure freedom from contamination by Mr. Haffkine's 'standard method' of manufacturing plague vaccine than with the 'water-agar process' as employed by him. (3) The Institute is in entire agreement with the Commission as to the value of 0.5 per cent. carbolic acid in restraining tetanus growth when added to plague prophylactic, and its experiments emphasise still further the importance of this addition in preventing the growth and toxin formation in a vaccine which might be liable to the possibility of contamination with the spores of tetanus. (4) The conclusions of the Institute coincide with those of the Commission that in all probability the tetanus was, at the time of the inoculation, in the fluid contained in the bottle, but that it is impossible to determine at what stage in its history or in what way bottle 53N became contaminated." Were those the conclusions that appeared over your signature?—Yes.

12299. Then the conclusions of the Institute apparently did coincide with those of the Commission?—I think, in drafting that report, I used the word "coincide" when I should not have done. I think the Commission went a little further. The conclusions of the Institute coincided with my view of the findings of the Commission, but, as was pointed out to me afterwards, the Commission had gone a little further than we had in their views.

12300. Do you think that the inoculation with Haffkine's vaccine or any other vaccine in India is having a valuable influence in checking the spread of plague?—Very valuable.

12301. Do the figures indicate that?—I think so.

12302. I have before me a memorandum of the Royal College of Physicians, in July, 1905, on Plague, in which they say: "The total number of recorded deaths from plague in India during 1896, the first year of the present epidemic, was 30,000. In many weeks of last year, 1904, the mortality amounted to over 30,000 a week, and the total mortality from the disease for that year was 1,040,000. This year 687,705 deaths from plague were recorded for the first four months. The

mortality from plague in India has become greater in each successive year for nine years, the year 1900 excepted; and since 1896 nearly four million persons have died of the disease." From recent replies in Parliament I gather that from September, 1906, to April, 1907, 5,326,000 deaths from plague occurred, and in April, 1907, there were 314,000. Has plague ever been more prevalent in India in the memory of man than it has been during the past year?—Not that I am aware of. In reply to a question by Sir Mackenzie Chalmers, last week, I was unable to agree that the evidence that some hundred or so inoculated persons under our control in India had not suffered from plague to any extent was of any value, on account of the small number; but I have since remembered that there was a very good observation made amongst all the sweepers of the whole City of Bombay, who were being utilised in connection with our Commission to gather in dead rats every morning, and the amount came to upwards of 5,000 a week, all of which were examined for plague by the Commission.

12303. (*Sir Mackenzie Chalmers.*) 5,000 dead rats, or sweepers?—5,000 dead rats. These men were under the control of the health authorities—the municipal officer of health—they were working to assist us. The figures there, I think, are large enough to be significant. There were 7,182 inoculated sweepers, and there were 418 uninoculated sweepers. The attacks of plague were as follows:—Amongst the 7,000 inoculated, 14; amongst the 4,000 uninoculated, 28. The deaths among the 7,000 inoculated were 13, and amongst the 4,000 uninoculated 26. In other words, the percentage of attacks among the uninoculated was 6.7, and among the inoculated 1.9.

12304. (*Sir William Collins.*) What paper are you quoting?—This is from the Report of the Plague Research Laboratory for the official year ending the 31st March, 1906.

12305. Were they all housed in the same way; because I saw some reference to sweepers being housed in certain quarters, in one of the official reports?—The sweepers are a low caste people; they always live by themselves. But not altogether for the whole of Bombay. In each quarter of the town there are certain streets where the sweepers live.

12306. Are there any living in mud huts?—Not in Bombay, I imagine.

12307. (*Chairman.*) The mortality among those attacked seems to have been about the same in each case?—Yes.

12308. 26 out of 28, and 13 out of 14?—Yes.

12309. (*Mr. Tomkinson.*) Are those percentages the percentages of those attacked inoculated or not inoculated?—Yes.

12310. Not the deaths of those attacked?—No.

12311. (*Sir William Church.*) To what should you attribute the fact that Semmelweis's teaching did not spread in Europe or Holmes' in America with regard to puerperal fever?—To the natural disability of man to assimilate a new idea.

12312. That is to say, they were unable to demonstrate the cause of the spread of the disease. It was a mere theory?—Yes.

12313. Until bacteriology enabled a definite cause to be demonstrated?—That is my opinion.

12314. Sir William Collins mentioned a number of diseases to which we are liable, and on which no light has yet been thrown by bacteriology as to their cause—mumps, typhoid, measles, chicken-pox, and various others; but are any of those diseases diseases which we share in common with other animals?—That has been one of the difficulties in ascertaining their cause. In most of those cases it is impossible to communicate them to animals and make an experiment.

12315. Therefore we have been unable to study them on animals?—Yes; I think that is largely the reason why we have proceeded such a short distance.

12316. I want to ask you one or two questions about diphtheria. First of all I want to know what is your definition of paralysis? You have been good enough to furnish us with a table showing the total number of cases of paralysis, some severe, and the number of fatal cases, in answer to Question 11736?—For the purposes of that table I imagine that the slightest interference with motive power in any part of the body is regarded as a case of diphtheritic paralysis.

12317. It would include then all cases of paralysis of the palate or of the eyes?—Yes.

12318. And also of the limbs?—Certainly, the slightest difficulty in swallowing will be called a case of diphtheritic paralysis.

12319. This includes everything?—Yes.

12320. You have no means of telling us whether the number of severe cases of paralysis has increased—cases in which the limbs are paralysed?—No; the figures are very small, you see, for severe paralysis; they would not be very significant.

12321. Then how was it, as you have been taking immense labour in collecting statistics with regard to the antitoxin treatment, that you were not acquainted with the information that Dr. Wilson gave us from Toronto?—I have no doubt that had I spent more time I could have made out a much stronger case. I could, anyhow, have given much larger numbers of figures. But Dr. Wilson quoted from a report in a provincial town in Canada, and I did not look in those places for it.

12322.—You were not acquainted with it?—No, I was not acquainted with it.

12323. You are not any more acquainted than the Commission are with it; therefore you do not know whether it is exactly comparable?—No.

12324. Quite apart from the statistics, which I do not wish to go into, have you ever met with any doctor who has had a large experience of diphtheria before the introduction of antitoxin, and subsequently to the introduction of antitoxin, who has not thought it of value?—No, I have never met such an one.

12325. (*Colonel Lockwood.*) We were talking about muzzling being a preventive of the disease of rabies in England. Are you aware that in France, especially in the south, muzzling is largely used?—It may be used at the present time.

12326. It has been for ten years?—Has it?

12327. And is very strongly enforced, but rabies still exists very largely, does it not, in France?—I am unable to say offhand to what extent; it varies so much from year to year. It always occurs in epidemics. If you get one rabid dog you may get a hundred; they bite one another. And then it disappears almost entirely from the neighbourhood.

12328. But it does exist in France at the present time?—Yes, it certainly exists.

12329. In reference to the answers that you gave on the question of diphtheria, you are probably aware that the diagnosis of diphtheria in country districts is extremely loose?—I imagine it is not so efficient as it is in the Metropolitan Asylums Board.

12330. Are not the medical officers of health paid so much for every case of diphtheria that they detect? Perhaps you do not know?—I do not know whether they have a notification fee now or not. If so, it would be such a very small amount that I do not imagine that it would tempt even the most impecunious medical man to increase the rate. (*Note.*—Colonel Lockwood was presumably referring to the Medical practitioner and not to the Medical Officer of Health, and I answered the question in that sense.)

12331. You are aware of the very strong feeling existing in a large section of the public against the employment of experiments on living animals?—I am.

12332. To what would you attribute that feeling?—I think it is the natural instinct of a decent-minded man; and it has to be reasoned out of him.

12333. Do you attribute it entirely to sentiment?—I attribute it to the natural disinclination that any right-minded person has to inconvenience any animal unnecessarily.

12334. But do not the medical profession, for whom I have the greatest respect, equally share in that opinion?—I think the medical profession share equally in the sentiment which I have just expressed; but they understand the usefulness and the necessity for inconveniencing animals and utilising them for our own ends.

12335. You think that the reason why there is no repugnance, if that is the right expression to use, among the medical profession to experiment upon living animals is due to a wish to benefit human nature, and that their natural sentiment is not deadened by continual experimentation?—I think not at all, any more than the sympathy of a medical man for children

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in general is any less fine because he has to cause pain to children in the ordinary course of his occupation.

12336. You have told us in your evidence how valuable you think these experiments have been to science. Do you consider that science has suffered at all since the regulations existing at the present time have been imposed?—I do not think materially. I think a certain number of people may have wasted some of their time in correspondence; but I think no material hindrance has been imposed.

12337. Or do you think that our scientific men are less advanced than their brethren on the continent in consequence of those regulations?—No.

12338. When you answered the question with regard to the kindness of scientific men abroad in welcoming their *confrères* in their laboratories, and that we did not allow them in ours, there are some cases, are there not, of foreigners practising in England, what we may call roughly vivisection?—I believe not at the present time.

12339. There have been?—Yes, there have been. I was referring to the recent action of the Home Office, within the last few months, in refusing a licence to foreigners who come to work in our laboratories.

12340. But that only refers to the last few months?—Only to the last few months.

12341. Do you think that there are any extra measures of protection to animals that would tend to allay the state of public opinion, which, I am sure, you dislike as much as anybody? Could you not suggest to us any measure that would tend to allay that alarm that some of the public feel, and which, to the medical profession would not be repugnant?—I am unable to suggest any offhand, but I will think over the matter, and if I have any fertile suggestion to make, I will make it when I have studied Mr. Coleridge's proposed Bill.

12342. Thank you. That would be very valuable to us all. You have witnessed a good many experiments yourself?—Yes, a very large number.

12343. And in all those experiments have you never witnessed any, what we should call cruelty?—No.

12344. Much suffering?—Not much suffering.

12345. And in England, where the experiments you have witnessed are under licence, has the anaesthesia always been complete?—Yes, always.

12346. And the law strictly carried out?—And the law strictly carried out.

12347. And the carrying out of that law assisted and aided by all present?—Yes.

12348. Have you ever witnessed any levity on the part of the students?—I have had no experience with experiments before students in this country.

12349. But I will take your general answer, that you have never seen any cruelty exhibited, and that under the Act anaesthesia has always been complete?—Yes.

12350. Now, in answer to Question 12056, you stated that you had performed experiments in New South Wales, where there was no vivisection Act, and at 12060, in Melbourne, where there was a certain amount of restriction on experiments on animals?—That is so.

12351. As far as regards the progress of science in those two places, do you think there was any difference?—None whatever. I can take no exception to the form of the licence which I held in Melbourne; I think it was only right and proper.

12352. You see my reason, naturally, for asking. You do not think that the regulations imposed in Melbourne have any deterrent effect upon the advance of science?—None whatever; they were not in the least irksome.

12353. They existed?—They existed.

12354. (Dr. Wilson.) Have you yourself had any experience in the treatment of diphtheria?—Not a great experience.

12355. But you have had some?—Yes.

12356. And do you still adhere to that view which you expressed so strongly at the last sitting of the Commission, that if a patient sickening for diphtheria receives an adequate dose of antitoxin the case will not prove fatal, no matter of what intensity the infection may be?—Most emphatically. And I would point out that when asked by Sir William Collins whether I could supply any data from the figures at Chicago to substantiate that view, I said that I was unable to

do so; but I find on looking through my evidence that those figures had already been supplied. 8,000 cases of bacteriologically verified diphtheria treated with antitoxin by the Chicago Health Department between 1895 and 1905 were cited, in which the mortality of 608 cases treated on the first day was reduced to 0.32 per cent; and just above that table was given the experience of the Brook Hospital of the Metropolitan Asylums Board for the years 1897 to 1902, in which the mortality of all cases treated on the first day was during all those years nil.

12357. (Sir William Collins.) Does the first day mean that the bacillus has been found and the invasion of symptoms, but no membrane in the throat?—It means that the patient is taken into hospital within 24 hours of the discovery of the disease.

12358. What does the discovery of the disease consist of?—It may mean a sore throat, which is certified to by a doctor, but in the majority of cases it would be a bacteriological examination nowadays.

12359. But would there be a false membrane in any or many of the cases on the first day of the disease?—No.

12360. Scarcely any?—Very few.

12361. (Dr. Wilson.) You admit that there are great errors of diagnosis without a false membrane?—No, I do not admit that. I admit that there are great errors in the clinical diagnosis of diphtheria, but I do not think that the errors of bacteriological diagnosis amount to more than 10 per cent. By "bacteriological diagnosis" in this sense I mean the examination of the growth obtained from the throat on one occasion.

12362. (Sir Mackenzie Chalmers.) May I call your attention to the paragraph in Sir James Thornton's pamphlet, "The Principal Claims on behalf of Vivisection," about Pasteur's anti-rabic inoculations, and ask you whether you have any observations that you wish to make upon it?—(After referring.) No, I have really no observations to make upon it. I have no doubt that Professor Billroth and Professor von Frisch may have expressed that opinion in early days. The Pasteurian treatment was not very popular in Germany when it was first introduced, owing to a good many reasons.

12363. There is no date given to Professor Billroth's opinion?—No, but I do not think Professor Billroth would have expressed that opinion recently.

12364. Is there any known or authenticated case in which it has been shown that the anti-rabic inoculation has itself created hydrophobia?—I am not aware of any.

12365. In the case of experiments on animals is rabies ever produced by inoculation in an unbitten animal?—Do you mean that should one take the cord straight out of a rabid animal and inject it under the skin of another animal would it give him rabies?

12366. No; what I meant was this: If you take the actual inoculations used for human beings, do the inoculations used for human beings, if inoculated into an animal, produce rabies?—If the animal is treated as human beings are treated it does not get rabies.

12367. That has been tried and tested?—It was, of course, tried a large number of times before Pasteur ever attempted it upon mankind.

12368. Then with the intensity of the treatment gradually increasing, does the same conclusion apply?—Yes.

12369. I was asking the question in reference to the fact that there certainly has been a fear that by undergoing the Pasteur treatment people may contract rabies?—I think that fear is unfounded. I gather that you wish to know whether there is a possibility, however remote, that by undergoing the Pasteur treatment, especially in its intensified form, a person might get hydrophobia who had not been infected by a dog?

12370. Yes?—There is a possibility, but it is extremely remote.

12371. (Dr. Gaskell.) Is it not the fact that the intensified form of treatment has been practically given up?—No; on the contrary, the intensified form of treatment is much more generally used now than it used to be, especially in Russia; they have had more confidence, and have realised more and more that the danger which Sir Mackenzie Chalmers has referred to is less and less.

12372. (Sir Mackenzie Chalmers.) Should you have any hesitation yourself in recommending anyone who

had been bitten to undergo the treatment?—None whatever.

12373. (*Mr. Ram.*) May I ask you one question with regard to what you said about the use of tuberculin? You said, as I gathered, that it was used, that doubts were cast upon it as to whether it was effective, and now there has been an increase in its use, with a modification of the dose?—That is so.

12374. What led to the re-introduction of it, or the re-use of it?—It was due to methods of ascertaining the action of tuberculin which were discovered by Wright, owing to which he found that the effect of a dose of 1-1000th of what used to be given in the early days was equally efficacious, and that the large doses were rather detrimental.

12375. Is Professor Wright now using it largely?—Extensively; nearly all the hospitals in London seem to me to be using it.

12376. Have you had any experience yourself of the results from its use?—My opinion on that would not be valuable, because I have not followed the cases. I have seen a series of cases one day and another series of cases another day. That is a question for the clinician.

Mr. DUDLEY WILMOT BUXTON, M.D., B.S., M.R.C.P., called in; and Examined.

12383. (*Chairman.*) You are a Doctor of Medicine and Past President of the Society of Anæsthetists?—I am.

12384. And you are Consulting Anæsthetist to the National Hospital for Paralysis and Epilepsy in Queen Square?—Yes.

12385. And you are Anæsthetist and Lecturer on Anæsthetics in University College Hospital?—I am.

12386. You have had a very large experience in the study and administration of anæsthetics?—Yes, very large.

12387. Anæsthetics, I suppose, were already used when you began your studies?—Yes.

12388. But since then it has been necessary to study them very carefully, and in the course of that study to make use of experiments upon animals?—That is so.

12389. We have had a good deal of controversial evidence as to the amount of sensation to pain which there is in animals under certain circumstances, under certain classes of anæsthetics, and under certain strengths of dose in application, and we should be very glad of your assistance in giving us information on those points, if you will. I think in your *précis*, which you have kindly sent us, you go very shortly and clearly into these matters. You say that anæsthesia is marked by progressive effects divided into degrees of narcosis upon the nervous system?—Yes.

12390. Will you explain that to us in your own way, especially with reference to the stages at which sensation of pain is lost, and more particularly with regard to the symptoms which occur during narcosis, and explain how far those may be ascertained to be symptomatic of pain or not?—Sensation in the first degree is *ab initio* deadened; in the second degree it is materially lessened; and in the third degree there is absolutely no sensation of pain. In the first degree I should say the sensation was very much lessened, in the second degree still more lessened, and in the third degree and after absolutely lost.

12391. How do you define the three degrees?—The degrees are roughly defined in this way. The first degree is generally from the commencement of the inhalation until the loss of conscious, voluntary movement; in the second degree there is movement, but it is not controlled by the higher perceptive centres.

12392. Is there sensation to pain in both those degrees?—There is some sensation to pain in the first degree, and less, but still some, in the second. I am now speaking of the induction period. Then the third degree is marked by the loss of all movement. The only movements which can obtain at the end of the second degree (all the degrees verge into one another) and in the third degree, are due to reflex excitation, and are not in any way connected with the higher perceptive centres.

12377. Is it now claimed by Professor Wright that an absolute cure can be effected after there is undoubted tubercle by this treatment?—In the case of some forms of tubercle I believe that Professor Wright holds that opinion strongly.

12378. I think one of the witnesses told us it was being used in the case of tubercle in the bladder, which cannot, of course, be got at. Do you know any such case as that?—Yes, I have seen several cases of tuberclosis of the bladder and urinary apparatus.

12379. Treated in this method?—Treated in this method.

12380. Have you ever known of any case which has been believed to be an absolute cure?—Yes, but whether it was entirely due to tuberculin I am unable to say. I am not in a position to say. It can only be judged by large figures.

12381. (*Sir William Collins.*) Do you assert of your own knowledge that tuberculin cures consumption?—I have not, yet.

12382. I ask: Do you assert of your own knowledge that tuberculin cures consumption?—I am unable to assert that. I think, like all things in medicine, there are limitations to almost all our endeavours.

12393. Are they accompanied by any sensation of pain?—Absolutely none.

12394. Is that third degree what is sometimes called surgical anæsthesia?—Yes.

12395. Does that denote the amount of anæsthesia which any surgeon would insist upon in performing a serious painful operation?—Yes.

12396. To take a very familiar example, if you were administering an anæsthetic to a patient who was going to have a tooth extracted, would you go to the second degree or the third degree?—To the third.

12397. Whatever anæsthetic you are using, I suppose the third degree would have to be reached?—That is so. It is more difficult to discriminate the degrees under nitrous oxide (or laughing gas) than it is with chloroform, because the whole thing is so quick.

12398. But that is what you aim at?—You aim at complete anæsthesia, the third degree.

12399. You do not signal to the operator that he can begin until you consider that you have arrived at that condition?—That is so.

12400. Do you say that third degree would affect even the reflex movements?—Some of the reflex movements will persist, but not the superficial ones.

12401. Would you give us a precise definition of what you include in reflex movements. We have had the phrase used very often, but I do not know that anyone has precisely defined it?—A reflex movement, as I understand it, is a movement which is associated with stimulation of the nerve, the nerve travelling from the periphery, from the skin or the mucous membrane, to the spinal cord.

12402. You call that the afferent nerve?—Yes. It thus communicates with the centre of the spinal cord and then by an efferent, along an efferent nerve, it communicates with the muscle and produces contraction. In the chain of communication the higher perceptive centres are not included.

12403. You mean that that reflex movement travels from a nerve, along a nerve to the spinal cord and back again to the extreme of another nerve?—Yes, the muscular nerve.

12404. But it does not cause sensation to the spinal cord and the brain?—No.

12405. There is no sensation of pain in the brain?—No.

12406. It will produce movement although it does not produce pain?—That is so.

12407. Is that a common incident in operation under anæsthesia?—In certain operations, yes.

12408. Does the amount of movement depend upon the amount of pain which would be given by the operation?—No.

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12409. Does it depend upon the locality of the operation?—It depends upon the locality entirely.

12410. And in some operations, I suppose, you may almost rely upon it that, however satisfactorily you have arrived at the third degree, there will be reflex movements?—That is so.

12411. And that to a certain extent you can judge beforehand whether there will be any?—Yes.

12412. We have had witnesses before us who have described movements which some of them have considered to be reflex movements, while others have suggested that they were movements indicating pain in the animal. Have you seen that evidence?—I think I have read most of the evidence; I have read all that has been published, at any rate.

12413. I am afraid that I have not gone through it in view of your evidence sufficiently to put specific cases to you, but perhaps some other members of the Commission may do. At any rate, what you have said to me is a general account of what reflex movement you may expect without necessarily indicating pain?—Yes.

12414. Are you able to ascertain with any certainty, and if so, will you tell us by what means, when that surgical anaesthesia has been reached?—The sign that one relies upon mainly is the loss of the eye reflex, the conjunctival reflex. If I touch my eye it blinks. In the third degree if I lift the lid and touch the eye there is no blinking, the normal conjunctival reflex.

12415. We have heard, for instance, of corneal reflexes being seen, and they have been referred to as being evidence that pain was felt. What do you say as to that?—I say that it is not true.

12416. You say that that is no indication that pain is felt?—I do.

12417. Have you seen that corneal reflex happen when there has been complete surgical anaesthesia?—Yes, certainly.

12418. Have you said all that you wish to say as to how you ascertain when the third degree is reached?—I gave you the salient sign by which it is usually recognised; one would depend not only upon that but upon various other signs. In watching the muscular system, for example, in most cases the muscular system will show rigidity at first, and having passed through that rigidity will become flaccid. Then one would know by that that the patient had gone through the second and passed into the third degree.

12419. Has a skilled anaesthetist any difficulty whatever in assuring himself that perfect insensibility to pain has been reached?—None at all.

12420. There are plenty of symptoms by which he can make himself quite sure?—Yes.

12421. Does the difficulty of arriving at that stage differ according to the kind of anaesthetic that is used, or do you ascertain it by the same means in the case of every anaesthetic?—No, it differs slightly. For example, in nitrous oxide one would judge by the breathing and not by the eye. In the case of chloroform and ether you judge mainly by the eye.

12422. (Sir Mackenzie Chalmers.) The eye of the patient or your own eye?—The eye of the patient.

12423. (Chairman.) Nitrous oxide is what we call laughing gas?—Yes.

12425. It is only given to human patients?—Yes. except in special researches undertaken to elucidate the physiological action of that agent.

12425. It is only given to human patients?—Yes.

12426. You have to watch also that respiration is not interfered with, have you not?—Yes, very closely.

12427. And the necessity for that differs very much according to the anaesthetic employed?—Yes.

12428. Which are the anaesthetics that most affect the respiration?—Chloroform and all the volatile anaesthetics.

12429. Is it always safe to use chloroform as an anaesthetic?—It depends upon the method which you adopt. In many cases it is not at all safe. If you adopt a regulating method, and know the percentage and the danger of using a high percentage of vapour—if you can keep your percentage of vapour below 2 per cent., you may give chloroform with impunity. If you give above 2 per cent. or give it by a method which does not enable you to regulate your percentage, you are treading upon dangerous ground.

12430. And you can mix chloroform with something else, I suppose, which will diminish the danger to respiration?—That is one way of getting a lower percentage.

12431. I think there has been some confusion in some of the evidence before us as to the use of the term light anaesthesia. It has been spoken of by some witnesses—I do not think by professional witnesses, but others—as if it meant something less than complete anaesthesia. Would you explain what the term means as employed by skilled workers?—We mean by light anaesthesia that the patient has been put through the third degree, has been completely anaesthetised, and then the depth of the anaesthesia has been lessened, so that he has been absolutely anaesthetised, and then we find that by giving a smaller quantity later on he remains absolutely anaesthetised and yet he is actually perhaps in the second degree of anaesthesia for a long operation. It is extremely important in long operations to limit the amount of anaesthetic that is given. Light anaesthesia has, of course, to be clearly distinguished from incomplete anaesthesia, where the patient has never been completely anaesthetised. In one case you have the blood at a certain degree of saturation; in the other case you have an irregular distribution of chloroform or other anaesthetic, and the patient has never been completely anaesthetised; he is always hovering between consciousness and unconsciousness.

12432. But in all operations do I rightly understand that if you are to avoid pain you must begin, at any rate, by bringing the patient to the state of what you call surgical anaesthesia?—Yes, that is so.

12433. If it is a long operation, I understand that you rather lighten the amount of anaesthesia as compared with what you would do if it was a short operation?—That is my meaning.

12434. And when it is so lightened under those circumstances, it is your business as an anaesthetist to see that it is never reduced below the condition in which the patient is free from pain?—Yes.

12435. And can that be done consistently with what you have described as light anaesthesia?—Absolutely.

12436. There is no doubt about it?—None at all.

12437. If you spoke of incomplete anaesthesia, I suppose it would mean that the patient was not necessarily free from pain?—Yes.

12438. You use the terms complete and incomplete with reference to pain?—Yes.

12439. Is there any difference, and, if so, will you explain it to us, in the method of operation of anaesthetics upon man and upon the lower animals?—None.

12440. Do you note their condition of insensibility and the degree of it in the same way with animals as you would with man?—Always.

12441. And are the dangers of particular anaesthetics the same with animals, or do they differ according to different animals?—I should say that the danger was practically the same. Animals perhaps die more frequently because they may not have been so carefully watched.

12442. That is to say, they may not have been so carefully watched as regards the amount of stress put upon them?—Yes, the amount of dosage employed.

12443. But if they die of excessive anaesthesia, that of course does not involve pain?—No.

12444. It may involve death, but not pain?—That is so; it is a negation of pain.

12445. Are animals subject to the same stress upon respiration as mankind are?—Yes.

12446. And do they die in the same way that human beings would if they had too much anaesthetic?—Yes.

12447. Will you tell us what are the general anaesthetics that are used in surgical practice?—Nitrous oxide, chloroform, and ether are very common. Chloride of methyl is used occasionally, chloride of ethyl more frequently; bromide of ethyl has been used, and scopolamine; and for local anaesthesia strovaine, eucaine, cocaine, and others of that nature.

12448. Morphin?—Morphin is seldom used in the ordinary sense of an anaesthetic by itself. It has been used largely with chloroform and with ether, but it is not very commonly used now owing to its action upon the respiratory centres. In working with a volatile anaesthetic you limit within a short time the effect of

your drug. Having once injected your morphin, you lose control of the action of your drug, and its action may go on, and your patient may succumb; you have no means of preventing it. You can remove your chloroform; you cannot remove your morphin.

12449. Are morphia and morphin the same?—Yes.

12450. Are morphin and chloroform used together?—They have been used together; they are less used now than formerly.

12451. And chloroform and ether are used together?—Yes, they are, as a mixture. And the A.C.E. mixture is used, which was invented by Harley. Then alcohol and chloroform, the mixture 1 in 10, invented by Professor Schäfer, has been recently used, and a mixture of chloroform and ether which has also been introduced is largely used, and a very large number of mixtures containing different proportions of ether and chloroform. Petroleum ether was used in a mixture or solution with sulphuric ether and chloroform some years ago. The number of mixtures is very large.

12452. Are these mixtures that you make use of governed by the length of the operation or the particular condition of the patient, or the particular choice and fancy of the operator?—They are largely governed, I think, by the choice of the anaesthetist. He would, of course, be regulated by the necessity of the operation and the patient; he would examine his patient, and he would be familiar with the requirements of the operation, and he would make his choice upon that.

12453. Then, besides those anaesthetics which you have mentioned, are there other drugs which are not generally spoken of as anaesthetics, but are soporifics, which, however, will be anaesthetic in their operation if enough of them is used?—Yes.

12454. Is there any advantage in using a strong dose of an ordinary soporific instead of an anaesthetic?—None as a rule in surgery.

12455. You generally use an anaesthetic if you want to operate?—Practically always.

12456. There is no object in using a soporific?—None whatever.

12457. Have the use and the action of anaesthetics been ascertained largely by means of experiments upon living animals?—Certainly.

12458. By testing them upon animals?—Yes.

12459. I think you have already explained to us that if you apply too strong a dose to an animal you may kill it, but you kill it without pain?—Yes.

12460. Therefore, you are not so afraid of testing it strongly upon an animal as you would be, naturally, of testing it upon a human being?—That is so.

12461. (Colonel Lockwood.) Have you witnessed many experiments upon living animals under the Act?—A large number.

12462. Is the same care taken as to the degree of anaesthesia of which you have been speaking in the case of animals which are undergoing severe operations as in the case of human beings?—I should say certainly.

12463. And you invariably wait for the same degree of insensibility?—I always do.

12464. And the various precautions to insure that degree being reached which are used with reference to human beings you have always, so far as you are acquainted, seen used for animals?—Yes, you would adopt the same criteria.

12465. That you have always adopted?—Yes.

12466. That is to say, the anaesthesia has always been complete before these severe operations which are performed under licence are begun?—Certainly.

12467. And where, under the licence, the animal is required to be killed before recovery from anaesthesia takes place, that has always been carried out?—Yes, in my experience.

12468. I mean so far as your experience goes?—That is so.

12469. You have never witnessed anything to the contrary?—No.

12470. How many kinds of anaesthetics are used, or is it always the ordinary A.C.E. mixture that is used for animals?—No; it differs a great deal. Sometimes the A.C.E. is used, sometimes chloroform, sometimes ether.

12471. But always chloroform in some form or another?—Sometimes chloroform, sometimes ether.

12472. But always one of those two or the mixture?—Yes.

12473. Not opium, we will say?—Morphia has been used—not in my own experience.

12474. In your opinion is morphia a complete anaesthetic?—Quite.

12475. For severe operations?—Certainly.

12476. No pain would be present?—Not if you had a sufficient dose of it.

12477. Are you acquainted with the general provisions of the Act under which these experiments are carried out?—I am.

12478. Are you of opinion that they sufficiently guard animals from suffering?—Yes, I think that occasionally animals are unnecessarily killed.

12479. You mean under the provisions of the Act?—Yes.

12480. You think that they might safely be allowed to recover?—Certainly.

12481. That instead of adding extra precautions to those under the Act you would be in favour of relaxing some of them?—In that sense, certainly. I think that animals are sometimes extinguished when they might live long and have a happy life.

12482. You mean rather a happy life in the cause of science, not for their comfort?—Possibly for their own comfort.

12483. But you are talking, I presume, more of the scientific life?—Perhaps it is more a side issue, but I mean to say that in cases where an operation has been performed, it not being one to give the animal inconvenience afterwards, it seems a pity that it should be killed.

12484. Therefore, you are not prepared to assist us in any way by suggesting any further restrictions?—No.

12485. You think that the Act goes plenty far enough as regards the animals and the prevention of cruelty on the part of operators?—I think so. I think the well-being of the animals is quite safeguarded.

12486. You have never seen any cruelty in any work that you have witnessed?—Never.

12487. Nor have you seen an animal struggling or crying?—I have seen reflex movements. I have never seen conscious movements.

12488. (Sir William Church.) Have you had much experience in giving anaesthetics and seeing anaesthetics administered to animals other than man?—I have actually done it very many times to animals.

12489. We have been told by several witnesses that dogs are particularly difficult to keep under anaesthetics. Is that your experience?—Certainly not.

12490. There is no difficulty in keeping a dog for several hours under anaesthetics?—Not if you understand your business.

12491. We have also been told that it requires a very skilled anaesthetist to anaesthetise a dog. Is that so?—That would depend entirely upon the apparatus employed.

12492. Perhaps you would describe to the Commission the ordinary way in which a dog is anaesthetised for physiological purposes?—There, again, that must depend very largely upon the laboratory and upon what the purpose of the operation is. I should say that the most common method was by means of a Woulf bottle, allowing the air to pass over the surface of the chloroform.

12493. That would be a case in which the trachea is opened for a very prolonged experiment?—Yes.

12494. What is the ordinary way of anaesthetising a dog in a physiological laboratory?—By means of tow or lint with the anaesthetic poured upon it.

12495. Either by means of a mask, the animal being held under the mask, or by placing the dog in a basket with the anaesthetic?—Yes.

12496. And in your opinion is any special skill or knowledge required for anaesthetising a dog in order to produce anaesthesia?—No, I think not. Of course, an expert person may produce an overdose by giving too much. I do not think he is likely to err on the other side.

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12497. And in the case of those prolonged experiments in which it is necessary to administer an anæsthetic through a tracheotomy tube, do you think that there is any difficulty in regulating the dose of the anæsthetic?—None whatever.

12498. It is quite easy to regulate the dose of the anæsthetic in proportion to the air supplied to the animal?—Quite.

12499. Therefore, if the dose of anæsthetic was insufficient, the dose of air would be insufficient, and the animal would die?—That is so.

12500. That is your opinion?—That is my opinion.

12501. Have you any experience of administering morphia to dogs before you commence or as soon as you commence administering the chloroform, giving the two together?—I have no personal experience.

12502. Perhaps, then, you would not like to state before the Commission whether or not it is true that subsequent to the injection of morphia a lighter and less amount of chloroform is required?—I can certainly state that, because I have frequently employed morphia in the human subject before other drugs, such as chloroform or ether or scopolamine, and I have always found the amount of general anæsthetic required extremely small when morphia is given beforehand. The same, of course, would obtain in the case of the lower animals.

12503. Is it a common practice in human patients to administer a small dose of morphia subsequently to the chloroform?—Yes, it is very common.

12504. The purpose of it being what?—To keep the patient quiet and to prevent their having any subsequent pain, and to ensure sleep and rest.

12505. Just to keep them quiet?—Yes.

12506. Can you tell the Commission anything about the action of morphia upon dogs apart from chloroform?—I do not think, really, my evidence would be of much value upon the point.

12507. Have you any experience of administering opium, in the form of tincture of opium, to dogs?—No.

12508. You would not like to say what would be the effect of injecting a drachm of opium into the veins of a dog?—No.

12509. Would it be subject to pain?—No.

12510. Taking your division of the effects of chloroform into three degrees?—I only got so far as the third; there are five degrees really.

12511. I think you said the first degree was until loss of control of voluntary movement?—Yes.

12512. The second degree was when the movements were not controlled voluntarily?—Yes.

12513. And the third degree was that of complete surgical anæsthesia?—Yes.

12514. In labour cases chloroform is frequently used, is it not?—Yes.

12515. Which degree of anæsthesia do you produce then?—The second, for alleviation of pain; not, of course, in a midwifery case necessitating a surgical operation.

12516. I am speaking of natural labour, of course, not labour in which surgical aid has to be rendered?—That would be the second degree. If any operation has to be performed it would be the third degree.

12517. We have heard a great deal of the value of different terms here which I think have been used without, perhaps, due consideration. You would consider incomplete anæsthesia any form in which voluntary movement remains?—Yes.

12518. But would you consider it a condition of incomplete anæsthesia in which uncontrolled movements occurred?—Oh, no.

12519. Is incomplete anæsthesia a term that is used often by anæsthetists?—Yes, I think so.

12520. And would it, in your mind, be equivalent to the term which has also been used before us of light anæsthesia?—No.

12521. It would be something less than light anæsthesia?—Yes, in a way it is quite a different thing. Light anæsthesia is complete anæsthesia; light narcosis is complete anæsthesia.

12522. You would include light anæsthesia under complete anæsthesia?—Yes.

12523. It has also been put before us by a witness here that, short of the abolition of the corneal reflex, you cannot have anæsthesia. Is that so, in your opinion?—I do not admit that.

12524. Is that belief held by any competent chloroformist?—I believe not.

12525. Reflex movements, which are of very different natures, can be called forth, can they not? Reflex movements of various sorts can be obtained in very different stages of anæsthesia?—That is so.

12526. That is to say, there are certain parts of the body which, when stimulated, produce reflex movements in what one might call a deep state of anæsthesia?—Certainly.

12527. That is in a state of anæsthesia so complete that all muscular action almost is lost?—All voluntary muscular action?

12528. I meant all voluntary muscular action?—Yes.

12529. Is so deep an anæsthesia as that a condition which is often called for in surgical operations on man?—I mean, absolute abolition of all movement?—No, you cannot do that unless you kill your patient.

12530. But it is often done, is it not, in physiological operations upon animals for the sake of abolition of all movement?—If you have artificial respiration carried on, of course.

12531. Therefore, the fact that occasionally reflex movements may be seen in animals which are being operated upon by physiologists or research workers is no proof that they are not completely anæsthetic, although it is a proof that they are not in the profoundest condition of anæsthesia?—That is so.

12532. (Sir William Collins.) Do you hold a vivisection licence yourself?—Yes.

12533. Have you held it for long?—For a good many years.

12534. What form of licence is it?—The ordinary form of licence.

12535. You have no certificate?—No certificate.

12536. You hold it with a view to investigating the action of anæsthetics?—Quite so.

12537. And no pain would occur in any of your experiments?—None.

12538. Are there still some problems in connection with anæsthetics which require elucidation?—I am sorry to say there are.

12539. Should I be right in thinking that the discovery of anæsthetics was not due to experiments on animals?—The actual discovery of anæsthesia was not. Our knowledge of the precise action of anæsthesia is entirely due to such experiments.

12540. Is our knowledge of the precise action of anæsthesia complete to-day?—Not complete. Our knowledge of very few subjects is complete.

12541. Have you witnessed any surgical operation of any magnitude upon a human subject under morphia as the only anæsthetic?—I have never seen any.

12542. Would you advise it?—Certainly not.

12543. You have used, and other witnesses have used the terms, apparently discriminating between them, of light anæsthesia, incomplete anæsthesia, and complete anæsthesia?—Yes.

12544. Would it be troubling you too much if I asked you to give us a definition of each of those three terms now?—Complete anæsthesia is that degree of narcosis where there is no pain felt. Incomplete anæsthesia is that degree of narcosis where pain is felt. Light anæsthesia is complete anæsthesia. You have complete anæsthesia in profound narcosis; or you may have complete anæsthesia with light narcosis. That is as near as I can get to a definition.

12545. So far as I can gather, the appeal in each case is to pain as the differentia?—Yes.

12546. Can you suggest any other test, not subjective, but objective, by which the three varieties could be discriminated?—I have already mentioned various ocular phenomena that guide one as to whether there is pain or not pain, which would direct the anæsthetist's attention to the presence or absence of pain—the ocular phenomena and purposive movement.

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12547. Is the conjunctival reflex present in complete anæsthesia?—Conjunctival reflex is absent. It may be very sluggish in very light anæsthesia; it is not brisk. In incomplete anæsthesia it will be fairly brisk.

12548. Then there is a stage of anæsthesia, I understand from you, in which the corneal reflex is present, and even brisk, and yet there is insensibility to pain?—By the corneal reflex, do I rightly understand you to mean the conjunctival reflex?

12549. I meant to use the conjunctival reflex all through, if you make a distinction?—I do.

12550. Will you tell us what the distinction is?—If you touch the conjunctiva you may get blinking. If you produce the condition of narcosis you get very little, hardly any movement of the eye, on touching the conjunctiva. If you touch the cornea you do get movement.

12551. Strictly speaking, the conjunctiva extends over the cornea, does it not?—Yes, I am speaking of pressing upon the border of the eye lid.

12552. Do I rightly understand that there is a period, in your opinion, in which the corneal and conjunctival reflexes are present, and yet there is insensibility to pain?—The patient, having been in complete anæsthesia, the normal conjunctival reflex may, during the continuance of the operation, be known to pass into a condition where there is slight reaction to touching the conjunctiva, and yet be absolutely free from pain.

12553. Is that condition one of a narrow margin which it is difficult to maintain?—Fairly narrow.

12554. Then in regard to the induction period, as you happily termed it, should I be wrong in thinking that the same period might occur—that is to say, absence of pain with presence of corneal and conjunctival reflexes?—I take it that that just in passing from the second to the third degree, when the conjunctival reflex is very sluggish, when the eyeball is almost fixed, there follows a period of absolute freedom from pain.

12555. Would that be a wider or a narrower margin than the period subsequent to induction?—Narrower.

12556. And more difficult, therefore, to maintain?—Yes.

12557. Have you studied the influence of morphia upon different varieties of the lower animals?—No.

12558. Should I be right in thinking that it differs in its action in the frog from its action in the dog?—No, I do not think so.

12559. Have you studied the physiological influence of curare at all as to whether it be an anæsthetic or not?—Only as a reader of literature. I have never worked with curare.

12560. Do you care to give the Commission your opinion on the subject?—Certainly, if they wish it. I can only go by what I have read. I know there is a case reported of curare poisoning, where the patient expressed himself as not having been conscious of any pain while the surgical operation was being performed. That is the case of Mr. Joseph White, of Nottingham.

12561. Do you conclude from that that curare is an anæsthetic?—That is my impression. I believe the general consensus of opinion is that it is an anæsthetic.

12562. Was that the opinion of Claude Bernard?—No; but a good deal of water has flown under London Bridge since Claude Bernard's day. In fact, we know more about it.

12563. Do you think the restriction in the Act which is at present operative, whereby curare is not held to be an anæsthetic, is erroneous?—No; I think if there is any doubt it is better to have the restriction.

12564. Do you think there is a doubt?—I think there is doubt in the minds of some people.

12565. Have you any doubt in your own mind?—I have an open mind upon the question. I do not know enough about it.

12566. Is the same apparatus used for giving anæsthetics to the lower animals as is employed for man?—What apparatus are you referring to?

12567. You referred to the Wolff's bottle?—We have a very similar apparatus, the Junker apparatus. The principle is very much the same; you pump air through.

12568. (Sir Mackenzie Chalmers.) You told Sir

William Church just now that you would not recommend morphia as an anæsthetic in the case of human beings. Is that because you doubt its anæsthetic power, or because it has other deleterious effects?—Because it has other deleterious effects.

12569. And in a sufficient dose to produce anæsthesia would it be dangerous to life?—I think so.

12570. When you have been experimenting on animals with anæsthetics, what is the nature of the operations you have had to employ?—Taking blood pressures, examining the action of the heart, and examining the action of respiration.

12571. In all your experiments the animal is killed before it recovers consciousness?—Yes.

12572. Have you seen other people as well as yourself experiment?—Yes.

12573. You said, in reply to Colonel Lockwood, that you had never seen any cruelty. May I put it rather more fully, and ask you: Have you ever seen any carelessness in relation to experiments on animals which has tended to increase suffering?—No, I cannot say that I ever have.

12574. So far as your own personal experience goes, the experiments have always been carefully carried out, and the anæsthesia has been carefully maintained?—Yes.

12575. Do I rightly understand, as regards the administration of general anæsthetics like chloroform, that the sense of pain from the beginning of the administration is gradually dulled, or does it go by stages?—It is gradually dulled. The division into degrees is an artificial one. There is a progressive downward curve of loss of sensation.

12576. So that, with what you would call incomplete anæsthesia, you would have a dull sense of pain, but still some sense of pain?—In some particular animals you might have no sensation; in other more sensitive animals you might have considerable sensation.

12577. Has your attention been called at all to some experiments that were performed by Dr. Crile on surgical shock?—I am aware of Crile's experiments in book form.

12578. May I put this to you? Dr. Crile in his work on surgical shock mentions certain experiments which he performed under what he called incomplete anæsthesia. He was then asked whether those animals suffered any pain or not, and his reply was: "Certainly not; they suffered no pain," although the term that he used was incomplete anæsthesia. What should you say to that?—I should think that probably he used the expression, incomplete anæsthesia, somewhat loosely.

12579. In the sense in which you would use light anæsthesia?—Yes, I should imagine so. Even that would depend very much upon how near complete anæsthesia his incomplete anæsthesia was. He probably meant that he had not got the condition where the animal had completely lost the conjunctival reflex and the eyeball had not become fixed, and so on.

12580. But, nevertheless, it might have reached the stage that you call complete surgical anæsthesia?—Yes, but not if it was incomplete anæsthesia.

12581. He called it incomplete anæsthesia, but said that the animal suffered no pain. Could those statements be reconciled if he was using the term in the same sense that you do?—I do not think he could.

12582. You think he was using the term incomplete anæsthesia (if his statement is correct that the animal suffered no pain) in a different sense from that in which you use it?—Yes.

12583. You told us that you had read the evidence already given before the Commission, I think?—I have read the two Blue Books which have been published.

12584. Are there any comments that you wish to make on any part of that evidence?—I merely came here prepared to answer questions.

12585. Have you seen the evidence of Miss Lind-af-Hageby?—I was reading it while I was waiting to come in here. Yes, I have read some of it.

12586. Where were your experiments performed—at what laboratory?—University College, London.

12587. The same building in which she witnessed certain experiments?—Yes.

12588. Have you read her account in the "Shambles

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of Science" or in her evidence here of certain movements and cries which she perceived in animals that were being used for demonstration?—I read it in the evidence before this Commission.

12589. Can you form any opinion as to whether they were merely reflex movements, which she misinterpreted, or whether the animals were actually incompletely anaesthetised?—The impression left upon my mind is that she had mistaken reflex movements for movements due to sensation of pain.

12590. In the case of an unskilled person seeing demonstrations of that kind would it be easy to mistake reflex movements for purposive movements, accompanied by pain?—Extremely easy. I have known surgeons to mistake reflex movements for movements of pain.

12591. In ordinary surgical operations, do you mean?—Yes.

12592. You have administered anaesthetics in a great many thousand cases, have you not?—Very many thousands.

12593. In any considerable proportion of cases do you get either movements, or even cries, from a patient that you are satisfied is insensible to pain?—According to the area of the operation you certainly get reflex movements not infrequently. You get cries from patients quite infrequently, but it does occur. One can recognise them by the character of the sound—the absolute difference from articulate speech.

12594. It would only be a skilled person who would recognise the distinction between such cries and a cry of pain?—Yes.

12595. I suppose it is a nervous action?—It is a rush of air through the glottis. I can quite understand a person not familiar with the thing being taken in by it.

12596. Would you get such a cry in the same cases in which you get strong reflex movement, or would they be different cases?—Quite similar cases.

12597. Would it be specially in rectum surgery?—It is very common in rectum surgery. You may get an ascending action of the diaphragm, the air forced out by the diaphragm, when you get what is spoken of as the rectum crowing; the patient makes a curious crowing sound.

12598. Were any of these experiments which Miss Hageby describes such as would be likely to give rise to reflex movements?—I am afraid that I do not remember what the particular experiments were.

12599. (Dr. Gaskell.) Have you any definite opinion as to where these different anaesthetics act—on what part of the central nervous system?—I take it that they act upon the whole central nervous system.

12600. Do they act equally, do you think, on the whole central nervous system?—In the case of the general anaesthetics, chloroform, ether, and such like things, I should think their action was one gradually extending from the higher centres downwards if you push it far enough.

12601. That is to say, in your opinion the anaesthetic acts on the higher centres first?—Yes.

12602. Most thoroughly, I mean?—Yes.

12603. The highest centres of all you call the cerebral hemispheres?—Yes.

12604. Would you consider that the anaesthetic action of the drug affected the cells in the cerebral hemispheres distinctly earlier than those in the spinal cord?—I have no doubt of it.

12605. Have you a definite opinion at all where pain is located in the central nervous system?—The perception of pain is in the higher centres.

12606. Do you think that an animal or person deprived of the cerebral hemispheres can feel pain?—No.

12607. You feel sure, for instance, that there would be no pain felt in the spinal cord?—Absolutely none.

12608. When you spoke of reflex action to Lord Selby you mentioned only the spinal cord. I presume that you would allow that reflex actions of the same kind would exist above the spinal cord?—Of course. I only referred to spinal cord reflexes by way of explanation.

12609. So that if the cerebral hemisphere were removed you might still get by reflex action cries?—Yes.

12610. And movements apparently of pain?—Certainly.

12611. Still, in your opinion, there would be no pain?—No.

12612. Do you think that it is very difficult to distinguish between an animal which is making struggles owing to feeling pain, and an animal which is making struggles through these lower centres reflexively. Does it require experience to be able to distinguish that?—Distinctly, I think so.

12613. But, in your own experience of vivisection experiments, you have never felt any difficulty in distinguishing that in an animal?—None of my own knowledge.

12614. For instance, a cry would not be sufficient to enable you to judge whether there was pain?—Certainly not, neither cry nor movement.

12615. Then with respect to such a light anaesthetic or anaesthesia as is given by laughing gas, is it not the experience in giving laughing gas for a dentist that the patient cries often out and struggles?—Very commonly.

12616. And even goes so far as to hit the dentist in the eye?—Yes, I have known it happen.

12617. So have I. Is that a sign of pain?—No.

12618. On seeing that yourself, would you be able to say that the patient was not suffering pain?—Certainly.

12619. And the patient afterwards says he has felt nothing?—Yes.

12620. Have you ever seen any cases of complete consciousness under an anaesthetic, and yet no pain?—I never have; they have been recorded, but I have never seen it.

12621. I once saw Sir George Murray Humphrey give bichloride of methylene to a patient in a hospital, who came round, and I chatted with him during the whole time of the operation. You have never seen a case like that?—No. I have seen cases recorded; I have never seen them myself.

12622. May I take it that in your opinion most definitely a comparatively small, what you would call not a very deep anaesthesia is requisite, and yet you may be sure that the animal does not suffer pain, pain goes first?—Yes.

12623. It goes very early?—Yes, certainly.

12624. You spoke of being able to regulate with certainty the dose of anaesthetics by means of Woullf's bottle, the respiration of it?—Yes.

12625. Could you feel certain that even if that animal had curare by the same method of regulation of the bottle, that animal was all the time completely anaesthetised, using chloroform and ether?—Certainly, with the air passing through, I say so.

12626. You know the case of Bayliss v. Coleridge?—Yes.

12627. In your opinion in that case was there any doubt whatever about the animal being completely anaesthetised?—I have no doubt whatever.

12628. And there is no reason for us to take it, that that is a mode which would give no certainty that the animal was not suffering pain?—Certainly not.

12629. There is only one other question I want to ask you. Have you any experience of urethane?—None.

12630. You cannot give us any advice upon that?—I am sorry I cannot.

12631. (Sir Mackenzie Chalmers.) Have you ever experimented yourself with a local anaesthetic on an animal?—Not on a lower animal. I have experimented, that is to say, I have employed it on man.

12632. At present local anaesthetics are not used. They certainly would not be authorised under licences for experiments on the lower animals?—I do not think they are. I have no doubt that local anaesthetics do produce complete loss of pain.

12633. What sort of severe operations have you seen done without pain by means of local anaesthetics?—The removal of breast, tumours of breasts, the radical cure for hernia, removal of testicle, removal of lower parts of the rectum, and amputation of fingers.

12634. Under local anaesthesia?—Yes, you inject it.

12635. By cocaine or eucaïne?—Or stovaine.

12636. Is that a new anaesthetic?—Yes.

12637. Have you actually discussed with a patient how far there was no sensation of pain?—Yes.

12638. Or dull sensation?—Yes.

12639. What has been the result?—In most cases they have felt no pain at all. Occasionally they have experienced discomfort.

12640. (*Dr. Gaskell.*) Do you consider that the pulse gives you any indication whatever as to whether the anæsthetic is sufficiently deep to prevent pain?—No.

12641. You could not judge by that at all?—No.

12642. (*Mr. Tomkinson.*) With regard to morphia, morphia, I suppose, was a substance well known, and was discovered long before chloroform?—Yes, opium was known.

12643. It was never used, was it, as an anæsthetic?—Opium was used.

12644. But not to any material extent for deadening pain, was it, in operations?—I believe so. I believe that in the pre-anæsthetic days opium was largely used.

12645. In operations?—In operations, to lessen the sensation of pain.

12646. I thought I had often seen descriptions of the terrors of the operating room?—I daresay. It was not universally used.

12647. I mean, when entirely relieved by any anæsthetic, and consequently the wonderful blessing to mankind of the discovery of chloroform as a real anæsthetic?—I never said it was universally used. I believe it was used in some cases.

12648. Do you hold that persons under the influence of morphia, or insensible, say, from alcohol—that is to say, insensible in the way of taking care of themselves, apparently in a profound sleep—are not subject to pain if they are cut?—I have no doubt of it, that they are not sensible to pain.

12649. You say that the value of anæsthetics has been discovered by experiments on living animals?—The properties of anæsthetics, I said.

12650. How is that specially so, more than experience of the use of it for human beings. You have to rely upon what human beings tell you as to their insensibility to pain afterwards surely, more than anything an animal can tell you?—You can examine the patients' hearts, you see; you cannot open their chests and examine the action of chloroform on the heart, nor can you examine their brain, nor can you examine the spinal cord. With an animal you can discover the action of chloroform or of ether, and you can tell what dose is dangerous, and what dose is not dangerous—whether it affects respiration or whether it affects circulation or other things.

12651. In physiological experiments on dogs, of which you have seen a great many, they are all strapped down first of all, are they not, before the chloroform is administered?—No, certainly not.

12652. Is there not some difficulty in administering it? The dogs struggle against it?—They are in a box; you can put them into a box.

12653. The dog struggles, I presume, to get out of the box?—No, it generally lies down and goes to sleep.

12654. Not at first. In the early stages does it not struggle or bite the box?—As a rule, my experience is that the dog turns round once or twice and goes to sleep.

12655. And then is it strapped down?—Yes, when it is unconscious, and tied on a board.

12656. That, you say, is to prevent reflex action?—No, I did not say anything of the sort.

12657. What is the object?—To keep them in the position required for the operation. In general anæsthesia for man, we strap the man up for operating on his anus. We do not do it to keep him still. We want him in a particular position there, therefore we tie him there, the same as with the dog.

12658. I forget whether you said that you had often seen or administered curare?—I have not.

12659. Do you approve of it?—In what sense do you mean?

12660. Do you think there is danger in it as a paralyzing element, and thereby to some extent depriving the animal of the power of showing consciousness in

the event of sensibility returning?—I do not think there is any danger, because it is always employed with chloroform or morphia—with some competent anæsthetic. I should not employ it myself alone, because I am not sufficiently satisfied of its action.

12661. And in your experience it has not been necessary; you have hardly ever used it?—That is so.

12662. Allusion has been made to Dr. Crile's experiments; you have read of them, I suppose?—Yes, I have.

12663. They were of a tremendous nature, many of them?—Yes.

12664. Do you think there was not danger, in such shocks as that being administered to an animal, of pain being felt, even through the anæsthetic?—None. I think an anæsthetic would be absolutely of no use if you could not safeguard human beings and animals from the possibility of any shock, however severe, making itself felt through the anæsthesia; it would cease to be an anæsthetic. An anæsthetic, to be an anæsthetic, must be capable of preventing pain of any character or kind. By shock, you mean sensation of pain.

12665. In at least one of those operations it was admitted that the dog did show signs of feeling and of pain?—Then it did not have enough anæsthetic.

12666. Exactly; the anæsthesia was imperfect. I think a burner was applied to the dog's nose; it had attempted to struggle as far as possible, and it did show distinct signs of consciousness and pain. You have seen experiments on frogs, have you not?—Many.

12667. Are they anæsthetised?—They are pithed.

12668. Always?—Certainly.

12669. Does that mean the severance of the brain from the body?—Yes, from the cord.

12670. And does it absolutely preclude sensation?—Absolutely.

12671. Is that the case with other cold-blooded animals, too, which are operated upon?—If you destroy the higher brain they no longer can feel pain.

12672. Is pithing resorted to in the case of all cold-blooded animals?—The only cold-blooded animals I have pithed have been frogs.

12673. They are the only cold-blooded animals on which you have seen operations?—I think I may say so.

12674. (*Dr. Wilson.*) But even with pithed frogs it has been contended here that they do exhibit purposive movements. For example, they can reproduce the species—so it has been stated by witnesses. Would that modify your opinion as to whether all possibility of pain is banished by pithing?—Not in the slightest.

12675. Then with regard to these prolonged operations on human beings under chloroform, when slight anæsthesia is maintained towards the close of the operation, have you not to be very careful about varying the dose?—You have always to be careful in giving chloroform to a human being.

12676. You could not keep up a continuous dose as you would through Woullf's bottle?—Certainly you can.

12677. With safety you can do that?—Yes, certainly.

12678. In anæsthetising animals, no matter how severe or prolonged the operation, do you think there is no difficulty at all in maintaining absolute anæsthesia throughout?—I have no doubt of it whatever.

12679. But can there be absolute assurance that there is no pain?—I have no doubt whatever.

12680. In those deaths which sometimes take place during severe operations on the human subject is it not sometimes difficult to tell whether death is due to the anæsthetic or to the shock of the operation while the patient is on the operating table under a severe operation?—You have to decide that upon the merits of each case really. I do not think I can answer generally. I might go so far as to say that I should think you can soon tell. You really have to decide the case upon its own merits.

12681. You believe complete narcosis, of course, is complete anæsthesia, that with narcotics you can induce complete anæsthesia?—Yes, I do.

12682. You have been asked about morphia, and you stated about eucaïne, and also urethane; have you any experience with regard to paraldehyde as an anæsthetic?—None.

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Mr. D. W. Buxton M.D., B.S., M.R.C.P. 12683. Do you think it could be used for anæsthetising animals?—I have no idea. I have no knowledge of it.

17 July 1907. 12684. Have you ever anæsthetised animals for operations on account of disease or accident?—Yes, I have.

12685. And do you observe the same rules then as are applicable to human beings?—Yes, quite.

12686. But not exactly the same rules as are followed in a physiological laboratory?—Probably it is done in a more formal manner. I spoke about a dog being put into a box. On occasions when I have had to chloroform a valuable dog, or the dog of a friend, I have not put it into a box; I have put it on a table, patted it, and given it chloroform, which it has inhaled, and gone off without any sign of discomfort or fear.

12687. You would not think it necessary to use

Woullf's bottle?—I should think it was very convenient to use a Woullf's bottle, but not necessary.

12688. (*Sir William Church.*) I should like to ask you one question, because I think, perhaps, some of the public and some of the Commission may not put a right interpretation upon the illustrations which show the boards that animals are strapped upon for some of these operations. That is to keep them not quiet, but to keep them in a special position in order that the necessary operation can be performed, is it not?—Quite.

12689. It is equivalent to the fact that even still, for a large number of operations for men, it is found that it is advisable to strap the patient into a certain position before the operation begins?—That is so.

12690. Then from your experience you tell us that that is a necessity which very frequently arises in the practice of human surgery?—Certainly.

TWENTY-EIGHTH DAY.

Wednesday, 24th July 1907.

PRESENT :

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel the Right Hon. A. M. LOCKWOOD, C.V.O., M.P.

Sir W. S. CHURCH, Bart., K.C.B., M.D.

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.

Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.

Mr. W. H. GASKELL, M.D., F.R.S.

Mr. J. TOMKINSON, M.P.,

Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

The Right Hon. Sir JOHN FLETCHER MOULTON, called in; and Examined.

12691. (*Chairman*.) You are a Member of the Privy Council, a Fellow of the Royal Society, and a Lord Justice of Appeal?—I am.

12692. For many years you have taken an interest in the progress of curative science?—Yes, I have taken a very keen interest in it both from the scientific side and also from what is called the ethical side. Having been a politician, and realising that the public take to themselves the right and the duty of controlling everything that goes on in the kingdom, I have realised for many years that the question which this Commission is directed to consider would come on in some form or other in the way of an inquiry; that, in other words, the justification for scientific research in connection with curative science would be examined. As I have in many other things observed, the advance of science takes the workers in science more and more beyond the ken of the ordinary public, and their work grows to be little understood and much misunderstood; and I have felt that, as in many other cases, the need would come for interpreters between those who are carrying on scientific research and the public, in order to explain and justify their work. The consequence is that I have considered the subject very much from the very point of view from which this Commission has to consider it. I have come, many years ago, to a very deep conviction upon it, and therefore I am glad to have the opportunity of putting before this Commission the reason for the faith that is in me.

12693. You say that you have taken great interest in the subject for a long time, and I believe you have very closely watched what has been going on with reference to it on both sides?—Yes, I have watched it very closely on both sides. Of course, you quite understand that so far as the scientific side is concerned, though I am qualified to judge of the scientific results and of the evidence for them, I am as an outsider immeasurably less qualified to speak with authority on the scientific points themselves than are some of those whom you have had before you; but, on the other hand, I think that with regard to the scientific methods and with regard to the bearing and meaning of the results upon the subject of this Commission, I am, as a close observer from outside—

12694. A highly qualified observer, I may say for you?—I am qualified to give some assistance to the Commission.

12695. In your long experience at the Bar, before you went on to the Bench, you were specially interested and employed in matters that dealt with questions of scientific investigation of all kinds?—Yes; I have had,

I think, unrivalled experience in being what I have described as an interpreter between those who are doing the pioneer work in science and the people who have to judge of the results of their work; and it is, perhaps, more from that standpoint than from any other that I desired to put my views before the Commission with regard to this question.

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12696. In your view, I understand, the real issue raised by the agitation against experiments on animals is, whether or not curative science shall be studied experimentally or whether it shall become a purely observational science?—Yes, I think that in the controversy a great deal has been lost by looking at it in too narrow a light. It is not a question of vivisection—that is to say, the performing of cutting experiments on living animals; that is a mere nickname, a mere catch phrase which was originally invented, I have not the slightest doubt, for purposes of prejudice. I am not saying that with any desire to find fault. One always likes to get a name which expresses one's own point of view without further explanation; but it is a most imperfect representative of the real issue. The real issue is whether curative science is to be an observational science or whether it is to be an experimental science, and, as I shall hope to show this Commission, the importance of the decision of that issue is impossible to be exaggerated. But I shall deal with that a little later on, after I have dealt shortly with the grounds of the opposition to its being an experimental science.

12697. You have something to say, I think, on how far the opposition is based on humanitarian grounds?—Yes, the opposition purports to be based on the principle that we are bound to regard the sufferings of animals, and that the avoidance of the suffering of animals is a motive which ought to influence every man with a conscience. It is a consequence, no doubt, of a change which has come over the views of mankind decidedly in the right direction. Of old, cruelty was thought nothing of, whether it was to men or animals. You cannot read the history of the world down to, say, the seventeenth century, without feeling that there was an extraordinary callousness to suffering of every kind. A great change from that time has been passing over the world. First, there came the regard for the sufferings of men, and cruelty towards men gradually passed out of favour; and now the thought of human suffering and the desire to avoid it is one of the most unquestioned and influential motives that guide the action of mankind. Presently there followed on that the extension of the feeling to the sufferings of animals, and there is no doubt that now, with the best part of

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humanity, sympathy for the sufferings of animals and the desire to lessen those sufferings are most influential motives of action. Now that appears to me all in the right direction, and so far as my opinion goes it is not only not contested by either party to this controversy, but both of them would in words accept it as a duty to lessen so far as possible the sufferings of animals as well as of men. I do not wish to be misunderstood when I talk about the sufferings of animals in the same breath, as it were, with the sufferings of men. I must therefore point out a very broad distinction between the two, which must be borne in mind if we are to come to right conclusions in this matter. The sufferings of animals are substantially physical only. The sufferings of men are not solely physical. Almost as important a factor in that suffering is mental suffering, arising partly from a man's relations to society—that is to say, that death or sickness will cause pain and suffering and trouble and misery to those who are intimately connected with the man, and partly in the individual himself. He is capable of feeling anxiety, regret, dread, and many other things which are of the most serious importance in measuring human suffering, but which are practically absent from animal suffering. I do not say this in any way to detract what I have said about the importance of avoiding and preventing, so far as we can, animal suffering; it is only that we may keep true our ideas of what suffering is, and to do that, one must bear in mind the broad distinction I have just mentioned.

12698. You mean that if a man and an animal have each to be subjected to a serious operation under anaesthetics, the whole of the pain and suffering that the animal experiences after it is properly anaesthetised and dies under the anaesthetic, is covered by the anaesthetic?—Yes.

12699. And it has not suffering before or after?—No.

12700. But in the case of a man, he has the suffering of anticipation, thinking of the future, of those who belong to him, and if he dies he has the suffering of his family. If he survives, he has suffering from that again, which may amount also to disablement and loss of some of his functions?—Yes, that is a very striking example of the difference, and it brings out one other point which you must also bear in mind, and that is, our responsibility with regard to mankind as compared with our responsibility to animals. In the case of an animal, if the future is to be a future of pain we unhesitatingly kill it. No matter how certainly the future must be a future of pain in a man, we must by universal acceptance keep him alive. Therefore in deciding on our duty to men and animals we have to realise that no matter what be the consequences we have no right to decide with regard to man as to whether it is better to terminate his sufferings or not. If you take the case of a man with some terrible disease over him which may perhaps be cured by an operation, he has the terror of the uncertainty of the cure. The operation is fortunately under anaesthetics, and he does not feel it, but afterwards there may come an only partial cure, and a painful life ahead of it. Not one of those elements exists in the case of the animal. The animal, if the pain is going to be greater than the value of life, is put out of the way painlessly. So that we must remember that distinction, because it is not permissible in a serious inquiry like this to shut our eyes to any truth. There is no safe side on which one may lean in the way of exaggeration in the examination of a question like this, and although you are going to be loyal to the duty of lessening the pain of animals, you must not mistake the relative position of pain with them and pain with us.

12701. You admit that there is a responsibility on everyone who operates on animals to avoid unnecessary pain?—Oh, yes. In fact, I go further, and I think that one ought to admit as a fundamental principle that it is the duty of all men to work for diminishing pain. The diminution of the totality of pain in animals and in men ought, I think, to be a guiding motive and a prominent motive in the case of people in the position of those who are working in curative science, and also in the position of people in ordinary life; and in my opinion it is just as operative, and, if I might give my private opinion, it is far more operative in those who are connected with the curative sciences than in the people who attack them so bitterly. And it is for this reason: Because those who are dealing with the curative sciences are perpetually brought in face of pain, and it is a point of honour with them to conquer that pain and relieve it, till that aim becomes to them a kind of second nature—a kind of all-pervading motive.

12702. You are speaking rather, are you not, of the practising physician or surgeon than of one who devotes himself entirely to research?—I am; but the reason why I cite that is this, that there is an almost unanimous opinion among those who are engaged in the practical work of the curative sciences in favour of experimental research in connection with them. That support has been discounted by people saying: "Oh, those who are engaged in curative science get callous to pain." Now, in my opinion, they are more sensitive to pain, because it is perpetually appealing to them, and, if I might say so, it is the life-long foe which they are engaged in fighting. I mentioned therefore the extent to which the desire to diminish pain was present to those who are engaged in it, not so much as dealing, as you say, with those who are engaged in research, as with those who, with a full knowledge of the matter, give them their support in order to point out that that support has the very high sanction of coming from people who all their lives are fighting pain, and fighting it often at very great personal discomfort and self-sacrifice, till it has become to them, as I say, an almost all-dominant motive. The difference between the two schools is this: Those with whom I range myself, who are desirous of extending and supporting experimental research in the curative sciences, consider not only the pain that is inflicted, but they consider the pain that might be prevented, and they hold themselves responsible for permitting pain which they could stop, just as much as for inflicting pain deliberately—they look at the two together. The other school consider only inflicted pain. They think it their duty to prevent pain being inflicted, even though the infliction of pain may lead to the prevention of many times that much pain in the future.

12703. Of course, your argument, if carried out—I am not disputing it, or offering any opinion about it—would lead to this, that if anaesthetics had never been invented, you would still be justified in carrying out experiments on living animals, and inflicting the necessary pain upon them for the sake of the knowledge that would be acquired in the saving of pain in the future?—It would; but if you will allow me I will put my practical conclusions latest. I want first of all to lay down the foundation of the decision. I want to get before the Commission the fundamental principles as I view them, and to establish them, and then I propose to take the practical conclusions, when I will deal with such a matter as you have referred to.

12704. But, of course, the actual fact that anaesthetics and their use have been discovered now does not render the argument I was putting a very practical one. It is rather a logical consequence than a practical one?—Certainly; I quite understand that. Now I want the Commission to allow me to take what I may call a simile or an illustration. I am going to take a case which in my opinion exactly represents the attitude of the two rival schools; it is not connected with experiments on animals at all, and that is one of the reasons why I have chosen it. I want the Commission to understand it that I take as explaining my views, and I shall trust to bring the question which is before this Commission up to the level of this illustration. I will suppose that a ship which is plague-stricken, and has got rats on board, arrives at a port. A man with a sense of his responsibility, knowing that there is high probability that rats convey the plague, would unhesitatingly extirpate those rats, even though his only method of doing so was by putting them to a painful death, whether it was by poisoning them with phosphorus or by stifling them, or by even more painful methods. He would not hesitate; he would feel it his duty to extirpate them. Now, supposing that a person were to come and say, "I could not bear to see those poor creatures running for their lives and in danger, trying to save themselves from their relentless pursuers, and so I let as many as I could escape," I have no doubt that the person who did that would think that he acted from humane motives. But what would be the consequence? It might communicate plague not only to a town, but to a whole nation. It might bring positively measureless misery. The first man would be right, because he would look at the inflicted pain which would be to the bad side of the ledger. No one would have a right to inflict that suffering merely capriciously. But he would see beyond that pain he was inflicting—that in acting thus he was preventing an amount of pain which was beyond all measure greater than that which he was inflicting; and if he was a man to whom pain appealed, who had a heart which felt keenly suffering whether in men or animals, he would do it all the more unhesitatingly. The other man would

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think only of the inflicted pain, and say, "I am too tender-hearted to inflict it." He would not consider that by not doing it he was causing preventible pain on such an enormous scale. That is typical of the struggle between the two parties. I have chosen that example, not because it is an exaggerated one, or because it is an unfair one. I have chosen it because in that case there is no veil between the act and its consequences. One can see plainly that the letting free of those infected rats might produce those consequences. But now just let me take a hypothesis. I will suppose that, instead of its bringing a plague, the killing of those rats would lead to the discovery of the antiseptic treatment. That antiseptic treatment put an end—substantially, of course, I mean—to an amount of human misery that we can scarcely realise. The suppurating of wounds, accidents leading to months of painful sickness and a recovery which was only partial, and left the people maimed, the horrors of war doubled, the deaths in war enormously increased were all consequences of sepsis—it was just as bad as a plague. Now, instead of its being a question of "either you extirpate the rats or you have the plague," it had been "either you extirpate the rats or the antiseptic method will not be discovered, and the suppurating of wounds and all the horrors which follow from that must remain in the world," you see at once that to the thoughtful man the argument in the two cases is precisely the same—"If I do not inflict this pain I permit an unmeasured amount of pain which I could prevent." In my opinion, when you once see clearly the causal connection between the pain you inflict and the diminution of pain which follows from it, it makes no difference in what way it follows. Your duty is to take that line which produces the minimum total pain, and whether the pain is inflicted pain or whether it is preventible pain which is not prevented is in my opinion one and the same thing.

12705. Supposing that you have not got an immediate prospect of preventing pain or of preventing septic conditions, but are pursuing these experiments for the purpose of acquiring knowledge that you do not possess, with the hope and belief that it will lead to some good, how do you deal with that?—You have anticipated the very next step which, in my opinion, we have to take in the examination of the subject. The reason why I gave this illustration was that I wanted to put before you by an example the criterion which I say every humane man ought to follow. It is that he ought to take that line which before his conscience he thinks will lead to the diminution of total pain, and if he is satisfied that the infliction of a certain amount of pain is the right way to diminish the total pain he is bound, from his very feelings of humanity, to take it. And now you say—and, if I might respectfully say so, quite rightly—if that is going to guide you in a matter of this kind, you must be satisfied that a man ought to realise that the practice of the experimental method is the wisest way to work for the diminution of the totality of pain. If I have brought the question to this point it is all I want to bring out of the example which I have taken. I admit that those who claim that they should inflict pain are bound to show that it is the wisest thing to do with the view to reduce the totality of pain. But I want to point out one other thing, and that is this—that a man would be bound to take the same course with regard to the rats if there was not a certainty, but a reasonable ground for thinking, that plague would follow. He would have no right to say, "It is not demonstrated; I will risk it."

12706. There is a case, of course, beyond that, where a man is making experiments upon a living animal with a view to ascertain precisely what the action of certain parts of the body is—what their functions are?—Yes.

12707. Not that he can point to any particular saving of pain or good that can be done by it, but he is doing it to acquire knowledge which he does not possess, with a philosophic belief that it will lead to the good of reducing pain?—It is a real pleasure to me to have such points put to me, for this reason—it convinces me, as indeed I knew before, that I have the honour of speaking before those who have thought very seriously over this matter, and realise the various points which arise. You will find that I shall take that point up in its proper place; because, in my opinion, it is of the greatest importance that we should face specific questions of that kind. You cannot, in this matter, come to a wise conclusion by simply dealing with vague generalities. But if you will allow me, I will take it up later, and you will find, I think, that I have put it in

its logical position. Now, I said that I took that example of the plague-stricken ship, not because it was exaggerated, but because there was an obvious connection between the act and its consequences, and then I pointed out that if you could get an equally obvious connection between pain inflicted and a great discovery it would be the same thing. I said that the parallel I have taken was not unfair to either of the two parties, and I want to digress for a moment to show that I am justified in saying so. I think if you were to ask a man who had given thought to the matter, who, of all living men, had been privileged to do most to reduce pain, the answer would almost, without exception, be "Lord Lister," who introduced the antiseptic method, which has been followed now by the various aseptic and antiseptic methods which obtain all over the world. Yet I saw the other day in a paper that at an anti-vivisection meeting Lord Lister's name was mentioned, and it was greeted with shouts of "Brute." They called a "brute" the man who had done most of all living men to lessen pain. And that was, if I might say so, perfectly consistent. They knew that he had voluntarily inflicted pain in his experiments, and that was all that they looked to; and they did not realise that if their real motive, the thing which was the real spring of their action, was the desire to lessen pain, Lord Lister's name should have been the most highly honoured of all. Now, I am not speaking bitterly at all of these people, though I oppose their action so much; I only want to point out that they are acting according to the effect on their emotions of the contemplation of pain; not according to the pain itself. The prevented pain, which has made Lord Lister's name so esteemed, is not present to their minds. The inflicted pain strikes their imagination, and the consequence is that they, with the best of motives, denounce him because he is willing to inflict pain, without realising that he, who is the most capable, at any rate one of the most capable, of all men to know the ultimate result of his actions, has been doing it for the purposes of reducing suffering, and has succeeded beyond all hope. So that I am not treating those who oppose me unfairly in pointing out that they do not regard the more distant consequences of their action—they only regard the question of the immediately inflicted pain.

Now, I want to go on to what the Chairman has put to me, viz., that I must establish that experimental research is the wise mode of action if we wish to diminish pain. I start from the following fundamental facts. I say that if you look round the world, the only way in which we can diminish pain is by human action. Animals go on regardless of the pain they cause to one another. In fact, the universe is built on pain. Whole races of animals live simply by killing other races and killing them without the slightest regard to suffering. So that we must look to human action alone to diminish pain. Now, what is it that prevents our diminishing pain in the world? One feels at once that it is not the absence of desire to do it. I believe that desire is very prominently present. It is because we do not know how to do it; it is our ignorance. Take the case of doctors. Doctors are as desirous as they can possibly be of lessening suffering in the world, and yet how often they stand by the bedside and the patient goes on suffering and they can do nothing. Now, the absence of our power of diminishing pain is due to ignorance, to our not understanding two things. First, not understanding the causes which excite pain; and, secondly, not understanding the action of those causes on the organism—that is to say, how it is that those morbid causes, if I might use the phrase, produce pain? I call the first one (simply to distinguish it) the pathological factor—that is to say, what are the springs of disease; and the other is the physiological factor—that is to say, how those springs of disease cause pain in the organism; that is just a division for convenience of the knowledge which we must possess in order to be able to lessen and control pain. Let me give an example of the way in which lack of knowledge increases suffering. The discovery that the organism could, by means of anaesthetics, pass into unconsciousness without passing into death, and that all the living operations could go on equally vigorously while consciousness—that is to say, the possibility of pain—was entirely suppressed, led to the use of anaesthetics, and, as a consequence, to the diminution of suffering, both directly during operations, and indirectly in increasing the possibilities of surgery. Without the knowledge obtained from that discovery men were obliged to perform operations in the brutal method of the old surgeons, because there was no other alternative. This is only an example of the way in

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which our power is limited by our knowledge. And I wish to point out with regard to this point, that here again we have no difference of opinion. Not only the whole medical profession, but the whole public agree that doctors ought to be armed with all the knowledge of the time. There is no difference of view as to this. Every attempt is made to increase the efficiency of the education of the doctor; because we feel that by arming the doctor with the knowledge of the time we arm him as best we can with the means of diminishing pain. That, of course, is true with regard to the knowledge that has been attained during the last, let me take it, 40 years—the only years as to which I can speak; and that knowledge has been mainly due to experimental research. Therefore all must feel that this knowledge which has been obtained by experimental research is useful for the purpose of lessening pain, because we try our very best to make all those who are engaged in practical curative work masters of it. I do not think I need point out to this Commission further how all-important knowledge is where you are dealing with organisms, and you want to stop suffering.

There is one other thing I want to say, and that is this. That our inability to stop suffering, owing to absence of knowledge, is not only to be found in the case of obscure diseases. They afford striking instances of it, no doubt, and are by no means rare. I should think there are very few of us who have not seen those near to them suffer from diseases which have baffled the doctors, and whether or not they could be eased or saved was decided by whether or not the doctors could read what was the cause of the trouble. A friend of mine has just died, apparently of a painful disease, which the doctors could not touch. A *post-mortem* showed that it was only because they did not realise what the true cause of the disease was; it was one which might almost with certainty have been relieved. Such diseases may have marked symptoms, but the doctors are unable to read from those symptoms what is the cause, and therefore they cannot fight the cause. But, although these cases are striking instances of the principle, they are nothing like the most important; the most important are diseases which are quite common, of which we cannot yet find the cause, and not finding the cause all we can do is to deal with the symptoms.

12708. I think we all agree that there is a very vast field of medical knowledge yet; I think every medical man would agree to that, as well as ourselves. But really, the question is how far that knowledge can only be found in experiments upon animals?—Precisely. That, I think you will find from the *précis*, is the next point I want to mention. But I wished to put in the forefront that it is want of knowledge which prevents our diminishing suffering, because I shall trust to show to this Commission that the pole star by which we steer is knowledge. It is ignorance that hampers us. I am using the term ignorance in a sense that will not, I trust, be misunderstood. I am aware of the gigantic amount of knowledge that the present and past members of the medical profession, and those who have been engaged in research, have already accumulated, but what I mean by ignorance is the vast field of what yet may be known, but is not at present known. I desire to impress upon the Commission that that is the real cause of our impotence. Now, let me take the question of how we are to get rid of this ignorance—and here we are on firm ground. No man who knows anything of science has any doubt whatever that the right way to advance knowledge is by experiment. You can take the whole range of the sciences, and I would challenge an opponent to name one in which advance, if it has been rapid and striking, has not been through experiment. Where we are reduced to observation a science crawls. Where and in proportion as you can use experiment, the science advances rapidly. I could take any science to show that; it is much easier to show that experiment has led to advance than to find a case in which advance has been made purely by observation; because a scientific man rushes at once to experiment if he can do so. He adds, of course, the testimony of observation—that is extremely valuable for the purpose of guiding and starting his experiment; but everybody knows that each observation becomes only the nucleus, or, rather, the basis, from which you start your experiment. I was puzzling to think of some branch of science where there was only observation, and the only thing I could think of was the case of volcanoes. Now, there is not anything that has attracted human attention more, or fascinated it more, or has been on a bigger scale or more open to observation,

than volcanoes; and yet we are almost in the same state of ignorance of the origin and the *modus operandi* of volcanoes as Pliny was.

12709. The difficulty is just as great now as then for anybody to go down a volcano when it is in eruption?—Precisely. In no way can you bring experiment to bear upon it. As soon as you can bring experiment to bear upon a subject you are free; but as long as you can merely observe, your progress is very slow. The reason is that experiment is like cross-examination. You can put the question you want, and Nature always answers it. She does not answer the question you meant to put; she answers the question you actually did put. She swears by the card in the most shocking manner. She does not care in the least what you meant to ask, but she does care what you asked, and she answers it with perfect truthfulness. And the consequence is, that when you adopt experiment, the great experimenter can put a question, the answer to which lets the whole secret out. I am going to give an example that I remember impressed me very deeply. I cannot tell how many years ago, but it was just about the time when it was first discovered that guinea-pigs were susceptible to tuberculosis. I heard from a friend that a doctor of his acquaintance, a practising physician, I suppose, had been troubled as to whether consumption was a communicable disease or not: I should say that the vast preponderance of opinion at that date was that it was hereditary. I remember, as all of us remember when we were young, the way in which consumption would, as it was called, go through a family, and it was thought that this was because it was in the strain. Then some bold people suggested that it was possible that it might be due to its being communicable, although probably to the ideas of that time it looked about as likely to be communicable as a spinal disease. What that doctor did was this: He took a dozen guinea-pigs, divided them into two sixes, and he let one six have the run of the wards of a consumption hospital, and the other six have the run of the wards of another hospital, just about in the same conditions of temperature; he left them there for the requisite number of weeks, whatever it was, and then he killed the whole dozen, and he found that the six which had been running in the consumption hospital had tuberculosis, and that the six that had been running in the other hospital had no trace of it.

12710. (Chairman.) The other was not a consumption hospital?—No, it was not.

12711. (Sir William Collins.) Whose experiments were those?—I cannot tell you the name of the doctor, because it was not given me, but I heard it long ago, in the early days of research upon tuberculosis, and I believe it to be true. But if it was not actually performed, it would still serve as a typical example of experiment. I have no reason to doubt that this experiment was actually performed, because the man who told me was a most intelligent man, and he told it to me at the time as having been done quite privately, by a doctor whom he knew, in order that the doctor might settle the doubt in his own mind.

12712. (Chairman.) You would say that it is rather an illustration than a typical example if it is not given us at first hand in some way?—You may take it simply as an illustration, although the very thing has been done, I know, by other people. Klein did it. If you remember, Klein put guinea-pigs into an air shaft which led from the wards of a consumption hospital, and found that they developed tuberculosis. But we will take it merely as an illustration. I am not basing anything on the fact. What I want to point out is this, that such an experiment would settle the question more than would twenty years of observation.—I might say a century of observation—for this reason, that you had placed these similar animals under entirely similar circumstances, with the exception that in the one case, but not in the other, there were present in the immediate neighbourhood persons who were sick of the disease, and you will find as a result that in the one case they become sick, and in the other they do not. There is no question of heredity, and there is no question of the acquired disease being the consequence of different circumstances of living, of different circumstances of nutrition, or anything of that kind. The only thing that is present as a cause is the neighbourhood of the sick people in one case and not in the other. In the result the difference is disease in the one and not in the other.

12713. What was the fact that it was wanted to

discover there—whether guinea-pigs could take the complaint?—No, whether the disease was communicable from one person to another.

12714. But there are plenty of examples of people who have not tuberculosis daily frequenting hospitals and not acquiring it, are there not?—Oh, yes; but the point of the experiment was not under what circumstances was the disease communicable, but whether it was communicable at all. Just take the case of persons who are ill of spinal disease.

12715. I follow you?—Their relatives go freely among them, attend to them, and try to make their sickness as easily borne as possible. And so it undoubtedly used to be when members of a family were sick of tuberculosis. Everybody thought it was only kindness that the brothers and sisters should attend to their sick sister, and nobody thought that there was danger. But when you had found that this was a communicable disease, your duty with regard to children and other people in the presence of the disease became very different, and it was only to find out this that the experiment was tried. The experiment showed affirmatively it was communicable. That was only, of course, the first of a necessary series of experiments to show how it was communicable; but from that moment that doctor knew how great was the responsibility of allowing people to attend or be in intimate contact with one who was affected by consumption. Just try and see how you could get an equal degree of certainty from observation. You take a case where it has run through a family. Yes; but it might be that it was hereditary in the family. Then you find that in a particular place it is very largely prevalent. Yes; but that may be because, as compared with other places, the climate or soil of that place produces a predisposition to it. Neither group of cases points necessarily to communicability. Some people will be struck in one way with a series of these cases, others will be struck in another way, and there will be a perpetual conflict as to which is the true explanation. But when you take such an experiment, there is not a doubt; you cannot explain it, except that the disease is communicable. And the consequence is that you commence your further examination with the consciousness that it has been demonstrated that it can pass from the sick to the well. Now, I was very much struck—as illustrating the impossibility of arriving at these things by observation—by a piece of cross-examination by Sir William Collins of one of the witnesses before the Commission; it illustrated it in the most striking way. He was dealing with the result of the serum treatment of diphtheria. An appeal was made to the statistics of diphtheria hospitals in London, and the witness said: "Oh, the serum treatment is demonstrated as being a great success, because you have here the same hospital in the same place dealing for a consecutive number of years with the same population, and the statistics show a great diminution of the percentage of deaths after the introduction of the serum treatment." Sir William Collins, if I might say so, quite justifiably cross-examined on that. He will know whether I am fairly representing him, but, as I remember it, he asked are we now getting the cases earlier; is it not true that we have made such advances in the diagnosis that we know better what is diphtheria and what is not? And he went on pointing to differences in the circumstances of the earlier years and the later years as taking away from the conclusiveness of the statistics.

12716. (*Sir William Collins.*) As an element that required consideration?—Exactly, as an element that required consideration. That is perfectly fair. Statistics that are got in that way must be rightly open to that criticism. And this is the reason: You dare not let the children wait in the later years so as to make the time at which they are taken up for the purpose of treatment comparable with what it was in the earlier years, because you are bound to do for every child the best thing that you can, and the consequence is that you dare not make your experiment scientifically more perfect, because there is the paramount duty towards human life. Now, you could not have had a stronger case, *i.e.*, one in which the observational method was working under greater advantages. You had hospitals devoted to this disease alone, dealing, as I have said, with the same population, in the same place, under, as far as possible, the same circumstances; but you could not, and you dare not, and you ought not to, make the circumstances identical

because of your duty to the individual. Compare that with the case I was putting forward of the guinea-pigs; I could give another and more striking example, which is the classical experiment in connection with anthrax, in which, after Pasteur had worked out the treatment by inoculation against anthrax, he took 50 sheep for his experiment; 25 of them were inoculated and 25 were left uninoculated. Then the whole of the 50 were inoculated with virulent anthrax, and within four or five days every one of the 25 that had not been inoculated died, while of the others 24 were alive, and one was shown to have died from things unconnected with anthrax. You see by these examples how in experimental research you can make the circumstances identical, and one experiment of that kind (properly repeated, so as to eliminate error) gives you a certainty that years of observation could not give. And all those years of observation have paid their toll in suffering. It is no use saying, Well, we shall learn in a few years hence, instead of learning it now. All that time there has been preventible pain, which has not been prevented because you have not known how.

What, therefore, I want to point out is that we know that the way to acquire knowledge with certainty is by experiment; it follows from its very nature, and from our universal experience in all sciences, that this is the way in which you best acquire knowledge, because you can make each experiment answer some critical question. You can arrange matters so that you have two cases precisely similar in all respects, save that the one is without that cause present and the other is with that cause present, and the contrast gives you a light which you could not get from observing instances where you cannot eliminate other causes. The more complex the subject is, the more factors there are at work, the more essential the experimental method is; and the most complex of all phenomena are those that relate to living beings. I am sure there are those here who have made experiments in connection with life, and know from experience the truth of what I say. The number of factors at work is so large, the difficulty of isolating them is so great, that it often takes very much longer to realise how you can put the question to Nature than it does to put it, and to draw your conclusions from it when you get the answer. But the more complex the subject is, I say the more are you driven away from observation, which only gives you the total result of many causes which are varying present in all cases, to something which will enable you to isolate several of the causes and determine separately their effects.

12717. (*Chairman.*) That brings you, I think, to the question which I was putting to you about pure experimentation, and the justification of experiments on animals where you have no specific disease in view?—Yes. Many will think me a heretic on this point, but I have a strong view that, when you have once realised that experiment is the right way to acquire the necessary knowledge, it is a bad thing to work too directly and too consciously for practical results.

12718. When you call that a heresy, of whom are you speaking as the orthodox?—Most people say that you should only make such experiments with the direct view of some practical application for the benefit of mankind.

12719. We have had a good many witnesses before us who take what you call the heretical view?—I am delighted to find that I am not alone.

12720. As well as those who take the other view; and others who would say that you do not go far enough?—I am glad to find that I have got my comrades. You must not misunderstand me. I think that the object and the reward and the ultimate justification of research is the benefit it confers. But what I wish to urge is that it is unwholesome to keep that before your mind when you are working, at all events, in the earlier stages. May I give a parallel which I think explains and justifies this view? I take the history of alchemy. For centuries men of the highest intellectual attainments—for there can be no doubt of the ability of the men who worked at these subjects—worked hard at the transmutation of metals, and the result of those centuries has only been snippets of knowledge, if I might use such a phrase. I do not undervalue them; they gave us, I believe, some very valuable reagents, but that is practically all. And the reason was that they did not commence by working so as to get to know the nature and properties of the things on which they were working; they worked straight for a result.

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12721. For an impossible result, was it not?—It may have been an impossible result. But whether it was impossible or possible would have made very little difference if they had commenced by laying the foundation in knowledge, because, although we should not have obtained the transmutation of metals, we should have had modern chemistry some three centuries earlier than we did. But they only worked for a result.

12722. They did not care about chemistry. Their researches were not chemical researches?—Well, transmutation of metals was intended to be a chemical triumph. If I might take a parallel in curative science; this work was like the shots at specific cures for cancer—violet leaves and all that sort of thing—made by people working straight for a nostrum that will cure a disease like that, the nature of which is shrouded in such mystery.

12723. I only meant those people were fighting a particular disease which they had before them, and which was evident under their eyes, and they were seeking remedies blindly?—Yes.

12724. The other people were seeking something in the clouds altogether; they were not pursuing what you would call chemical research, although they used certain chemical experiments?—But if they had only laid a basis; if they had only worked at it as you try to reduce a fortress, by parallels, where each parallel brings you nearer to the fortress. If you attempt to take it by a rush and fail, you are no further than you were before; but if you make your parallels you are so much nearer, and that is a permanent advance. Just in the same way I think that if I were to ask any scientific man of position in the medical profession, "Where do you look for the solution of the great problem of cancer?" he would say, "I look to a gradual acquisition of knowledge about it, and the breaking up of the obstinate silence of the disease which will tell us nothing, neither its preferences nor the things it dislikes, nor its origin, nor anything." Such men do not give much heed to the investigations aimed directly at its cure, because they do not believe they will succeed. They believe that we shall have to know a great deal more about the disease itself before we know in what direction success can come. But what they do think is this, that by gradually acquiring knowledge of it, knowledge acquired quite independently of whether it is a thing we could use or not, the disease will gradually cease to be a mystery, and when it has ceased to be a mystery we shall find some weak point in its armour. Therefore I am satisfied that the wisest thing to do is to realise broadly that our ignorance of the springs of disease and the nature of the organism that suffers, are the cause of our powerlessness, and that all research which is intended to lessen our ignorance is in the right direction, and that it is in vain for us beforehand to say which will soonest lead to a beneficial result. I feel, therefore, strongly that this is the principle which, with proper regulations and restrictions, we ought to accept; and that we ought to fix our eyes on knowledge and not on immediate promise—immediate promise is likely to be delusive—and when I come to the instances by which I trust to justify the serviceability of experimental research, I think I shall be able to show that those things have sometimes reaped the richest harvest in which the promise came quite late.

12725. You think, to sum up that part of your evidence, that the experimental method is justifiable even though you have no immediate practical object in view beyond the acquiring of knowledge and the hope and belief that it will lead to some ultimate useful purpose?—Yes, you have the hope and belief that it will, and you have the certainty that it is the right way to work for it. Where you are ignorant of the evil and ignorant of how it operates, you are like a hunter who is not yet on the trail of the fox. He has to cast about in order to come upon it. Similarly, it takes a very great deal of experiment to know in which direction practical success may be looked for.

I want to say one more thing before I go to the examination of whether, as a matter of fact, the experimental method has shown itself to be of this great practical value, has thus justified its use. So strongly do I hold that what we ought to fix our eyes upon is the acquisition of knowledge and the removal of ignorance, that if I was here without being able to show a single practical result of experimental research

in curative science I should give evidence in the same direction, and so far as I personally am concerned with the same confidence, for this reason: Supposing I was to go back, say, 40 years (I am not suggesting for a moment that prior to 40 years ago there was not a great deal done in research, but I take that period as including the great outburst of experimental research, beginning with the early sixties and since then) I should have all my data. I should have the fact that ignorance was vast, vaster even than now, that ignorance paralysed our power of preventing suffering, and that the right way to get rid of that ignorance was by the experimental method; and therefore, even though I could not show that we had obtained any practical results, I should say to this Commission, if it were sitting in the early sixties, that it was their duty to take care that experimental research was not hindered. The idea that you do not justify experimental research unless you can show that it has already produced practical results would make it never justifiable to commence it, and, in my eyes, it would be just about as ridiculous as to revile winter sowing because in April there were no crops. The moment when the practical results come depends upon the nature of the subject. You cannot foresee it; they may come at a burst. After long investigation which apparently leads to nothing you may suddenly find your reward.

12726. It might not have been unreasonable in Adam, but since his day there has been a very long experience of winter-sown crops?—There has been a long experience of winter crops, but what I mean is, that a man has got no business to say that because the crop has not yet come last winter's sowing has failed.

12727. It would have been an unreasonable thing in Adam?—I agree with you; but, if I may say so, I should use that in my favour; because I should say that if you look at any science you will find that theoretical investigation has always led ultimately to practical power. I could give examples in industries; but they will occur to most of those present. Investigations which have been purely scientific have almost always led to practical results, because the knowledge has been able to be used, and it has produced great results. So that, just as since Adam, we have had great experience in winter sowing, so I should say that since the days of Bacon or Newton we have had great experience in this—that you never can have an increase of knowledge in any subject bearing on practical life without its leading to increased practical power.

I want now to deal with the justification of the use of the experimental method by what it has actually performed, and I am going to ask you to allow me to confine myself to the work of the last 40 years, because that is the period which has fallen under my direct notice. I shall group it in the way I group it in my own mind in order to make out this justification. As I have said, to my mind this direct appeal to practical results is not necessary, though it is immensely helpful, to enable one to make up one's mind as to what is the right course to pursue; but it is all important in dealing with the many well-meaning persons who do not go along with me in their views with regard to it. The difference is not, I think, because there is a difference of fundamental principle—they are as loyal, probably, according to their lights, to the doctrine that we ought to do our best to lessen suffering as I am; but they have to learn that which scientific training easily teaches those who have had much to do with it, that the right way to do so is to make full use of the experimental method. I wish, therefore, to take the results of experimental work in the last 40 years, and point out how it has removed ignorance—that is to say, how it has given us knowledge and how that knowledge has become of practical value in reducing suffering.

12728. If you please?—Of course the first example which one must take is that of sepsis and the antiseptic method. If you look at the books of 40 years ago you find that the idea that sepsis in the case of open wounds was due to organisms derived from outside, was rarely held; I doubt whether it was held by any, it was certainly held by very few, and it was not put into practice in any way. And the work of those who demonstrated that sepsis was in all cases due to organisms, and where access was possible, it was almost always due to organisms coming from outside the animal attacked, gave rise to the antiseptic treatment, the value of which it is quite unnecessary

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to dwell upon. It was purely the result of experimental research, and it is a triumph the practical value of which puts it at the head of all the changes in the last 40 years. But I am not going to deal with that any further than just to refer to it, because I am sure that its value and its origin are present to the mind of everyone here.

I wish chiefly to deal with the case of microbic diseases, which for reasons that will appear, I shall divide into infectious diseases and diseases communicable otherwise than by what is ordinarily called infection. If you go back 40 years you will find that these diseases were complete mysteries; they were, alas! recognised as facts, because they could not be otherwise; they were ever present with us. They were known as things which ran through a certain course, their symptoms were studied, and the doctors of that time did their best to alleviate them, no doubt, by nursing and treatment. A great number, no doubt, tried to do so also by drugs, which, I think, most authorities to-day would say were practically of little use. But the diseases themselves were, so far as their causation is concerned, perfect mysteries. Now experiment has changed that from top to bottom. The bulk of these diseases are known now to be the result of an invasion of the system by specific organisms which multiply in the system, and the disease is due to the presence and action of these foreign organisms. In fact, I do not think that I should be giving an incorrect idea of the result of the work of these 40 years if I was to say that it has revealed the fact that we live not as isolated living organisms, but that we are surrounded by and penetrated by infinitely small living things of very varied descriptions—that we are, as it were, bathed within and without in a sea of microbic life; I am using the word microbic in its broadest sense. Within us we have them permanently and probably normally; in fact, if you include the organisms which live in the blood, certainly normally. We are surrounded by them, and instead of diseases of this kind having to be looked at as due to defects or failure in normal action in the organism itself, we have grown to realise that they are the result of attacks upon the organisms by external foes.

12729. Do you regard all this state of things which you have been describing as wholly due to experiments on animals?—Absolutely. It is absolutely due to them.

12730. We have had a good deal of evidence from other witnesses on the subject, and we need not trouble you to describe the instances?—Quite so. It is difficult for me to say which is the earliest case in which this was established; but I will take one of them, because I wish to show how baseless is the idea that the work which has been done in this respect is work which has only led to doubtful or hypothetical results. I wish to show the actual and certain advance of knowledge. Then I wish to show next the way in which that is due to experimental methods. Then I wish to show the practical methods to which it has led. I will take a single example—any one will do—but I will take the example of anthrax. When Pasteur took up anthrax, it was only known as a terrible disease among cattle, leading to a vast proportion of deaths. He ascertained that it was due to an invasion of a particular microbe, which is known as the microbe of anthrax, which multiplied with infinite rapidity, and which ultimately led to the death of the animal. He then found out a method of inoculation (I am not going to deal with the details of his method for a moment) against it, and in the classical experiment to which I have referred, he exposed to anthrax the 25 sheep which were inoculated, and the 25 which were not inoculated. He might have done it by putting them in company with diseased animals. As a matter of fact, he took the severer test of deliberately inoculating them with the disease. The result was that those which were treated with his process did not die, and the others did. Now, that is a scientific result as certain as the action of nitric acid would be on iron; I mean that it was a definite experiment, capable of being repeated, and showing conclusively that there was a way of defeating this disease, a way which might, or might not, be availed of by mankind, according as the importance of warding off the disease was great or was not. There was nothing hypothetical in it; it was a fact which was just as much demonstrated as any fact that has been demonstrated in chemistry or in any other science. I have there taken a case which relates solely to animals. I

have not approached the question of the applicability of the process to mankind, because, fortunately, anthrax is very rare among mankind. But if you confine yourself to the consideration of it as a disease of animals, you see the advance that has been made. First of all, there is the knowledge of the cause—that is to say, that it is due to the presence of microbes. Then (as I shall presently point out) the fact that it was due to the presence of microbes led to the discovery of the method of combating it; and that led to the practical application of that method, and, if you choose to avail yourself of that method, the disease is conquered. The whole is based upon the discovery of the origin of the disease. It was the knowledge that it was due to microbes which led to the whole of that discovery. Persons might say: "What good does it do to mankind to know that this which they call a disease is really a consequence of a particular type of small life which has got into the organism and multiplies there? It does not alter the nature of the disease; people die of it all the same." And you would think that the pure theoretical knowledge that it was due to this particular cause left the practical problem where it was. May I point out how that piece of knowledge did not leave the problem where it was, but put it in a totally different position, and necessarily led to its solution? You can see it in this way. It instantly put the phenomenon of recovery in a new position. When an animal was healthy, and there were only a few microbes put in it, when you would say it was best calculated to repel invasion, they multiplied furiously. At the moment when it was weakest, when the microbes were strongest, most numerous, and the animal was most exhausted (if it was a case of recovery), the tide turned, and the enfeebled organism was able to fight the strengthened assailants successfully. That necessarily led to the conclusion that there must have been a change in the organism during that period, which entirely altered the relative strength of the two forces; and that was the beginning of the realisation that in these diseases there comes some protective change in the organism that is attacked which brings recovery at the time when you would least expect it. This led to the inquiry: What is that change? It must be something that survives the disease, because we know that when a man is recovering from an infectious disease, he can give it to other people. Yet, although he is more exposed to infection from himself than any other person could possibly be, he is not infected. Therefore it must be something that survives the disease. Then the question came: If it survives the disease, is it possible to produce it under circumstances so that it may be present in the organism independently of the disease? In fact, men came to realise that recovery in a case of microbic disease means the "turn of the tide" in a process where, on the one hand, the invading microbes are multiplying, and on the other hand these protective forces are multiplying, and recovery or death means whether the turn of the tide comes too late or not. The moment you see that that is so, the whole question becomes—Can you antedate this turn of the tide? Can you contrive that this protective condition shall be produced earlier, before the mischief is done? It is no use putting a fire out when all that is valuable in the house is burnt. From these ideas there sprung almost immediately the two main methods of treatment. The one seeks to produce this protective state during the period of the disease, and it is a treatment of this type which succeeded in rabies; the other seeks to produce the protective state before the disease comes, and is the method that was illustrated in anthrax. Finally, they led to the third line of discovery. Is it possible to get the protective substance from somewhere else, and introduce it during the disease, in order to antedate the time when the system will become strong enough to repel the attacks of these foreign invaders? Now, if you think of all those three lines of discovery which have respectively led up to the treatment of rabies, to the prophylactic methods which are used in the case of anthrax, pneumonia, and other diseases, and to the serum treatment of diphtheria, you will find that they all came from the knowledge that the cause of the disease was a foreign organism, capable of multiplication, and that Nature defeated it by a change in the organism which made it more capable of resisting. These practical applications were the direct outcome of those two facts, which were pure pieces of scientific knowledge. So long as the disease was considered merely as a disease which ran its course, which was due to an unknown cause, whose consequences were only known by its

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symptoms, no one of those things could possibly be discovered. Their discovery was solely due to the experimental examination of natural processes. I was much amused by some evidence given by one of the lady witnesses who have given evidence before you. She seemed to be very much disgusted with the consequences of all this knowledge, and, if I might paraphrase what she said, it was "Nature has made such nice drugs and put them in the plants all around us. Why should we not use these, and not go to such messy things as serum?" Those are not her words, but they express the effect of them. Nature has put all kinds of useful drugs in plants, but she never dreams of using them for curative purposes. Her method of curing is entirely by means of the messy things with which the researches of the last 40 years have led us to work. We are now, thanks to the theoretical knowledge that we have acquired of the cause of these diseases, working to help Nature along the very lines which she has from the foundation of the world herself taken in fighting these diseases; and therefore, in my opinion, if we are to be guided in our action by our respect for what Nature has so kindly done for us, our course would be to pay very little attention to drugs and to pay very much attention to those lines of treatment which are based on the study of the two closely analogous phenomena of recovery and immunity.

12731. Assisting our natural friends in fighting our natural enemies inside us?—Yes, fighting our natural enemies by those means which, in the cases in which we do beat them, are the weapons which we use.

12732. (Chairman.) I think you were going to deal with the question of the influence of experiment on communicable disease?—I have been pointing out that the whole modern conception of the nature of these diseases and the mode of their cure arise from discoveries which have been arrived at purely by experimental methods, including necessarily experiments on living animals, namely, the discovery that they were due to foreign organisms that entered and multiplied in the system, and the discovery that the changes in the system which unquestionably produce recovery are closely allied to those which also produce immunity after recovery. So far I have only spoken of our attempts to effect a cure by introducing those protective substances which Nature, when there is need, produces in some strange manner. But it looks at this present moment as if we were going to combat some microbial diseases still more successfully by stimulating other agencies by which she effects her cure. I do not know whether Sir Almroth Wright is to give evidence before this Commission, but if so he will point out how he has devised a method of stimulating the action whereby the white blood corpuscles actually destroy physically the organisms themselves, so that we are gradually utilising both of Nature's methods of cure (and utilising them exactly as Nature does) for the purpose of antedating what I have called the turn of the tide, that is, strengthening the resistance of the organism that is attacked more than Nature would do merely by her methods of reaction. I now want to pass from these to another class of diseases in which equally we have been studying during the last forty years the cause of the disease and its action on the organism, but where research has led to a totally different method of fighting them—I refer to the communicable diseases which cannot be called infectious. Take the case of malaria, the tsetse in animals, and Malta fever.

12733. And rabies?—Rabies belong to the first set, because we fight it by generating in the body a protective substance. The way in which we fight it is, we have got a method of giving an attack of rabies which is very quick but very slight, and the consequence is that it produces very swiftly a small amount of protective substances. We then give another one which is also quick, but is stronger, but which can be borne by reason of the presence of the protective substance, and continue doing so while the main body of the attack is moving up. We have thus gradually accumulated a body of protective material, which, when the main attack comes, which would have been inevitably fatal, enables the system to resist it.

12734. I merely thought that the gnat was serving the same purpose and function that the mad dog was; therefore I thought it was really in the same class of disease?—May I apologise. You are quite right. It is not infectious but is communicable in a different way, and ought to be in the second class. The reason I already mentioned it with the first class was

because we there use the method of cure of which I have spoken—that is to say, we attempt to produce a protective substance. It was I that was wrong. I want now to pass to cases where we cannot imitate the method of cure, but because we know the cause of the disease and its method of communication we are able to intercept it. Take the case of malaria. I do not know that our methods of curing malaria have advanced substantially, but we now know the cause of malaria, and we are destroying the possibility of communication. The work that has been done at Ismailia is an example of this. They have got rid almost entirely of the mosquito, which communicates the disease by its bite, and have thus almost got rid of the disease.

12735. Would you say that that was discovered by experiments on animals?—Certainly. They had first to discover in what way this disease was communicated. As soon as they had discovered in what way it was communicated, the destruction of the communicating animal presented itself at once as a method of preventing the disease. A similar case is that of the tsetse, which is absolutely fatal to cows in districts where the tsetse fly is found. They find that it inoculates an organism which produces death. I do not think they have made any advances in fighting the disease when the disease is once communicated; but knowing that it is communicated in that way, our attention is at once turned to discovering how one can fight the tsetse fly, or how one can prevent the tsetse fly itself getting the organism into it which it passes into the animal that it bites. In certain places in England, where there used to be ague, ague is now stopped. It is quite possible that that ague was communicated by the bite of an insect that derived the organism which is the cause of ague from biting some affected animal. We have drained the country; the insect probably exists, but in far smaller numbers, and there are fewer people there, if any, who are affected with ague from whom it can get the infection. The consequence is that the insect is harmless, because it cannot communicate the poison, or rather the poisoning organism, because it cannot get it. And, similarly, we can, when we know the cause of the disease, set to work to prevent the occurrence of those circumstances which enable it to be communicated. This is now being done with yellow fever and many other diseases. It may be that the disease is so bad that the only method we have of fighting its spread is to kill the animal. Take the case of glanders. Now that we thoroughly understand what glanders are, when we thoroughly understand the method of testing whether an animal is suffering from glanders, the animal may be killed at once, and that stops the propagation of the disease. Without that knowledge we could not ascertain so soon what animals we ought to kill, and they would be left as centres of contagion. So that it may be equally effective from the point of practical utility whether we find out the means of communication and stop it, or whether we attack the disease when it is communicated. In both cases it is the knowledge of the cause of the disease and the knowledge of the way that that cause comes to operate on the organism which enable us to devise these practical methods. And all that knowledge has been derived entirely from research work, conducted for the purpose of acquiring knowledge, and from the knowledge has come the power. I do not suppose the investigation of the cause of these diseases was carried on upon the lines of any direct idea of a protective method, but the object was to find out the real nature and operation of that against which protection was necessary. When this is done, the step to practical utility always comes sooner or later.

I have been dealing thus far with the cases of disease. I will now turn to another matter which I see has formed the subject of a great deal of evidence, and that is the study of the organism on which these diseases operate. A vast amount of experimental work of the last forty years has been directed to discover the physiological actions which are going on in man. Mere anatomy, mere structure, will not tell you physiological action; the knowledge of physiological actions must be obtained by experiment from a living creature, because as a rule as soon as death comes they cease. The importance of this work is just as great as is the importance of the investigation of the causes of disease. Take, as an example, that which I see has been the subject of evidence before this Commission—the localisation of function in the brain—the discovery from the symptoms of paralysis, or epilepsy, of the place where the interference with the

nervous system is taking place. It would be quite impossible to ascertain the laws which govern this excepting by experiments on a working animal—that is to say, a living animal. In many cases it need not be a sentient animal; the experiments may be done when consciousness has ceased; but it is absolutely impossible to ascertain them unless you make experiments on an animal that is still living. But there are similarly important experiments which go to blood pressure, to digestion, to the function of the great organs, such as the liver, or the pancreas, or the kidney, or the suprarenal capsules, in which it may be necessary to trace out consequences which do not occur immediately. In that case you must perform them on animals which are not only living at the time of the experiment but which are allowed to live afterwards. The examples which have been given you, I submit, show how all-important such investigations are for several reasons. One of the reasons is that without the knowledge so acquired you cannot read causes from symptoms. I should define the great change between curative science, as it exists now, and curative science as it used to exist many years ago, as being that now we try to fight causes, and then they only tried, or mainly tried, to fight symptoms. Fighting symptoms may be an extremely dangerous thing. A man who, not knowing anything about an engine and finding an over-driven boiler blow off, should fight the symptoms by screwing down the safety valve, would come to serious trouble. And in the same way, if in the case of a symptom—say the symptom of the rise of temperature going up—you say, the disease makes the temperature go up; very well, then, we will help the organism by putting it down. You may get into serious trouble supposing it should turn out that the rise of temperature, by lessening the resistance to the circulation, eases the heart and makes its work less onerous; it may be that the fast-beating heart has been able to do its work without injury only because the temperature has risen and its work is lighter. If, now, you put the temperature down without removing the cause of fast-beating, it may be that you overstrain the heart—do mischief just as the man who screws down the safety valve. I am not saying that this is so. That is not a matter on which I can speak. All I want to point out is that it is only by intimate knowledge of the organism that we know how far it is safe to fight symptoms. But, more than that, it is only by such intimate knowledge that we are able to diagnose causes. You get a headache; it is a mere symptom which may come from many different causes. Unless we have perfect knowledge of the organism we may imagine that a thing which has been successful for one headache is useful for another, whereas it may positively aggravate it instead of doing it good. The aim of all this investigation into physiological action, which is so varied in its character, and relates to every different part of the organism, is to put the medical man of to-day in the position of a man who knows the construction and operation of the engine he is working, who knows what is the meaning of every symptom of irregular action; and the increase of his power of alleviating suffering and staying off death is enormously increased by this increase of his knowledge and power of diagnosing which has arisen from these experimental researches. As I have said, these experiments must be experiments carried on upon living animals, because their object is to ascertain what is the mode of working of each particular organ, and what is the effect of any particular interference with that organ. Sometimes these experiments have resulted in curative methods. You could not have a better example than that which was put before the Commission, namely, the relation of the thyroid gland to myxoedema, and cretinism, where, I think, to the surprise of everybody, it was discovered that we have glands within us which are producing protective substances for the normal life, just as nature will, when there is an attack of a microbic disease, produce *pro hac vice*, a protective substance against the foreign invader. When you take away the thyroid gland, and there is not this protective substance, which is no longer formed, then you find the body gets out of order because that is no longer present which was necessary in order to keep down the inherent forces of dissolution. Then it was discovered that, if you supplied artificially that which this gland produced, you could keep down this form of disease just in the same way as you can stop the effects of diphtheria, if you put in from another animal that which nature would produce in order to repel the microbic attack. Just as in diph-

theria you get recovery, so you find in myxoedema that when you supply this substance which the body has ceased to have the power of producing, you get the remarkable discovery. And this appears to be only the commencement of a series of analogous successes. But, apart from the question of direct curative methods so arrived at, the power of diminishing suffering that is given through increased knowledge of the organism is so great that it is difficult to over-estimate it. I do not think it is necessary to give further instances to this Commission. I only want to put those forward to show that there is abundant justification for saying that knowledge which, from its very nature, must be obtained by experiment has already been of the very widest benefit to mankind. I will go on to the question of whether experiment is permissible for the purpose of acquiring such knowledge. Here I must go back for one moment to the example which I gave, as a typical one, of preventing plague getting into a town by killing all the animals which could convey it. In my view, we are just as much bound to prevent suffering due to ignorance, and just as much justified in inflicting such pain as is necessary for the purpose of doing so, as we should be in killing noxious animals. I have pointed out that this necessary knowledge has in the past been and must be obtained by experiment. It must be done by experiment, it can only be done by experiment on men or by experiment on animals. I unhesitatingly say that our right to experiment on men is extremely limited. In the first place, we have no right, voluntarily, to allow death to occur, even though the man would be willing to permit it. Nor do I think we have any right, voluntarily, to allow serious damage to occur, unless it perhaps may be where it is directly for the purpose of saving life. So I do not think, in view of mankind's power of anticipation, of memory, and the possibility of future suffering, we ought to take the responsibility of allowing experiments which might produce serious consequences to be made on man. Let me give an example. I do not think that we should have the right for experimental purposes to allow a man to infect himself under special circumstances, even though they would assist science, with a disease like syphilis, in order to add to knowledge. It is too serious a responsibility to take to allow that to be done, considering that that man will live for years afterwards, and cannot divest himself from social relations.

12736. Supposing he was sacrificing himself in that way, and by his sacrifice he could make probable the saving of a great many lives in future, why should you prevent him on the principle that you have to look forward to ultimate results of torture or sacrifice?—Well, it should be the last resource. But I will tell you what, in my opinion, I think constitutes the real difference. I do not think that you ought to lessen in any way the feeling of sanctity of human life, because the consequences are so very wide-spread. To let a man, in a moment of enthusiasm, do a thing which might, or perhaps must, have permanent evil effects throughout all his life, approaches, in my mind, very close to an infringement of the great principles which make us refuse to consider whether it would be a benefit that one man's life should be taken. Everyone of us knows men whom it would be a blessing to mankind to remove, whose lives are a curse to those around them, but, however much we feel that this is the case, we do not allow ourselves to act upon it, because if we were once to commence to act in that way towards men, the consequences that would follow would be so far-reaching that we have found it best to lay it down as a rule that it has been, except in special cases prescribed by law, not permissible under any circumstances for an individual to take human life or to permit a man to die if he can prevent it; and you will find that doctors will not only work as hard for a bad man as they would for a good man, but they will actually bring a man back from the doors of death, although they know that he is going to be hung for what he has done; because we find it is absolutely necessary to take as an unchallengeable principle that human life is sacred. Therefore I think that although in small matters you may use experiment on man, and for the very last stage of experiment you probably must do so, you ought to stop there.

12737. (Sir Mackenzie Chalmers.) You mean with the consent of the man himself?—Yes, even with the consent of the man himself. Where you have established a well-founded scientific probability that the thing is

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safe, then I think you are justified, but before you attempt to experiment on man you must have done everything to prepare for it, and you must know with fair scientific certainty what will happen. And that is the reason why I do not think that experiments on man are to be considered at all when you are thinking of the enormous amount of work that has to be done in scientific research. The necessary work must be done otherwise. Now, I turn to the question of whether we should use animals for the purpose of research. In my opinion, unquestionably, it is not only permitted to us, but it is our duty to try to remove ignorance by, if necessary, experiments on animals; and I want to give the Commission the principal reasons which make me have such a confident opinion on this point. The first reason is the overwhelming preponderance of the prevention of suffering by this method. The great bulk of the necessary experiments, as you know, can be made without any pain to the animals at all, because the animal never need be conscious, and therefore no pain is caused at all. But taking the others, the amount of experiment, if properly done, which causes pain is incredibly small compared with the results of the knowledge obtained, even if they be measured by what has already been achieved. Just look at the complete change that there is on our knowledge of all those great groups of diseases which I have called infectious or communicable. We are no longer fighting them blindly; we are no longer striking in the dark; we know what they are. This very knowledge enables us to avail ourselves of experiments which are not painful for the purpose of assisting us in the task, and every man who feels his responsibility does all he can without occasioning pain. But what is done is done with a knowledge of the enemy we are fighting, so that the experiments grow more telling. The number of painful experiments, as is shown by looking at the actual experiments which have been made, is quite infinitesimal. I read the description of the experiments done under licence in England, where excellent research work is done, and most carefully done too; I read through the list of those which were thus done in a year, and I very much doubt whether the total amount of suffering caused by those experiments would be much greater than would be caused in a single fairly large shooting party, where there were one or two bad shots. If you consider the amount of suffering that is caused in the world, not only thoughtlessly, but even deliberately done by people who are ordinary normal men, and may be able to show a reasonable defence for what they do, it dwarfs so utterly the amount that is requisite to produce this useful knowledge that if the matter was not so serious it would be almost ludicrous to think that there was this organised opposition to the pain caused in scientific research in proper hands. Why, what is attacked here is the only bit of fruitful pain in the world. The greater part of pain had better not be. A man suffers and dies, or suffers and gets well, and all the pain he has suffered has benefited nobody. And in the case of animals there is all this vast mass of pain which is inflicted or permitted, and people tolerate it and say nothing about it, and look upon it as an ordinary thing; but there is one little bit which brings return in lessening the sufferings of the world, and the people are to be found to organise themselves against it, and throw the whole of their strength into denouncing and preventing it. In my opinion, if you look at the medical science of to-day and the medical science of 40 years ago, and realise that the advance has been caused by physiological research, which is largely carried on under the very Acts which restrict, which regulate, but which distinctly permit painful experiments, and then consider on the one hand how much pain that has caused and how completely it has changed our power of dealing with disease and of alleviating suffering, it is really incomprehensible that anybody who sees the totality of misery in the world should think that the small amount of pain that has produced that gigantic result is the first that ought to be stopped. That brings me to the next point, and that is this. Mankind not only claims, but perpetually exercises and properly exercises the right to cause or permit suffering in animals when needful or desirable. It requires considerable thought to realise the extent to which that is admitted by all, and is, indeed, unchallengeable. Take the case which I gave of the rats on board the ship. I pointed out that it was right to kill those rats in order to prevent the possi-

bility of their spreading the plague, but no such serious reason is necessary to justify killing them. I do not suppose there is anyone who would challenge the right of the captain to do it if they would injure the cargo, if they would produce a small pecuniary loss under these circumstances it would be considered that it was perfectly right for him to do it. We unhesitatingly kill vermin—we kill things that do pecuniary harm, and no one dreams that it is a thing which a man of high principle will not do. So that nobody ever dreams of questioning the right of causing pain for the purpose of a useful result, when that useful result is merely pecuniary. But when instead of the result being pecuniary, it means diminishing the suffering of the world for all time and for all the world, forsooth, it is challenged. Take the case, for instance, of a herd of cattle attacked by some painful disease, from which, we will say, 50 per cent. of those attacked survive. A man would be considered perfectly justified in allowing them all to go through the disease. The 50 per cent. that would die would die, the rest would have the pain of the disease and get better; and he would say, "Well, but I could not afford to sacrifice the 50 per cent. that would survive—it would be too big a loss to me." For the £10 apiece that they were worth he would have permitted them to be killed on the spot; but he considers that he has a just and perfect right to allow them to suffer all that pain in order to save himself from the loss of the 50 per cent. that survive. And I do not say he is wrong, because the comfort of his family and other things of importance to him may depend upon the question of how great is the loss inflicted on him pecuniarily by this attack. But if it is allowable to permit the whole of that suffering in the form of in order to avoid a pecuniary loss, the question as to whether it is right to increase by a little the suffering by well-designed scientific experiment to secure a return, not in money, but in the power of stopping suffering, seems to me literally to permit of only one answer. No man could consider the matter without realising that such infliction of pain was justified if he would do what I hold he ought to do: add inflicted and preventible pain together. The moment you take as your guiding principle that you are responsible for pain which you could prevent and that this and the pain which you inflict must be taken together in order to guide your action, it appears to me that there is no painful treatment of animals which is nearly so completely justified as that which leads to knowledge. Nobody, so far as I know, would hesitate to overdrive a horse in order to save a life or to bring assistance to a life that was in danger, and yet that would be suffering inflicted for the purpose of benefit to a single individual. The people that are engaged in scientific research are working for something which will benefit not one single individual, but will benefit the race for all time.

I feel myself that the justification for the necessary use of even painful experiments, where they are required for the purpose of obtaining knowledge, can only be questioned by shutting the eyes to the further consequences of our action—putting a veil between our action and what it ultimately leads to. And I cannot help thinking that the people who lead the movement on the other side must be aware of this because they deal so persistently in misrepresentation. It is very easy to misrepresent the work of those who are a long distance in advance from the ordinary man. I have known this illustrated again and again in manufacture. Processes have been stigmatised as adulteration, as falsification, and everything that was bad, whereas if people had followed out the advance of science they would have known that they were simply better methods for doing the same thing as earlier processes, and doing it with greater certainty and with greater cheapness. But they had not followed it out, and because the new processes were different, a contrast to the old ways, and people thought that therefore they were something to be stigmatised, so it is here. If we could get people to learn what science is and what it has done, they would never attack those who are at work on it. And it is to keep them from learning that there is this frightful misrepresentation. I remember, and I think the Chairman of the Commission probably remembers, how in the seventies the walls of London were placarded with a poster representing a rabbit in the process of being roasted alive.

12738. (Chairman.) I do not remember it, but we have been reminded of it; we have heard about the

roasted rabbit?—I remember it well, because I wrote about it to the late Sir Michael Foster, and I asked him, What is the lethal temperature; because I recognised at once that the experiment which was thus misrepresented must have been one to find out when temperature alone would suffice to occasion death.

12739. You may take it for granted that we know all about it; we have had it explained by Sir Lauder Brunton?—Very well. It turned out that the temperature was something like 109 or 111 degrees.

12740. (*Sir Mackenzie Chalmers.*) 114, to be exact?—It differs very much, of course, in different animals.

12741. (*Chairman.*) That was the outside temperature, not the blood temperature?—I was giving the temperature of the blood. Anyone would realise, of course, that the object was to do it under such circumstances that no reaction should be set up—that there was simply an increase of temperature, and that therefore there must be no disturbance of the organism, and that the poster was absolutely false. Yet that placard was all over London. It is not surprising that a great number of people join this organisation, because they get their ideas from these very serious misrepresentations. They believe themselves to be humane, but when one realises the evil that ignorance does, and that the only way in which ignorance can be removed is by the experimental method, and the enormous advantages in the way of saving pain that these results have produced, one must feel that the truly humane men are the people who are defending scientific research.

Now I should like to turn to the practical conclusions to which I have come with regard to this subject. You see that the good basis of all these movements is the desire of saving pain in animals and men. Now if that is so, the whole motive ceases with the conscious life of the animal. As soon as it has finally passed out of the conscious life the whole *ratio agendi* ceases. It is absurd to object to an experiment on an animal that has ceased to be conscious and will never again be conscious as it would be to object to the dissection of a dead animal. The only motive is the desire to save suffering, and suffering then is as impossible as if death had actually occurred. Now what is the consequence of that? Hence there ought to be no interference with experiments on animals which are in anaesthesia, except that they should be permitted only to persons who can and will take care that the anaesthesia is complete, and that the animal is killed before it comes out. There is no other principle on which any interference could justifiably take place. There must be somebody in charge of or superintending the experiment who, from his character and position, can be trusted with the responsibility of seeing that there is what I may call permanent and effective anaesthesia. Therefore, with regard to such experiments, it is not only not necessary to interfere with their being used for educational purposes, but if the Commission may trust my experience they will indicate that in their opinion it is most important that they should be so used. For there is no comparison between the living effect of an experiment that you have seen and one that you have merely read of. May I give my own experience? I have many and many a time read over carefully-described processes, and have imagined that I understood them; but I knew of old what that meant, and used generally, in cases of any difficulty, to insist on what we call "having a view." I used to go down and insist on being taken, not necessarily by an educated person—I much preferred a workman—right through the whole process, and seeing it done before my eyes. I went back with a knowledge of the case, with a power of handling it, and with a power of drawing conclusions as to the processes which were quite new-born. So it is with the man whom you are educating for this serious duty of fighting disease. If he has actually seen an experiment, it lives in his memory. But that is not the only thing—it remains in his memory as a living thing, and when he acquires new experience or new knowledge connected with it, he will find that it will make the old experiment mean more than it did to him even at the moment when he saw it; because he sees the whole thing before his mind's eye, and not only reads a description which, after all, only states certain results. And, as there is no ground, therefore, from the side of humanity to interfere with these experiments which cause no pain, I think they should be largely used educationally.

Medical education is of prime importance to the community. The greatest difficulty in the way of giving to the world the full benefit of all the accumulated knowledge of this last 40 years is that you have to educate your practical men up to the level of it. Just as in war it is of no use having medical appliances at the base if you cannot get them up to the front of the army, so it is of no use to have this accumulated knowledge unless the power of using it to prevent suffering is in the hands of those whom you train. Therefore the efficacy of our methods of tuition ought not to be looked upon as a matter of little importance—it is a matter of the very gravest importance; and I should put no restrictions upon the use of painless experiments in connection with it, excepting that the experiments must be under the supervision of someone possessing a certificate such as I have indicated, which should be a certificate granted to persons who could be trusted with regard to anaesthesia. And, whether you have reports of those experiments or not I do not think is of the slightest importance, provided it is thoroughly understood that it is the duty of those possessing the certificates to see that the anaesthesia never ceases, and they are held responsible for so doing.

I turn now to the more difficult question, a question which require more careful examination—namely, that of experiments that are in themselves painful. There, I think, you should divide the certificates into two classes. Certain men of acknowledged capacity in research, who have shown that they are not only capable of serious research, but have already done it, should have general certificates entitling them to perform such experiments, subject only to one thing, and that is that they should report fully every experiment. They are immeasurably better able to judge what is worth doing than any body of men that you could put over them. They are the people who are working at the subject itself, and they are, as a rule, men of such high position and high character in England, that there can be no ground for being afraid of trusting them with this power. But I am satisfied that, both for the satisfaction of those who desire to proceed cautiously and for the good of mankind and also to prevent any possible abuse, they ought to report all that they have done. I do not think that a painful experiment should be allowed to pass into oblivion. It is made, in my opinion, for the benefit of the race and for the increase of knowledge; therefore I think such reports should be made, and I do not think that anybody concerned would really object to that being done.

12742. Do you mean that that report should be made by the inspector or by the operator?—By the operator.

12743. Sent into the Home Office?—Sent into the Home Office. I think he ought to do that. I would put him under no restriction as to the character of his experiments; I would not put him under any restriction as to the place in which they are done; but I would have a full report sent into the Home Office. Of course, a certificate of that kind would be given only in recognition of an acquired position and high worth in research. You can only do harm by interfering with men of that class. There is no danger of such men developing recklessness. I have not examined anything excepting the experiments that have been done in England, and I am therefore speaking only with regard to them. I have never, in the years that I have followed the matter, found anything which from this point of view I call recklessness of suffering in any experiment made for research.

12744. Have you found levity? We have heard accusations of levity on the part of those who see experiments?—I feel very strongly that such an accusation is unfair even in the case of students. I do not find that the medical student of to-day is a man of the Bob Sawyer type. I think they work extremely hard. Those who see them in their classes can give you better information on this point, but certainly those I have come across are hard workers and are not guilty of any levity.

12745. (*Mr. Ram.*) Would you have any inspection of these experiments?—I would have no inspection of the first-class certificates.

12746. (*Chairman.*) You do not mean the first-class certificates described in the Act?—No; I mean my first class. Just let me give an example. Supposing it was a question of giving a general certificate to Sir Lauder Brunton. The idea of controlling what he

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should do with it would, in my opinion, be nothing but a blunder. But he would have to report all that he did; and if, by some accident, a mistake was made in any case, if a person developed levity, or even recklessness or cruelty, his reports would show it, and his certificate might be withdrawn by reason of it. He might then be relegated to the second class, which I am going to describe.

12747. (*Sir Mackenzie Chalmers.*) How do you suggest that a man should get into the first class? You are imposing a new duty upon the Home Office?—I know it.

12748. (*Chairman.*) You begin by supposing that he is a first-class man?—Yes. I am quite prepared to say how I think the Home Office ought to decide. I think it ought to decide by the opinion of the leaders of the medical sciences, because I am satisfied that they possess, to an equal degree with any other body of men in the world, the moral qualities necessary to enable them to advise; and beyond question they possess, better than anybody else, the intellectual qualities and the qualities of knowledge which would enable them to do so efficiently; and a certificate of this kind should only be given where there was a consensus among the advisory body that the person held such a position in medical science that he might have this general certificate. I now pass to those who have not attained to that position—which, as I have said, would be an exceptional position, although I am happy to say that there are many men in England who would merit it. I think that in the case of younger men or men who had not yet attained to the commanding position of those to whom I would give this general certificate the lines of the present Act are the right lines. I think it is reasonable to expect them to accept specific limitation of the type of experiment, of the scope of the research, and of the place where the experiment is to be performed, in order that there might be the security of the supervision of those who are over the laboratory, if there be any such persons, and also the security of inspection. Limitation of this kind, which the people would be bound to respect, and I believe would respect, should prevent all danger of abuse, which, though I think it would rarely become a reality, is more possible to occur than it would be in the case of the exceptional men to whom I have before referred. I see no reason why those restrictions should not be put on; and, as before, full and complete reports should always be made, so that a record of all these experiments and their results should be preserved. The line, then, that I should advise, would be as follows:—Certificates with regard to non-painful experiments—that is to say, experiments where the animal was kept under anaesthetics and not allowed to survive—should be quite general, but should only be issued to persons who would perform or supervise those experiments, and could be trusted to see that the anaesthesia was complete and permanent. For painful experiments there should be a small but select class who should have general certificates. The other class should have special certificates, specifying the nature of the research, the nature of the experiment, and the place where it was to be done; and there should be full and adequate inspection of those places while the experiments were going on.

12749. Strangers you would not have present, I understand?—I would not have strangers present, first of all, because, in my opinion, it would not assist the experiment, and, secondly, I do not think it would be a good thing for unqualified persons to see these things. They are the serious work of research, and persons who are not trained by medical or physiological study would gain no good from witnessing them. But I would encourage students being associated with this research. It is the finest training that they can have. So far as it can be done without interfering with research work it is a mistake to keep people out who are capable of learning from it; but that the public should be admitted would, I think, be a still greater mistake. In my opinion, they have no business there, and it is much better to exclude them wholly.

12750. (*Sir Mackenzie Chalmers.*) You mean that the public would go there in the same spirit that the public used to go to an execution?—Exactly. Now I hope that I shall be forgiven if I say one or two words about the two restrictions which have been recently suggested to the Home Office.

12751. (*Chairman.*) Yes, we should like to hear your suggestion about that?—I thoroughly disapprove of them. The first relates to foreign savants who are not naturalised Englishmen. There is no possible reason why they should be put at a disadvantage as compared with British subjects. Of course, for the first class of certificates under the system recommended they must have a recommendation from English people of the proper position to show that they deserved the privilege; but I am not talking about that for the moment. I am talking about the existing system. Foreign universities and foreign men of science have been extremely good to our students. They permit them to take part in their courses; they permit them to use their laboratories, and without good reason I would not show any want of reciprocal kindness towards foreigners. No reason is put forward for thus placing them at a disadvantage, if they possess the qualifications which would justify the certificate being granted to an Englishman, excepting that it is suggested that you cannot enforce the performance of the conditions. That is quite a mistake. The only difficulty in the way of enforcing the performance of the conditions is that they are capable of changing their residence; but an Englishman who wants to break the conditions is equally capable of changing his residence. The true remedy in both those cases is to have a responsible head of the laboratory, who has to see that the conditions are duly performed step by step as the work goes on, and, in my opinion, there would be no greater difficulty in ensuring this being done in the case of a foreigner than there would be in the case of an Englishman.

12752. That would be so in the case of a foreign doctor working in an English laboratory; but, supposing that he wanted a laboratory of his own, and hired a laboratory of his own, and started it for the work for a year in England, would you have a supervisor over him then?—I think you might quite fairly require that there should be some person, resident in England, responsible for the observance of the conditions—that there should be somebody, such as the head of the laboratory, to whom Government can look to see that the conditions are performed, not in a lump, but *pari passu* with the work done. You are justified in recognising a difference in the case of foreigners to that extent, but no further.

12753. Do you not think that if he took a laboratory by the year, we will say, the country has got just as much hold upon him as upon any Englishman who took it?—Yes. There are a great many foreigners in England who should be trusted to the same extent as an English person who is well known in England. But if there is any question about it, you must have somebody in England responsible.

12754. (*Sir Mackenzie Chalmers.*) The difficulty is that they may be well known to scientific men in England, but not known to the public?—Yes; but I think the action of the Home Office should be regulated by the knowledge of scientific men. These public reputations are apt to be misleading in both directions. You will find, if you take the public estimate of men, that the best lecturer is considered to be the greatest man in research. My experience is by no means so. There are men with a magnificent power of exposition, who are thereby so well known to the public that they are supposed to be the leaders of science, whereas in truth they are merely bearers of good news from the front, and are in no sense the leading workers. May I pass to the second restriction recently suggested to the Home Office?

12755. (*Chairman.*) If you please?—The second is that a medical diploma should be required. In my opinion, specialisation has gone so far that the distinction between people who actually work practically at the curative sciences, and those who devote their lives to research in those sciences is becoming marked. A man who is actively employed in practice, as a first-rate man ought to be, has not usually the time for consecutive research; he has to get his time in fragments just when he can. On the other hand, there are many men who are so devoted to research that they give up medical practice as a method of earning their livelihood, and devote themselves entirely to research. The wide diffusion of wealth in this country facilitates this. England has always been noted in science for the numbers of its first-class amateurs. Cavendish was an amateur, and I could mention many other great

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names of men who have had the wherewithal to devote themselves to research, and have made it their life's work without seeking any remuneration for their labour. What should guide the Home Office entirely is, whether or not the applicants for certificates or licenses are suitable for research. Whether or not they have taken out a medical diploma which gives them the nominal right to practice when they have no intention to do so, but mean to devote their whole time to research, is a matter which should be quite immaterial. I am quite satisfied that there are now, and there will be yet more such in the future, men in the very first rank of research work who do nothing in the way of the practice of medicine and who deliberately abstain from it because it would interfere with their scientific research by taking their time and their attention. Those people ought to be encouraged rather than discouraged. Therefore this proposed restriction would, in my opinion, be a retrograde step. So that I should strongly advise that neither of those two things should be adopted as a principle.

12756. Have you studied the Act with a view to suggesting amendments?—You see, I agree with a great deal of it, excepting that I think there are a certain class of men who should have these unrestricted powers of experiment, and that the practice of the Home Office with regard to non-painful experiments should not have regard to the nature of the experiment, but simply to the reliability of the person on whom is placed the responsibility of the anaesthesia. With these modifications I do not quarrel with the Act, because I do not think that the real worker if he is not really hampered in his research, chafes against these restrictions, which are, after all, only a recognition of what I might call the fundamental principle of his action. He is working that pain may be lessened. He is a person whom I have always found extremely sensitive to the cry of suffering, and he has no wish to protest against this Act, which does homage to that principle provided that it does not hamper him. I have not specially considered the provisions of the Act, because I thought it had been dealt with so much by people who knew its working, and are so much more qualified to deal with it than I am; but I doubt whether my conclusions clash very much with the Act as it at present stands.

12757. (Sir William Church.) I should like to ask one question with regard to those to whom you would grant these first-class certificates. Of course, you consider that any person who is placed in the position of head of a laboratory connected with a University or a public institution approaching in importance to a University would necessarily be a person of sufficiently good repute?—May I put it another way? I should hope that a University would never choose a man for the position who did not deserve it.

12758. Then, besides persons in charge of laboratories, you would grant these what you call first-class certificates to such men of repute; and in their case I understood you to say they should not be necessarily restricted to working in licensed laboratories?—No; I should leave them perfectly free, subject to report.

12759. So that they might work either in their own rooms or wherever was most convenient for their work to be done?—Yes, certainly.

12760. Then, with regard to all others who hold licences, I presume that you look forward to their all working in licensed laboratories?—Certainly. Of course, I should trust that the best of those who commenced with a licence would pass in time to the higher degree; but, as long as they only had a licence, they would work in proper laboratories subject to inspection.

12761. So that, even as you propose, it would not be possible for a research student, whether holding a medical diploma or not, for him to perform experiments on living animals even in a licensed laboratory, excepting if he held a licence himself?—No; not painful experiments. He could not perform painful experiments.

12762. And, of course, all those who held what you call first-class licences would be enabled to perform the experiments which are now performed under Certificate B without having to get anything further than their licence?—Yes, certainly.

12763. (Sir Mackenzie Chalmers.) I should like to take first the two points that you have raised about the Home Office practice. I suppose you agree that, apart from the wording of the Act, the Home Secretary is bound to have regard to public opinion to some

extent—that there is a real danger of the powers being lost if suddenly public excitement were got up against them?—He is bound to have regard, I should say, to the Act. I think that he has no need to regard public feeling beyond the extent to which it is expressed in the Act.

12764. You think that his sole duty is to administer the Act, without regard to whether he is imperilling the existence of the Act or not?—I think that the safest way to maintain the Act is to administer it in the way in which it is best for research. The real defence of research will come from the good that it has done; and I think it would be an unwise thing to administer the Act in a niggardly way, in the hope that it would lessen the possibility of a popular storm. I do not myself fear very much the popular storm. I know perfectly well the power of a certain small number of determined people to prejudice the minds of the public, and I appreciate that it is considerable; but the more that the public has experience of the good results of research, the safer it is in the hands of the public.

12765. Of course, as regards the foreigner himself, the man who is temporarily over here, he is to some extent free from the law. He performs certain experiments, and the results of those experiments are not published, probably, until he has returned; and then the Home Secretary may be blamed for allowing the law to be broken by a man over whom he had not effective control. You recognise that difficulty, do you not?—He ought to have effective control, and I think that this would be best obtained by requiring that the reports should be sent in from time to time, and that there should be some responsible person to see this is done.

12766. Would you go so far as this, that supposing a foreigner were authorised to experiment, say, at University College, you would make the head of the laboratory there surety for him?—Yes; I would make him see that the thing is done. And if the man is not performing his duty, it is the duty of the head of the laboratory to report to the Home Office that the experiments are being carried out without due observation of the conditions.

12767. But what practical power of enforcing it have we? Would you withdraw the licence of the head of the laboratory if there were any contravention of the conditions by a foreigner working in his laboratory?—Not unless he connived at it. But, in my opinion, the report of an experiment should be written out as nearly as possible at the time; and if he neglects to see that this is done it appears to me that he is responsible for the breach.

12768. Now, coming to medical diplomas, is there not this difficulty: that when once you get away from the medical diploma, you have no guarantee whatever as to a man. If he belongs to the medical profession, he has been trained up in a profession whose sole object is, as you say, to diminish pain. When once you get outside that, where are you to get your information from as to the qualifications, moral and scientific, of a person who is to have this licence?—Would you say that we had no knowledge of the qualifications of Frank Balfour, before he was killed, so unfortunately, years ago. You will remember that he started a biological school at Cambridge which was full of the greatest promise? Do you think that the addition of a medical diploma would have made Frank Balfour a more reliable man?

12769. Personally I do not. But then, you must take the case of a good many persons who are not known to the public, and you must remember that public opinion is nervous on the question?—I should require the most exceptional testimony in the case of a man who had not done scientific study at a university and under proper training; but that it should have taken the form of medical study seems to me unnecessary. You must understand that the scientific degrees, though they are often taken by medical men, and ought to be taken by them, are in themselves independent. They may not qualify for a diploma, but they connote an amount of knowledge which would quite warrant your recognising the bearers as fit men, if they were duly recommended by those who advise the Home Office. Of course, you will occasionally meet with a man who without any scientific study in a recognised university is a man of pre-eminent value. Michael Faraday had had, I think, no university training at all. But those cases are extraordinarily rare,

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and would only be dealt with when vouched for in a way which was quite unexceptionable. So that, generally, you would have the security of university recognition that the applicant was properly qualified.

12770. Do you think that the existing certifying authorities, which are mainly medical authorities, under the Act, would be able to deal with the case of the people you refer to without medical qualification?—I think so.

12771. Or ought the advising body to be enlarged?—Those who examine up at the Universities usually possess medical degrees, and a great many of them possess also science degrees. I do not know whether Sir Michael Foster had a medical degree.

12772. (Dr. Gaskell.) Yes, he had, and practised?—I did not know that. But you can easily imagine that there may be some who have not.

12773. (Dr. Wilson.) Pasteur had not; he was a chemist?—No, Pasteur had not; that is a good example.

12774. And Metchnikoff was a chemist?—Yes.

12775. (Sir Mackenzie Chalmers.) Then I should like to ask your opinion upon this. It has been suggested to us by Mr. Coleridge, whose evidence, perhaps, you have not seen?—I have not.

12776. It has been suggested that a certificate of humaneness should be required before a licence was given. What is your opinion on that? He suggested, I think, that a certificate should be given by one Justice of the Peace and one minister of religion. Perhaps you would rather not express an opinion?—It is not enough to say that it is unnecessary. It would be an absolute insult to the people whom you would be consulting. The suggestion that the heads of the medical profession are not judges of humanity, of humaneness, I ought to say, is a piece of the most intolerable insolence. It is shocking when you consider the way in which, as a rule, medical men disregard their own comfort, and put themselves to any amount of trouble and discomfort for the purpose of helping people who are sick, very often when it does not bring to them the slightest *kudos* or the slightest pecuniary return. To suggest that such people do not know what humaneness is and are not moved by suffering is intolerable.

12777. I think the suggestion rather was from the evidence that we have had, that the present certifying authorities regard only scientific ends and qualifications, and do not consider as within their province the question of humanity?—So far as Englishmen are concerned (of whom I can alone speak from personal experience) the question as to whether they are likely to use power properly from the point of view of regard for suffering is from my knowledge of the people who are likely to take out licences practically settled before the application is made. But the certifying authorities have to advise that licences should be granted to the applicants, and if they think that *parception* they are people who have not got any care for suffering, and will make a recklessly painful use of their powers, they are bound to say that they are not fit persons to receive licences.

12778. Do you think that ought to be expressed in the Act or the rules in any way?—I should say "fit and proper."

12779. You think that would cover it?—I think that would cover it.

12780. Now, coming to the ethical side of the question; may I rightly sum up your statement in this way—I want to know if I have understood it—that all pain is an evil, perhaps the supreme evil?—Yes.

12781. And that we are justified in the present infliction of a lesser evil when there is a reasonable prospect in the future of avoiding a very much greater evil?—Yes, quite so.

12782. That would be the test and the touchstone which we should apply to all animal experiments?—Yes, that is the touchstone. And in applying that touchstone we must conscientiously use the whole of the teaching that the history of science and the history of medicine have given us. The particulars I have given were in order to show that where there is serious research there is an overwhelming probability that the answer will be in the affirmative.

12783. Of course, as you know, by other ethical witnesses, we have had other tests suggested. For in-

stance, one of the witnesses suggested to us that the test would be the conscience of a progressive people. I must say it did not convey very much information to my mind?—It conveys none to my mind. I feel satisfied that that is an extremely bad definition. "Conscience" is too often used where the true expression should be "emotion." Emotion may be a good motive power, but it is a bad guide, and, in my opinion, there is a very great deal of force in the phrase which somebody has used: "If you want to do good in a particular way, and want to know how you can do it effectively, give your heart a rest and your brains a chance." The only thing that can safely guide you is your reason. Your emotions may give you a motive just as I say that the motive which should dominate the whole of this question is the desire for the suppression of pain. When you have got that motive and desire to obey it, it is your reasoning power which ought to tell what you ought to do.

12784. What is pressed upon us continually is this: Admitted that pain is an evil, you have no right to inflict a definite voluntary amount of pain when the future saving of pain is absolutely problematical, what do you say to that argument?—It is simply because the people have not studied the question that they talk about its being absolutely problematical. What you have to do in life is to act on probabilities. It is quite impossible to prophesy exactly what will happen in the future, but when you see from the teaching which the past has given us that there is an overwhelming probability in favour of a thing happening you are bound to act upon that probability. Would a person refuse to take a medicine because it had not always cured people? It would be a doctor's duty to give it if there was a probability that it would do so.

12785. Then passing from what I call first principles to the secondary principles, if I have understood your evidence, you say that at any rate we have no right to prevent experiment for the purpose of increasing knowledge and controlling pain and disease while we allow the infliction of death and pain in sport for the purpose of providing food for mankind and for a mere commercial purpose, such as spaying sows and castrating horses?—Yes, and in infinitely more cases than those—cases in which, as I have said, you inflict pain for the purpose of preventing pecuniary loss. If you have a valuable dog you see it through the distemper. If it is not valuable you kill it to begin with. Yet you might be perfectly willing to sell that dog in any case. That is simply permitting suffering for the purpose of avoiding pecuniary loss. Otherwise, I really do not understand why people consider it right to kill beetles. Beetles will only destroy a certain amount of their food, and yet they extirpate them, just as they extirpate mice and they extirpate rats, and consider that they are justified in doing so, because they say that such animals do harm. That harm is really pecuniary harm.

12786. Perhaps the fairest way of putting some of these ethical points that have been put before us is to call your attention to a little publication I have just received from Mr. John Page Hopps, who has given evidence here. May I ask you to look at the second paragraph on page 3, beginning with the word "But." May I divide it into paragraphs and ask you whether you have any comments to make upon it. May I read the first words that I want to ask you about: "But when all is said on the score of results, and the artist in vivisection has done his best to convince us that he is the apostle of mercy, many grave considerations 'give us pause.' For instance, what law of God or Nature justifies this treatment of our poor relations? Who gave to this amazing enthusiast the right to say off-hand that he was at liberty to exploit 'the lower animals' for experiments?"—May I point out what it is that gives the whole force of that to the people he is talking to? It is the use of the word "experiments." They do not know what it means; they do not know its importance. Supposing I was to put, instead of "experiments" "saving life or stopping suffering," the whole appeal would fall to the ground. "Experiment" to an uneducated person does not connote what it does to persons who are acquainted with the nature and results of research. If you read it in that way: "Who gave to this amazing enthusiast the right to say off-hand that he was at liberty to exploit 'the lower animals' for saving human life?" the answer would be "Why not?"

12787. As regards that expression of his, "What law of God or Nature justifies this treatment of our poor relations?" what is your comment?—It is all based on this want of appreciation of the meaning and effect of scientific research. Supposing I was to put it, for instance, "What law of God or Nature justifies our killing things, or permitting them to suffer, in order to save us from pecuniary loss?" Yet we do this, and must do it, every day. A beetle is a poor relation, or a mouse is a poor relation, just in the same way as a guinea-pig is; and yet we treat our poor relations in the shape of mice and beetles with phosphorus, and let them die in pain, because we say we must keep them down.

12788. If his argument was right, as a logical conclusion we should have no right to keep a cat who destroys mice by a cruel and lingering death?—Certainly not. We should first kill all the birds of prey, and then kill the birds they prey on because they prey on insects; and then we should kill the greater part of those insects because they live, many of them, on still smaller things. You get back to the state of the Hindoo. Nobody ever dreams of doing that. It is only when it is applied to scientific research, the advantage of which they do not know, that these qualms come in.

12789. They do not believe in scientific research and its results. They honestly do not believe in it?—They do not believe in it; but that is entirely due to their not having studied and mastered the subject, otherwise language such as that would not have any effect.

12790. And Nature herself nearly always provides a cruel and lingering death for every wild animal?—Yes, it is terrible. I think it is Seton Thompson who says: "The end of every wild animal's life is a tragedy." And it is perfectly true.

12791. Now may I take you to the next sentence: "For laboratory purposes, his spectacles would not be safe in the streets; and if he put his proposal into effect, and got his babies on the dissecting table, the very costermongers would raid his laboratory, and even medical students might help them. Why? The cutting-up of one baby might save the lives of thousands of other babies; and the torture of one for an hour might save thousands from suffering for years. Why object?"—I object because you ought to get that knowledge in other ways. Everybody who has studied the subject realises that the knowledge can be obtained in other ways. The analogy between living creatures is so marvellously close that you can, without any experiments on mankind (excepting that you ultimately apply to them the methods which you have worked out scientifically), acquire all the knowledge that is necessary.

12792. It has been suggested to us that if we encourage experiments on living animals, it *per se* will tend to encourage experiments on human beings. What do you say to that argument?—It is the alternative of it. I remember a discussion in society on the subject of these experiments, and a lady at last put this question to the anti-viviselector: "Would you prefer an experiment to be tried on a cat or a baby?" And the person replied, "I would have no experiment tried at all," not seeing that when you have not got knowledge, when you are working blindly, all you do is one long series of experiments on mankind. You are just as really trying experiments; you are not trying experiments under such circumstances that they enlighten you, and the consequence is that the result of what you do is to add very little to our knowledge.

12793. You mean that as long as you are treating disease empirically in mankind, you are experimenting?—Purely experimenting. You are either just sticking to ignorance, or, if you are trying anything new, you are experimenting on mankind.

12794. Pressing Mr. Hopps' argument to its logical conclusion, it would come to this, I suppose: If experiments on animals lead to experiments on babies, the eating of animals for food would lead to eating human beings for food?—You are honouring his argument by attempting to follow it out. That is what it would naturally lead to. But, in the case of experiments, it is because we will not try the experiments on babies that we do them on animals.

12795. Would you kindly look at the last paragraph on page 35, and the first paragraph on page 36. I think you have really dealt with that question. Mr. Page Hopps says, talking of voluntarily incurred experiments: "There is a splendid opportunity here for

the suicide. Instead of a coward in retreat, let him be a hero as an offering. 'The world,' he says, 'is full of misery—sordid, diseased, and despairing. I will get out of it.' He is full of pity for mankind, and is disgusted at its lot; so he decides to give it no help, but to go. What a lame and impotent conclusion! What an opportunity for heroism lost!" Does that strike you as somewhat unpractical?—I have dealt with that. It is not only impossible, but to my mind it is immoral in the highest degree, and it shows an absolutely crass ignorance of the nature of scientific research to think that by a few suicides you could replace the experiments on animals which, in a complicated matter like this, must be numerous, but need rarely be painful, and which, if they are painful, can, in the great majority of cases, be stopped by inflicting death.

12796. I suppose another difficulty would be that you would not get any scientific man to experiment on the would-be suicide?—If he did, he would probably be saved the trouble of suicide himself.

12797. Then Mr. Page Hopps, at page 36, makes another suggestion as regards minor experiments, as he calls them: "Or might it not be possible to connect some minor forms of vivisection with crime? Here is a criminal who has earned his fourteen years of penal servitude. Let him commute it for six months in the hands of a reliable vivisectionist, within limits. And so throughout the whole scale of penalties?"—I think that is loathsome levity, that is all I can say.

12798. Then, leaving that and coming to what you have said yourself, you thought that a man is not justified in experimenting on himself when there is any prospect of real danger?—Yes, I think so, unless he is obliged to do it—and I think he rarely is.

12799. Take a well-known instance. Haffkin invented a prophylactic serum, or injection rather, for plague, as you know?—Yes.

12800. The first person he tried it on was himself. Was he justified or not?—Certainly he was justified if he had scientifically worked it out till he had a well-grounded right to expect that it would not lead to evil consequences. At that stage a man may justifiably, and to the great benefit of the human race, experiment upon himself.

12801. Either on himself or any other person who thoroughly knows what he is doing and consents?—Yes.

12802. (Chairman.) It is very like Columbus taking out his ship to discover America?—Yes, except that Columbus ought to have made a few more observations before he did it.

12803. He had good reason for believing that there was land out there?—No doubt.

12804. (Sir Mackenzie Chalmers.) Would you justify the course of experiments of Dr. Leonard Hill and his assistant experimenting on themselves in the way of testing how many atmospheres they could subject themselves to?—Yes. That is a most interesting instance. He had worked at the subject until he felt clear that he had got, if I might use the phrase, the rule of danger, and having worked it out he had the confidence to submit himself to it, and in that way he enormously assisted the acceptance of his methods. I think he was right. But he would have been perfectly wrong to go trying one method after another upon himself before he had worked his method out to that high degree of scientific probability.

12805. I think the only difficulty that I feel is this, where do you draw the distinction between the man who, for a scientific purpose, submits himself to a dangerous experiment, and a doctor who goes into a hospital reeking with typhus at the imminent risk of his life, to carry on the ordinary work of his profession?—I said that I made an exception when it was necessary to save human life. You see, if a doctor knows that if he does not do that many lives in that hospital will certainly be lost, I think then that he is perfectly right to say, Well, I will risk my life in order to save those lives. But in the other case, I think that he is a bad scientific man if he does not find a way of working it out, without exposing his life, up to a point when he is justified in doing so. That is my feeling. In giving this evidence, I have in my mind all the time what science is. Science is not the bungling, haphazard sort of thing that the world outside believes it to be. It is organised common sense, enlightened by appeals to actual fact, which are framed for the express purpose of giving

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you the information which is necessary for your common sense to act upon. And when you realise this you feel that the man who is content to follow scientific methods is not driven to these heroic steps, he can, with the minimum of suffering to animals and without endangering human life, work out the problems which are before him. If he is impatient he would like, perhaps, to experiment upon himself or on men, but if he is patient he need not do so. And I want the scientific man to be patient in that way.

12806. There is only one other question that I want to ask you. You urge the importance of painless experiments before students?—Yes.

12807. On the old principle *Segnius irritant animos demissa per aures*?—Yes.

12808. But what we have been told is that the performance of these experiments tends to demoralise students, tends to create a morbid curiosity as to pain in their minds. What is your opinion as to that?—My opinion is that that fear is entirely bred in the minds of the people who express it. I do not believe there is the slightest justification for it. We compel students to be present at operations on living men and living women because we feel that we must do so, and no one would listen to the suggestion that they ought not to be present there. How in the world can it have a bad effect when they see operations on animals if it does not when they see them performed on man? I do not believe that there is the slightest justification for the fear, and, as a rule, I do not like or attach any importance to suspicions as to impurity in other people without adequate evidence.

12809. (Mr. Tomkinson.) All your evidence, I think, is pretty well based upon the assumption that practically perfect anaesthesia can be and is maintained in these physiological experiments?—Yes.

12810. And you have no qualms about it as to the possibility of perfect and continued anaesthesia in operations?—I have no doubt whatever about it. Some one expressed it to you in a way which I think exactly represents the truth, viz., that consciousness is the first thing to go and the last thing to come back.

12811. (Dr. Gaskell.) Consciousness of pain?—Yes, consciousness of pain, I mean, is the first thing to go and the last thing to come back. We have the experience of people who have had the most serious and long-continued operations performed upon them, and have come back to consciousness, and their universal testimony is to absolute unconsciousness of pain.

12812. (Mr. Tomkinson.) Then that being the case, and anaesthetics being an established fact, there is no reason for me to ask you how far you would press your theory of the justifiableness of inflicting pain, even intense pain, upon individual creatures in order to prevent a much larger amount of suffering spread over a vast number of individuals; in other words, the infliction of vicarious sacrifices?—You ought only to do that where it is necessary for you to get the knowledge

which will enable you to prevent pain in future, just in the same way as if in my simile of the plague-stricken ship. I think the man ought to take the most painless method that is applicable to kill those rats, but he must kill them even though it should be painful.

12813. But then that is to avoid and prevent a gigantic danger?—Yes.

12814. That would hardly be on all fours with a very painful experiment—an awfully painful experiment, say—upon an animal for a possibility of discovery?—That is just the point. There have been many painful experiments in discovering the true nature of disease which, as I say, makes us think so differently about all these microbic diseases that we cannot put ourselves back into the ignorance of 50 years ago; but they have been for the purpose of ascertaining the nature of these diseases in order that we might fight them. The number of very painful experiments is very few—extremely few. The greater number of the experiments have been of inoculations, and as soon as the disease has developed itself the animal is killed, because what the experimenters have wanted to find out was aye or no, would the disease be taken under such circumstances. The number of cases in which you have to allow really painful experiments is confined to these—that is, the cases where you are examining the distant effects of certain lesions of the nerves of interference with the digestive organs in order that when you see symptoms which are so produced you may know where the evil is. They are never, I think, performed casually; the very painful ones would almost always, I should think, in the history of English research in physiology be performed for some very definite object, in order to give us this very essential knowledge.

12815. And as a matter of fact, with regard to the destruction of dangerous or destructive animals, the common way of trapping them is about as cruel as can be adopted?—Quite so.

12816. And yet it is recognised?—Quite so.

12817. You probably would agree with me that some more stringent law in the direction of prevention of cruelty to animals would be very desirable in those regards?—Yes, I agree. That is just what I feel. I feel that this infliction of pain for the purpose of knowledge is the last thing you ought to touch. I should be very strongly in favour of legislation for lessening pain, if such a thing were possible; but I am happy to say that the need grows less. I think that there is a growing of increased sensitiveness towards suffering which would help legislation of the kind that you speak of, and which is even rendering such legislation unnecessary. I see it everywhere.

12818. Another instance, that entirely misleading statement of baking rabbits prompts the question of the boiling of lobsters?—Yes. There may be much that one might advantageously inquire into there.